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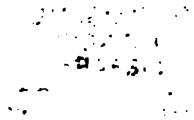
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ERRATA.

P. 101, l. 4, for "ethyl," read "silver."
" 102, l. 13, for "43·5," read "44·8" (as the half sum of equivalents U and Ni.)
" 102, l. 25, for "Mg," read "Mn."
" 107, l. 8, for "C," read "B."
" 110, l. 2 from bottom, for "Co," read "Cr."
" 111, l. 1, for "No," read "Nb."
" 272, l. 8 from bottom, for "HENTZ," read "HEINTZ."

Vol. XXVIII, p. 135, l. 33 from top, for Vol. VI, read Vol. IV.—P. 356, line 20 from bottom, after the word "cultivation," insert a period, putting a capital W for the word "where." Same page, line 19 from bottom, omit "then."—P. 357. line 10 from top, for "deposition of sand," read "deposition of mud."

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THE

AMERICAN

JOURNAL OF SCIENCE AND ARTS.

[SECOND SERIES.]

LAWRENCE SCIENTIFIC SCHOOL CHEMICAL DEPARTMENT

ART. I.—*On the Origination and Distribution of Species:—Introductory Essay to the Flora of Tasmania;* by Dr. JOSEPH D. HOOKER.*

§ 1. *Preliminary Remarks.*

THE Island of Tasmania does not contain a vegetation peculiar to itself, nor constitute an independent botanical region. Its plants are, with comparativelv few exceptions, natives of extra-tropical Australia; and I have consequently found it necessary to study the vegetation of a great part of that vast continent, in order to determine satisfactorily the nature, distribution, and

* *To the Editors of the American Journal of Science, &c.*:—The sheets of this *Introductory Essay*, having been obligingly communicated to me in advance of the publication of the concluding part of the *Flora of Tasmania*, to which it belongs, I asked and have received the distinguished author's permission to reprint them, or a considerable portion of them, in your Journal, and now offer them for that purpose. This is in order that we may have before us, at the earliest date, an essay which cannot fail to attract the immediate and profound attention of scientific men; but which, if confined to the pages of the *Flora of Tasmania*, would be seen by very few American readers. To those who have intelligently observed the course of scientific investigation, and the tendency of speculation, it has for some time been manifest that a re-statement of the Lamarkian hypothesis is at hand. We have this, in an improved and truly scientific form, in the theories which, recently propounded by Mr. Darwin, followed by Mr. Wallace, are here so ably and altogether independently maintained. When these views are fully laid before them, the naturalists of this country will be able to take part in the interesting discussion which they will not fail to call forth.

To save room, a few paragraphs are omitted which do not directly bear upon the subject in hand. A. G.

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affinities of the Tasmanian Flora. From the study of certain extratropical genera and species in their relation to those of Tasmania, I have been led to the far more comprehensive undertaking of arranging and classifying all the Australian plants accessible to me. This I commenced in the hope of being able thereby to extend our knowledge of the affinities of its Flora, and, if possible, to throw light on a very abstruse subject, viz. the origin of its vegetation, and the sources or causes of its peculiarity. This again has induced me to proceed with the inquiry into the oxigin and distribution of existing species; and, as I have already treated of these subjects in the Introduction to the New Zealand Flora, I now embrace the opportunity afforded me by a similar Introduction to the Tasmanian Flora, of revising the opinions I then entertained, and of again investigating the whole subject of the creation of species by variation, with the aid of the experience derived from my subsequent studies of the Floras of India and Australia in relation to one another and to those of neighboring countries, and of the recently published hypotheses of Mr. Darwin and Mr. Wallace. * * *

In the Introductory Essay to the New Zealand Flora, I advanced certain general propositions as to the origin of species, which I refrained from endorsing as articles of my own creed: amongst others was the still prevalent doctrine that these are, in the ordinary acceptation of the term, created as such, and are immutable. In the present Essay I shall advance the opposite hypothesis, that species are derivative and mutable; and this chiefly because, whatever opinions a naturalist may have adopted with regard to the origin and variation of species, every candid mind must admit that the facts and arguments upon which he has grounded his convictions require revision since the recent publication by the Linnean Society of the ingenious and original reasonings and theories of Mr. Darwin and Mr. Wallace.

Further, there must be many who, like myself, having hitherto refrained from expressing any positive opinion, now, after a careful consideration of these naturalists' theories, find the aspect of the question materially changed, and themselves freer to adopt such a theory as may best harmonize with the facts adduced by their own experience.

The Natural History of Australia seemed to me to be especially suited to test such a theory, on account of the comparative uniformity of its physical features being accompanied with a great variety in its Flora; of the differences in the vegetation of its several parts; and of the peculiarity both of its Fauna and Flora, as compared with those of other countries. I accordingly prepared a classified catalogue of all the Australian species in the Herbarium, with their ranges in longitude, latitude, and elevation, as far as I could ascertain them, and added what fur-

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ther information I could obtain from books. At the same time I made a careful study of the affinities and distribution of all the Tasmanian species, and of all those Australian ones which I believed to be found in other countries. I also determined as accurately as I could the genera of the remainder, and especially of those belonging to genera which are found in other countries, and I distinguished the species from one another in those genera which had not been previously arranged. In this manner I have brought together evidence of nearly 8000 flowering plants having been collected or observed in Australia, of which I have seen and catalogued upwards of 7000. About two-thirds of these are ascertained specifically with tolerable accuracy, and the remainder are distinguished from one another, and referred to genera with less certainty, being either undescribed, or described under several names, whilst some are members of such variable groups that I was left in doubt how to dispose of them.

To many who occupy themselves with smaller and better worked botanical districts, such results as may be deduced from the skeleton Flora I have compiled for Australia may seem too crude and imperfect to form data from which to determine its relations. But it is not from a consideration of specific details that such problems as those of the relations of Floras and the origin and distribution of organic forms will ever be solved, though we must eventually look to these details for proofs of the solutions we propose. The limits of the majority of species are so undefinable that few naturalists are agreed upon them;* to a great extent they are matters of opinion, even amongst those persons who believe that species are original and immutable creations; and as our knowledge of the forms and allies of each increases, so do these differences of opinions; the progress of systematic science being, in short, obviously unfavorable to the view that most species are limitable by descriptions or characters, unless large allowances are made for variation. On the other hand, when dealing with genera, or other combinations of species, all that is required is that these be classified in natural groups; and that such groups are true exponents of affinities settled by Nature is abundantly capable of demonstration. It is to an investigation of the extent, relations, and proportions of these natural combinations of species, then, that we must look for the means of obtaining and expressing the features of a Flora; and if in this instance the exotic species are well ascertained, it matters little whether or not the endemic are in all cases accurately distinguished from one another. Further, in a Flora so large as that of Australia, if the species are limited and

* The most conspicuous evidence of this lies in the fact, that the number of known species of flowering plants is by some assumed to be under 80,000, and by others over 150,000.

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estimated by one mind and eye, the errors made under each genus will so far counteract one another, that the mean results for the genera and orders will scarcely be affected. As it is, the method adopted has absorbed many weeks of labor during the last five years, and a much greater degree of accuracy could only have been obtained by a disproportionately greater outlay of time, whilst it would not have materially affected the general results.

With regard to my own views on the subjects of the variability of existing species and the fallacy of supposing we can ascertain anything through these alone of their ancestry or of originally created types, they are, in so far as they are liable to influence my estimate of the value of the facts collected for the analysis of the Australian Flora, unaltered from those which I maintained in the 'Flora of New Zealand.' On such theoretical questions, however, as the origin and ultimate permanence of species, they have been greatly influenced by the views and arguments of Mr. Darwin and Mr. Wallace above alluded to, which incline me to regard more favorably the hypothesis that it is to variation that we must look as the means which Nature has adopted for peopling the globe with those diverse existing forms which, when they tend to transmit their characters unchanged through many generations, are called species. Nevertheless I must repeat, what I have fully stated elsewhere, that these hypotheses should not influence our treatment of species, either as subjects of descriptive science, or as the means of investigating the phenomena of the succession of organic forms in time, or their dispersion and replacement in area, though they should lead us to more philosophical conceptions on these subjects, and stimulate us to seek for such combinations of their characters as may enable us to classify them better, and to trace their origin back to an epoch anterior to that of their present appearance and condition. In doing this, however, the believer in species being lineally related forms must employ the same methods of investigation and follow the same principles that guide the believer in their being actual creations, for the latter assumes that Nature has created species with mutual relations analogous to those which exist between the lineally-descended members of a family, and this is indeed the leading idea in all natural systems. On the other hand, there are so many checks to indiscriminate variation, so many inviolable laws that regulate the production of varieties, the time required to produce wide variations from any given specific type is so great, and the number of species and varieties known to propagate for indefinite periods a succession of absolutely identical members is so large, that all naturalists are agreed that for descriptive purposes species must be treated as if they were at their origin distinct,

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and are destined so to remain. Hence the descriptive naturalist who believes all species to be derivative and mutable, only differs in practice from him who asserts the contrary, in expecting that the posterity of the organism he describes as species may, at some indefinitely distant period of time, require redescription.

I need hardly remark that the classificatory branch of Botany is the only one from which this subject can be approached; for a good system must be founded on a due appreciation of all the attributes of individual plants,—upon a balance of their morphological, physiological, and anatomical relations at all periods of their growth. Species are conventionally assumed to represent, with a great amount of uniformity, the lowest degree of such relationship; and the facts that individuals are more easily grouped into species limited by characters, than into varieties, or than species are into limitable genera or groups of higher value, and that the relationships of species are transmitted hereditarily in a very eminent degree, are the strongest appearances in favor of species being original creations, and genera, etc., arbitrarily limited groups of these.

The difference between varieties and species and genera in respect to definable limitation is however one of degree only, and if increased materials and observation confirm the doctrine which I have for many years labored to establish, that far more species are variable, and far fewer limitable, than has been supposed, that hypothesis will be proportionally strengthened which assumes species to be arbitrarily limited groups of varieties. With the view of ascertaining how far my own experience in classification will bear out such a conclusion, I shall now endeavor to review, without reference to my previous conclusions, the impressions which I have derived from the retrospect of twenty years' study of plants. During that time I have classified many large and small Floras, arctic, temperate and tropical, insular and continental: embracing areas so extensive and varied as to justify, to my apprehension, the assumption that the results derived from these would also be applicable to the whole vegetable kingdom. I shall arrange these results successively under three heads; viz., facts derived from a study of classification; secondly, from distribution; thirdly, from fossils; after which I shall examine the theories with which these facts should harmonize.

§ 2. *On the General Phenomena of Variation in the Vegetable Kingdom.*

1. All vegetable forms are more or less prone to vary as to their sensible properties, or (as it has been happily expressed in regard to all organisms), "they are in a state of unstable equilibrium."* No organ is exactly symmetrical, no two are exact

* Essays: Scientific, Political, and Speculative; by Herbert Spencer: p. 280.

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counterparts, no two individuals are exactly alike, no two parts of the same individual exactly correspond, no two species have equal differences, and no two countries present all the varieties of a species common to both, nor are the species of any two countries alike in number and kind.

2. The rate at which plants vary is always slow, and the extent or degree of variation is graduated. Sports even in color are comparatively rare phenomena, and, as a general rule, the best-marked varieties occur on the confines of the geographical area which a species inhabits. Thus the scarlet Rhododendron (*R. arboreum*) of India inhabits all the Himalaya, the Khasia Mountains, the Peninsular Mountains, and Ceylon; and it is in the centre of its range (Sikkim and the Khasia) that these mean forms occur which by a graduated series unite into one variable species, the rough, rusty-leaved form of Ceylon, and the smooth, silvery-leaved form of the northwestern Himalaya. A white and a rose-colored sport of each variety is found growing with the scarlet in all these localities, but everywhere these sports are few in individuals. Also certain individuals flower earlier than others, and some occasionally twice a year, I believe in all localities.

3. I find that in every Flora all groups of species may be roughly classified into three large divisions: one in which most species are apparently unvarying; another in which most are conspicuously varying; and a third which consists of a mixture of both in more equal proportions. Of these the unvarying species appear so distinct from one another that most botanists agree as to their limits, and their offspring are at once referable by inspection to their parents; each presents several special characters, and it would require many intermediate forms to effect a graduated change from any one to another. The most varying species, on the contrary, so run into one another, that botanists are not agreed as to their limits, and often fail to refer the offspring with certainty to their parents, each being distinguished from one or more others by one or a few such trifling characters, that each group may be regarded as a continuous series of varieties, between the terms of which no hiatus exists suggesting the intercalation of any intermediate variety. The genera *Rubus*, *Rosa*, *Salix*, and *Saxifraga*, afford conspicuous examples of these unstable species; *Veronica*, *Campanula*, and *Lobelia*, of comparatively stable ones.

4. Of these natural groups of varying and unvarying species, some are large and some small; they are also variously distributed through the classes, orders, and genera of the Vegetable Kingdom; but, as a general rule, the varying species are relatively most numerous in those classes, orders, and genera which

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are the simplest in structure.* Complexity of structure is generally accompanied with a greater tendency to permanence in form: thus Acotyledons, Monocotyledons, and Dicotyledons are an ascending series in complexity and in constancy of form. In Dicotyledons, *Salices*, *Urticeæ*, *Chenopodiaceæ*, and other orders with incomplete or absent floral envelops, vary on the whole more than *Leguminosæ*, *Lythraceæ*, *Myrtaceæ*, or *Rosaceæ*, yet members of these present, in all countries, groups of notoriously varying species, as *Eucalyptus* in Australia, *Rosa* in Europe, and *Lotus*, *Epilobium*, and *Rubus* in both Europe and Australia. Again, even genera are divided: of the last named, most or all of the species are variable; of others, as *Epacris*, *Acacia*, and the majority of such as contain upwards of six or eight species, a larger or smaller proportion only are variable. But the prominent fact is, that this element of mutability pervades the whole vegetable kingdom; no class nor order nor genus of more than a few species claims absolute exemption, whilst the grand total of unstable forms generally assumed to be species probably exceeds that of the stable.

5. The above remarks are equally applicable to all the higher divisions of plants. Some genera and orders are as natural, and as limitable by characters, as are some species; others again, though they contain many very well-marked subordinate plans of construction, yet are so connected by intermediate forms with otherwise very different genera or orders, that it is impossible to limit them naturally. And as some of the best marked and limited species consist of a series of badly marked and illimitable varieties, so some of the most natural† and limitable orders

* Mr. Darwin, after a very laborious analysis of many Floras, finds that the species of large genera are relatively more variable than those of small; a result which I was long disposed to doubt, because of the number of variable small genera and the fact that monotypic genera seldom have their variations recorded in systematic works, but an examination of his data and methods compels me to acquiesce in his statement. It has also been remarked (Bory de Saint-Vincent, Voy. aux Quatre Iles de l'Afrique) that the species of islands are more variable than those of continents, an opinion I can scarcely subscribe to, and which is opposed to Mr. Darwin's facts, inasmuch as insular Floras are characterized by peculiar genera, and by having few species in proportion to genera. Bisexual trees and shrubs are generally more variable than unisexual, which however is only a corollary from what is stated above regarding plants of simple structure of flower. On the whole, I think herbs are more variable than shrubby plants, and annuals than perennials. It would be curious to ascertain the relative variableness of social and scattered plants. The individuals of a social plant, in each area it is social upon, are generally very constant, but individuals from different areas often differ much. The *Pinus sylvestris*, *Mughus*, and *uncinata* are cases in point, if considered as varieties of one; as are the Cedars of Atlas, Algeria, and the Himalaya.

† It should be borne in mind that the term *natural*, as applied to orders or other groups, has often a double significance; every natural order is so in the sense of each of its members being more closely related to one or more of its own group than to any of another; but the term is often used to designate an easily limited natural order, that is, one whose members are so very closely related to each other by conspicuous peculiarities that its differential characters can be expressed, and itself

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and genera may respectively consist of only undefinable groups of genera or of species. For instance, both *Gramineæ* and *Compositæ* are, in the present state of our knowledge, absolutely limited orders, and extremely natural ones also; but their genera are to a very eminent degree arbitrarily limited, and their species extremely variable. *Orchideæ* and *Leguminosæ* are also well-limited orders (though not so absolutely as the former), but they, on the contrary, consist of comparatively exceedingly well-marked genera and species. *Melanthaceæ* and *Scrophularineæ*, on the other hand, are not limitable as orders, and contain very many differently constructed groups; but their genera, and to a great extent their species also, are well-marked and limitable. The circumstance of a group being either isolated or having complex relations, is hence no indication of its members having the same characters.

Again, as with species, so with genera and orders, we find that upon the whole those are the best limited which consist of plants of complex floral structure: the orders of Dicotyledons are better limited than those of Monocotyledons, and the genera of Dichlamydeæ than those of Achlamydeæ.*

always recognized; these may be called *objective* orders; *Orchideæ* and *Gramineæ* are examples. Any naturalist, endowed with fair powers of observation and generalization, recognizes the close affinity between a pseudobulbous epiphytical, and a terrestrial tuberous-rooted Orchid, or between the Bamboo and Wheat, though the differences are exceedingly great in habit and in organs of vegetation and reproduction. Other orders are as natural and may be as well limited, but having no conspicuous characters in common, and presenting many subordinate distinct plans of structure, may be regarded as *subjective*. Such are *Ranunculaceæ* and *Leguminosæ*, of which a botanist must have a special and extensive knowledge before he can readily recognize very many of their members. No degree of natural sagacity will enable an uninstructed person to recognize the close affinity of *Clematis* and *Ranunculus*, or of *Acacia* and *Cytisus*, though these are really as closely related as the Orchids and Grasses mentioned above. We do not know why some orders are subjective and some objective; but if the theory of creation by variation is a true one, we ought through it to reach a solution.

* There are too many exceptions to this to admit of our concluding at once that it is attributable to any simple and uniform law of variation; but it may be explained by assuming that the degree or amount of variation is differently manifested at different epochs in the history of the group. Thus, if a genus is numerically increasing, and consequently running into varieties, it will present a group of species with complex relations *inter se;* if, on the contrary, it is numerically decreasing, such decrease must lead to the extinction of some varieties, and hence result in the better limitation of the remainder. The application of this assumption to the fact of the best limited groups being most prevalent among the higher classes (*i. e.*, among those most complicated in their organization), would at first sight appear an argument against progression, were it not for the consideration that the higher tribes of plants have in another respect proved themselves superior, in that they have not only far surpassed the lower in number of genera and species, but in individuals, and also in bulk and stature. And lastly, as all the highest orders of plants contain numerous species and often genera of as simple organization as any of the lower orders are, it follows that that physical superiority which is manifested in greater extent of variation, in better securing a succession of race, in more rapid multiplication of individuals, and even in increase of bulk, is in some senses of a higher order than that represented by mere complexity or specialization of organ.

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Now my object in dwelling on this parallelism between the characteristics of individuals in relation to species, of species in relation to genera, and of genera in relation to orders, is because I consider (Introd. Essay to Fl. N. Z.) that it is to the extinction of species and genera that we are indebted for our means of resolving plants into limitable genera and orders. This view is now, I believe, generally admitted, even by those who still regard species as the immutable units of the vegetable creation; and it therefore now remains to be seen how far we are warranted in extending it to the limitation of species by the elimination of their varieties through natural causes.*

6. The evidence of variability thus deduced from a rapid general survey of the prominent facts elicited from a study of the principles of classification, are to a certain extent tested by the behavior of plants under cultivation, which operates either by hastening the processes of nature (in rapidly inducing variation), or by effecting a prolepsis or anticipation of those processes (in producing sports, *i. e.* better marked varieties, without graduated stages), or by placing the plant in conditions to which it would never have been exposed in the ordinary course of natural events, and which eventually either kill it or give origin to a series of varieties which might otherwise have never existed.†

* It follows as a corollary to the proposition (that species, etc., are naturally rendered limitable by the destruction of varieties), that there must be some intimate relation between the rate of increase and the duration of genera (or other groups of species) on the one hand, and the limitability of their species on the other. Thus, when a genus consists of a multitude of illimitable forms, we may argue with much plausibility that it is on the increase, because no intermediates have as yet been destroyed, and that the birth of individuals and the production of new forms is proceeding at a greater proportional rate than in an equally large genus of which the species are limitable.

† My friend Mr. Wallace treats of animals under domestication, not only as if they were in very different physical conditions from those in a state of nature, inasmuch as every sense and faculty is continually fully exercised and strengthened by wild animals, whilst certain of these lie dormant in the domesticated, but also as if they were subject to the influence of fundamentally different laws. He says, "No inferences as to varieties in a state of nature can be deduced from the observation of those occurring among domestic animals. The two are so much opposed that what applies to the one is almost sure not to apply to the other." But, in the first place, of the same species of wild animals some families must be placed where certain faculties and senses are far more exercised than others, and the difference in this respect between the conditions of many families of wild animals is as great as those between many wild and tame families; and secondly, other senses and faculties, latent and unknown in the wild animal, but which are as proper to the species as any it exercised in its wild state, are manifested or developed by it under domestication. An animal in a state of nature is not then, as Mr. Wallace assumes, "in the full exercise of every part of its organization;" were it so, it could not vary or alter with altered conditions, nor could other faculties remain to be called into play under domestication. The tendency of species when varying cannot be to depart from the original type in a wild condition and to revert to it under domestication, for man cannot invert the order of nature, though he may hasten or retard some of its processes.

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7. Now the prominent phenomena presented by species under cultivation are analogous in kind and extent to those which we have derived from a survey of the affinities of plants in a state of nature: a large number remain apparently permanent and unalterable, and a large number vary indefinitely. Of the permanent there is little to remark, except that they belong to very many orders of plants, nor are they always those which are permanent in a state of nature. Many plants, acknowledged by all to be varieties, may be propagated by seed or otherwise, when their offspring retains for many successive generations the characters of the variety. On the other hand, species which have remained immutable for many generations under cultivation, do at length commence to vary, and having once begun, are thereafter peculiarly prone to vary further.

8. The variable cultivated species present us with the most important phenomena for investigating the laws of mutability and permanence; but these phenomena are so infinitely varied, complex, and apparently contradictory, as to defeat all attempts to elucidate the history of any individual case of variation by a study of its phases alone. It would often appear doubtful whether the natural operations of a plant tend most to induce or to oppose variation; and we hence find the advocates of original permanent creations, and those of mutable variable species, taking exactly opposite views in this respect, the truth, I believe, being that both are right. Nature has provided for the possibility of indefinite variation, but she regulates as to extent and duration; she will neither allow her offspring to be weakened or exhausted by promiscuous hybridization and incessant variation, nor will she suffer a new combination of external conditions to destroy one of these varieties without providing a substitute when necessary; hence some species remain so long hereditarily immutable as to give rise to the doctrine that all are so normally, while others are so mutable as to induce a belief in the very opposite doctrine, which demands incessant lawless change.

9. It would take far too long a time were I to attempt any analysis of the phenomena of cultivation, as illustrative of those of variability in a state of nature. There are however some broad facts which should be borne in mind in treating of variation by cross impregnation and hybridity.

10. Variation is effected by graduated changes; and the tendency of varieties, both in nature and under cultivation, when further varying, is rather to depart more and more widely from the original type, than to revert to it: the best marked varieties of a wild species occurring on the confines of the area the species inhabits, and the best marked varieties of the cultivated species being those last produced by the gardener. I am aware

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that the prevalent opinion is that there is a strong tendency in cultivated, and indeed in all varieties, to revert to the type from which they departed; and I have myself quoted this opinion, without questioning its accuracy,* as tending to support the views of those who regard species as permanent. A further acquaintance with the results of gardening operations leads me now to doubt the existence of this centripetal force in varieties, or at least to believe that in the phrase "reversion to the wild type," many very different phenomena are included. In the first place, the majority of cultivated vegetables and cerealia, such as the Cabbage and its numerous progeny, and the varieties of wall-fruit, show when neglected no disposition to assume the characters of the wild states of these plants;† they certainly degenerate, and even die if Nature does not supply the conditions which man (by anticipation of her operations, or otherwise) has provided; they become stunted, hard, and woody, and resemble their wild progenitors in so far as all stunted plants resemble wild plants of similar habit; but this is not a reversion to the original type, for most of these cultivated races are not *merely* luxuriant forms of the wild parent. In neglected fields and gardens we see plants of Scotch Kale, Brussels Sprouts, or Kohl-rabi, to be all as unlike their common parent, the wild *Brassica oleracea*, as they are unlike one another; so, too, most of our finer kinds of apples, if grown from seed, degenerate and become crabs, but in so doing they become crab states of the varieties to which they belong, and do not revert to the original wild Crab-apple. And the same is true to a great extent of cultivated Roses, of many varieties of trees, of the Raspberry, Strawberry, and indeed of most garden plants. It has also been held, that by imitating the conditions under which the wild state of a cultivated variety grows, we may induce that variety to revert to its original state; but, except in the false sense of reversion above explained, I doubt if this is supported by evidence. Cabbages grown by the seaside are not more like wild Cabbages than those grown elsewhere, and if cultivated states disseminate themselves along the coast, they there retain their cultivated form. This is however a subject which would fill a volume with most instructive matter for reflection, and which receives a hundredfold more illustration from the Animal than from the Vegetable Kingdom. I can here only indicate its bearing on the doctrine of variation, as evidence that Nature operates upon mutable forms by allowing great variation, and displaying

* Fl. N. Zeal., Introd. Essay, p. x, and Flora Indica, Introduction, p. 14.

† Hence the great and acknowledged difficulty of determining the wild parent species of most of our cultivated fruits, cerealia, etc., and in fact of almost every member of our Flora Cibaria. This would not be so were their any disposition in the neglected cultivated races to revert to the wild form.

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little tendency to reversion.* With this law the suggestive observation of M. Vilmorin well accords, that when once the constitution of a plant is so broken that variation is induced, it is easy to multiply the varieties in succeeding generations.

It may be objected to this line of argument that our cultivated plants are, as regards their constitution, in an artificial condition, and are, if unaided, incapable of self-perpetuation; but an artificially induced condition of constitution is not necessarily a diseased or unnatural one, and, so far as our cultivated plants are concerned, all we do is to place them under conditions which Nature does not provide *at the same particular place and time.* That Nature might supply the conditions at other places and times may be inferred from the fact that the plant is found to be provided with the means of availing itself of them when provided, while at the same time it retains all its functions, not only unimpaired, but in many cases in a more highly developed state. We have no reason to suppose that we have violated Nature's laws in producing a new variety of wheat,—we may have only anticipated them; nor is its constitution impaired because it cannot, unaided, perpetuate its race; it is in as sound and unbroken health and vigor during its life as any wild variety is, but its offspring has so many enemies that they do not perpetuate its race. In the case of annual plants, those only can secure the succession of their species which produce more seeds annually than can be eaten by animals or destroyed by the elements. Cultivated wheat will grow and ripen its seed in almost all soils and climates, and as its seeds are produced in great abundance, and can be preserved alive in any quantity, in the same climate, and for many years, it follows that it is not to the artificial or peculiar condition of the plant itself, and still less to any change effected by man upon it, that its annual extinction is due, but to causes that have no effect whatever upon its own constitution, and over which its constitutional peculiarities can exercise no control.

11. Again, the phenomena of cross impregnation amongst individuals of all species appear, according to Mr. Darwin's accurate observations, to have been hitherto much underrated, both as to extent and importance. The prominent fact that the stamens and pistil are so often placed in the same flower, and come to maturity at the same epoch, has led to the doctrine that flowers are usually self-impregnated, and that the effect is a conservative one as regards the permanence of specific forms. The ob-

* It is not meant by this that any character of a species which may be lost in its variety never reappears in the descendants of the latter, for some occasionally do so in great force; what is meant is, that the newly acquired characters of the variety are never so entirely obliterated that it has no longer a claim to be considered a variety.

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servations of Carl Sprengel and others have, however, proved that this is not always the case, and that while Nature has apparently provided for self-fertilization, she has often insiduously counteracted its operation, not only by placing in flowers lures for insects which cross-fertilize them, but often by interposing insuperable obstacles to self-fertilization, in the shape of structural impediments to the access of the pollen to the stigma of its own flower.* In all these instances the double object of Nature may be traced; for self-impregnation (or "breeding in"), while securing identity of form in the offspring, and hence hereditary permanence, at the same time tends to weakness of constitution, and hence to degeneracy and extinction: on the other hand, cross-impregnation, while tending to produce diversity of form in the offspring, and hence variation and apparent mutability, yet by strengthening the offspring favors longevity and apparent permanence of specific type. The ultimate effect of all these operations is of course favorable to the hypothesis that variability is the rule, and permanence the exception, or at any rate only a transitory phenomenon.

12. Hybridization, or cross-impregnation between species or very well marked varieties, again, is a phenomenon of a very different kind, however similar it may appear in operation and analogous in design. Hybridizable genera are rarer that is generally supposed, even in gardens, where they are so often operated upon, under circumstances the most favorable to the production of a hybrid, and unfavorable to self-impregnation. Hybrids are almost invariably barren, and their characters are not those of new varieties. The obvious tendency of hybridization between varieties or other very closely allied forms (in which case the offspring may be fertile) is not to enlarge the bounds of variation, but to contract them; and if between very different forms, it will only tend to confound these. That some supposed species may have their origin in hybridization cannot be denied, but we are now dealing with phenomena on a large scale, and balancing the tendencies of causes uniformly acting, whose effects are unmistakable, and which can be traced throughout the Vegetable Kingdom. In gardening operations the number of hybridized genera is small, their offspring doomed, and since they are more readily impregnated by the pollen of either parent than by their own, or by that of any other plant,† they eventu-

* Thus, in *Lobelia fulgens*, the pollen is entirely prevented by natural causes from reaching the stigma of its own flower. In kidney beans impregnation takes place imperfectly except the carina is worked up and down artificially, which is effected by bees, who may thus either impregnate the flower with its own pollen or with that brought from another plant. I am indebted to Mr. Darwin for both these facts: see 'Gardener's Chronicle,' 1858, p. 828.

† A very able and careful experimenter, M. Naudin, performed a series of experiments at the Jardin des Plantes at Paris, in order to discover the duration of the

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ally revert to one of their parents: on the other hand, the number of varieties is incalculable, the power to vary further is unimpaired in their progeny, and these tend to depart further and further in sensible properties from the original parent.

In conformity with my plan of starting from the variable and not the fixed aspect of Nature, I have now set down the prominent features of the Vegetable Kingdom, as surveyed from this point of view. From the preceding paragraphs the evidence appears to be certainly in favor of proneness to change in individuals, and of the power to change ceasing only with the life of the individual; and we have still to account for the fact that there are limits to these mutations, and laws that control the changes both as to degree and kind; that species are neither visionary nor even arbitrary creations of the naturalist; that they are, in short, realities, whether only temporarily so or not.

13. Granting then that the tendency of nature is first to multiply forms of existing plants by graduated changes, and next by destroying some to isolate the rest in area and in character, we are now in a condition to seek some theory of the *modus operandi* of Nature that will give temporary permanence of character to these changelings. And here we must appeal to theory or speculation; for our knowledge of the history of species in relation to one another, and to the incessant mutations of their environing physical conditions, is far too limited and incomplete to afford data for demonstrating the effects of these in the production of any one species in a native state.

Of these speculations by far the most important and philosophical is that of the delimitation of species by natural selection, for which we are indebted to two wholly independent and original thinkers, Mr. Darwin and Mr. Wallace.* These authors assume that all animal and vegetable forms are variable, that the average amount of space and annual supply of food for each species (or other group of individuals) is limited and constant, but that the increase of all organisms tends to proceed annually in a geometrical ratio; and that, as the sum of organic life on the surface of the globe does not increase, the individuals annually destroyed must be incalculably great; also that each species is ever warring against many enemies, and only holding its own by a slender tenure. In the ordinary course of nature this

progeny of fertile hybrids. He concludes that the fertile posterity of hybrids disappears, to give place to the pure typical form of one or other parent. "Il se peut sans doute qu'il y ait des exceptions à cette loi de retour, et que certains hydrides, à la fois très-fertiles et très-établis, tendent à faire souche d'espèce; mais le fait est loin d'être prouvé. Plus nous observons les phénomènes d'hybridité, plus nous inclinous à croire que les espèces sont indissolublement liées à une fonctions dans l'ensemble des choses, et que c'est le rôle même assigné à chacune d'elles qui en détermine la forme, la dimension et la durée." (Annales des Sc. Nat., sér. 4, v. 9.)

* Journal of the Linnean Society of London, Zoology, vol. iii, p. 45.

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annual destruction falls upon the eggs or seeds and young of the organisms, and as it is effected by a multitude of antagonistic, ever-changing natural causes, each more destructive of one organism than of any other, it operates with different effect on each group of individuals, in every locality, and at every returning season. Here then we have an infinite number of varying conditions, and a superabundant supply of variable organisms, to accommodate themselves to these conditions. Now the organisms can have no power of surviving any change in these conditions, except they are endowed with the means of accommodating themselves to it. The exercise of this power may be accompanied by a visible (morphological) change in the form or structure of the individual, or it may not, in which case there is still a change, but a physiological one, not outwardly manifested; but there is always a morphological change if the change of conditions be sudden, or when, through lapse of time, it becomes extreme. The new form is necessarily that best suited to the changed condition, and as its progeny are henceforth additional enemies to the old, they will eventually tend to replace their parent form in the same locality. Further, a greater proportion of the seeds and young of the old will annually be destroyed than of the new, and the survivors of the old, being less well adapted to the locality, will yield less seed, and hence have fewer descendants.

In the above operations Nature acts slowly on all organisms, but man does so rapidly on the few he cultivates or domesticates; he selects an organism suited to his own locality, and by so modifying its surrounding conditions that the food and space that were the share of others falls to it, he ensures a perpetuation of his variety, and a multiplication of its individuals, by means of the destruction of the previous inhabitants of the same locality; and in every instance, where he has worked long enough, he finds that changes of form have resulted far greater than would suffice to constitute conventional species amongst organisms in a state of nature, and he keeps them distinct by maintaining these conditions.

Mr. Darwin adduces another principle in action amongst living organisms as playing an important part in the origin of species, viz., that the same spot will support most life when peopled with very diverse forms, as is exemplified by the fact that in all isolated areas the number of classes, orders, and genera is very large in proportion to that of species.

§ 3. *On the General Phenomena of Distribution in Area.*

Turning now to another class of facts, those that refer to the distribution of plants on the surface of the globe, the following are the most obvious:—

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14. The most prominent feature in distribution is that circumscription of the area of species, which so forcibly suggests the hypothesis that all the individuals of each species have sprung from a common parent, and have spread in various directions from it. It is true that the area of some (especially cryptogamic and aquatic plants) is so great that we cannot indicate any apparent centre of diffusion, and that others are so sporadic that they appear to have had many such centres; but these species, though more numerous than is usually supposed, are few in comparison with those that have a definite or circumscribed area.

With respect to this limitation in area,* species do not essentially differ from varieties on the one hand, or from genera and higher groups on the other; and indeed, in respect of distribution, they hold an exactly intermediate position between them, varieties being more restricted in locality than species, and these again more than genera.

The universality of this feature (of groups having defined areas) affords to my mind all but conclusive evidence in favor of the hypothesis of similar forms having had but one parent, or pair of parents. And further, this circumscription of species and other groups in area, harmonizes well with that principle of divergence of form, which is opposed to the view that the same variety or species may have originated at different spots. It also follows that, as a general rule, the same species will not give rise to a series of similar varieties (and hence species) at different epochs; whence the geological evidence of contemporaneity derived from identity of fossil forms may be relied upon.

The most obvious cause of this limitation in area no doubt exists in the well-known fact that plants do not necessarily inhabit those areas in which they are constitutionally best fitted to thrive and to propagate; that they do not grow where they would most like to, but where they can find space and fewest enemies. We have seen (13) that most plants are at warfare with one or more competitors for the area they occupy, and that both the number of individuals of any one species and the area it covers are contingent on the conditions which determine these remaining so nicely balanced that each shall be able at least to

* It is a remarkable fact that there are some striking anomalies in the distribution of plants into provinces, as compared with animals. Thus there is no peculiarity in the vegetation of Australia to be compared with the rarity of placental mammals, nor with the fact of so many of the mammals, birds, and fish of Tasmania differing from those of the continent of Australia. Nearer home, we find the basin of the Mediterranean with a tolerably uniform flora on the European and North African sides, but these ranking as different zoological provinces. The much narrower delimitation in area of animals than plants, and greater restriction of faunas than floras, should lead us to anticipate that plant types are, geologically speaking, more ancient and permanent than the higher animal types are, and so I believe them to be, and I would extend the doctrine even to plants of highly complex structure.

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hold its own, and not succumb to the enervating or etiolating or smothering influences of its neighbors. The effects of this warfare are to extinguish some species, to spare only the hardier races of others, and especially to limit the remainder both as to area and characters. Exceptions occur in plants suited to very limited or abnormal conditions, such as desert plants, the chief obstacles to whose multiplication are such inorganic and principally atmospheric causes as other plants cannot overcome at all; such plants have no competitors, are generally widely distributed, and not very variable.*

15. The three great classes of plants, Acotyledons, Monocotyledons, and Dicotyledons (Gymnospermous and Angiospermous), are distributed with tolerable equality over the surface of the globe, inasmuch as we cannot indicate any of the six continents (Europe, Asia, Africa, North and South America, and Australia) as being peculiarly rich in one to the exclusion of another. Further, the distribution of some of the larger orders is remarkably equable, as *Compositæ*, *Leguminosæ*, *Gramineæ*, and others; facts which (supposing existing species to have originated in variation) would seem to indicate that the means of distribution have overcome, or been independent of the existing apparent impediments, and that the power of variation is equally distributed amongst these classes, and continuously exerted under very different conditions. I do not mean that all the classes are equally variable, but that each displays as much variety in one continent as in another.

16. Those classes and orders which are the least complex in organization are the most widely distributed, that is to say, they contain a larger proportion of widely diffused species. Thus the species of Acotyledons are more widely dispersed than those of Monocotyledons, and these again more so than those of Dicotyledons; so also the species of *Thallophytes* are among the most widely dispersed of Acotyledons, the *Gramineæ* of Monocotyledons, and the *Chenopodiaceæ* of Dicotyledons. This tendency of the least complex species to be most widely diffused is most marked in Acotyledons, and least so in Dicotyledons,† a fact which is analogous to that already stated (4), that the least complex are also the most variable.

* Though invariable forms, they may be, and often are, themselves varieties or races of a species that inhabits more fertile spots, as *Poa bulbosa*, which is a very well-marked and constant form of *P. pratensis*, occurring in dry sandy soil, from England to Northwestern India, its "meadow" relative being a very variable species in the same countries, and always struggling for existence amongst other grasses, etc.

† Very much, no doubt, because of the difficulty in classifying Dicotyledons by complexity of organization; in other words, of our inability to estimate in a classificatory point of view the relative value of the presence or absence of organs in plants, where many are present, and where those of low morphological importance may have a comparatively high physiological significance.

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17. Though we rarely find the same species running into the same varieties at widely sundered localities (unless starved or luxuriant forms be called varieties), yet we do often find a group of species represented in many distant places by other groups of allied forms; and if we suppose that individuals of the parent type have found their way to them all, the theory that existing species have originated in variation, and that varieties depart further from the parent form, will account for such groups of allied species being found at distant spots; as also for these groups being composed of representative species and genera.

18. No general relations have yet been established between the physical conditions of a country and the number of species or varieties which it contains, further than that the tropical and temperate regions are more fertile than the polar, and that perennial drought is eminently unfavorable to vegetation. It is not even ascertained whether the tropical climates produce more species than the temperate.

19. Though we cannot explain the general relations between the vegetation and physical condition of any two countries that contrast in these respects, we may conclude as a general rule that those tracts of land present the greatest variety in their vegetation that have the most varied combinations of conditions of heat, light, moisture, and mineral characters. It is, in the present state of our knowledge, impossible to measure the amount of the fluctuations of these conflicting conditions in a given country, nor if we could can we express them symbolically or otherwise so as to make them intelligible exponents of the amount of variety in the vegetation they affect; but the following facts in general distribution appear to me to be favorable to the idea that there is such a connection.

There are certain portions of the surface of the globe characterized by a remarkable uniformity in their phænogamic vegetation. These may be luxuriantly clothed, and abound in individuals, but are always poor in species. Such are the cooler temperate and subarctic lake regions of North America, Fuegia and the Falkland Islands, the Pampas of Buenos Ayres, Siberia and North Russia, Ireland and Western Scotland, the great Gangetic plain, and many other tracts of land. Now all these regions are characterized by a great uniformity in most of their physical characters, and an absence of those varying conditions which we assume to be stimulants to variation in a locality. On the other hand, it is in those tracts that have the most broken surface, varied composition of rocks, excessive climate (within the limits of vegetable endurance), and abundance of light, that the most species are found, as in South Africa, many parts of Brazil and the Andes, Southern France, Asia Minor, Spain, Algeria, Japan, and Australia.

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20. The Polar regions are chiefly peopled from the colder temperate zones, and the species from the latter which have spread into them are very variable, but only within comparatively small limits, particularly in stature, color, and vesture. Many of these polar and colder temperate plants are also found, together with other species closely allied to them, on the mountains of the warm temperate, and even tropical zones; to which it is difficult to conceive that they can have been transported by agencies now in operation.

21. The floras of islands present many points of interest. The total number of species they contain seems to be invariably less than an equal continental area possesses, and the relative numbers of species to genera (or other higher groups) is also much less than in similar continental areas.

The further an island is from a continent, the smaller is its flora numerically, the more peculiar is its vegetation, and the smaller its proportion of species to genera. In the case of very isolated islands, moreover, the generic types are often those of very distant countries, and not of the nearest land. Thus the St. Helena and Ascension forms are not so characteristic of tropical Africa as of the Cape of Good Hope. Those of Kerguelen's Land are Antarctic American, not African nor Indian. The Sandwich Islands contain many Northwest American and some New Zealand forms. Japan presents us with many genera and species unknown except to the *eastward* of the Rocky Mountains, in North America.* So too American, Abyssinian, and even South African genera and species are found in Madeira and the Canary Islands; and Fuegian ones in Tristan d'Acunha.

22. There is a strict analogy in this respect between the floras of islands and those of lofty mountain-ranges, no doubt in both cases owing to the same causes. Thus, as Japan contains various peculiar N. E. American species which are not found in N. W. America nor elsewhere on the globe, and the Canaries and Azores possess American genera not found in Europe nor Africa, so the lofty mountains of Borneo contain Tasmanian and Himalayan representatives; the Himalayas contain Andean, Rocky Mountain, and Japanese genera and species; and the alps of Victoria and Tasmania contain assemblages of New Zealand, Fuegian, Andean, and European genera and species. We cannot account for any of these cases of distribution between islands and mountains except by assuming that the species and genera common to these distant localities have found their way across the intervening spaces under conditions which no longer exist.

* Whilst these sheets are passing through the press, I have been informed by Professor Asa Gray that the flora of Japan and N. E. Asia is much more closely allied to that of the Northern United States than to that of America west of the Rocky Mountains.

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23. There is much to be observed in the condition and distribution of the introduced or naturalized plants of a country, which may be applied to the study of the origin of its indigenous vegetation. The greater proportion of these are the annual and other weeds of cultivated land, and plants which attach themselves to nitrogenous soils; naturalized perennials, shrubs, and trees occur consecutively in rapidly diminishing proportions. I can find no decided relation between complexity of structure and proneness to migrate, nor much between facilities for transport or power of endurance or vitality in the seed, and extent of distribution by artificial means. I shall return to this subject (which I have elsewhere discussed at length with reference to the Galapagos Archipelago*) when treating of the naturalized plants of Australia.

24. I venture to anticipate that a study of the vegetation of islands with reference to the peculiarities of their generic types on the one hand, and of their geological condition (whether as rising or sinking) on the other, may, in the present state of our knowledge, advance the subjects of distribution and variation considerably. The incompleteness of the collections at my command from the Polynesian islands, has frustrated my attempts to illustrate this branch of inquiry by extending my researches from the Australian Flora over that of the Pacific. I may however indicate as a general result, that I find the sinking islands, those (so determined by Darwin's able investigations) characterized as atolls, or as having barrier reefs, to contain comparatively fewer species and fewer peculiar generic types than those which are rising. Thus, commencing from the east coast of Africa, I find in the Indian Ocean the following islands marked in Darwin's chart† as bounded with fringing reefs or active volcanos, and hence rising:—The Seychelles, Madagascar, Mauritius, Bourbon, Ceylon, the Andamans, Nicobar, and Sumatra; the vegetation of all which is characterized by great diversity and much peculiarity of generic type: whereas those marked as atolls or barrier reefs, as the Maldives, Laccadives, and Keeling Island, contain few species, and those the same as grow on the nearest continents. In the Pacific Ocean, again, the groups of islands most remarkable for their ascertained number of very peculiar generic types are the Sandwich group, Galapagos, Juan Fernandez, Loochoo and Bonin, all of which are rising, and most have active volcanos: those with the least amount of peculiarity are the Society group and Fijis, both of which are sinking. In the present state of our knowledge it is not safe to lay much stress on these apparent facts, especially as the New Hebrides and New Caledonia, which lie very close together, and both, I believe,

* Linn. Trans., xx, 235.

† See his works on volcanic islands and on coral reefs.

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contain much peculiarity, are in opposite geological conditions, the Hebrides rising and Caledonia sinking; and the Friendly* and Fiji groups, equally near one another, and with, I suspect, very similar vegetation, are also represented as being in opposite conditions. On the other hand, in the whole of the group including the Low Archipelago and the Society Islands, extending over more than 2000 miles, I observe but one spot,† namely, Elizabeth Island, a mere speck of land, but which is the only known habitat of one of the most remarkable genera of *Compositæ*.‡

25. Many of the above facts in the general distribution of species cannot be wholly accounted for by the supposition that natural causes have dispersed them over such existing obstacles as seas, deserts, and mountain-chains; moreover, some of these facts are opposed to the theory that the creation of existing species has taken place subsequent to the present distribution of climates, and of land and water, and to that of their dispersion having been effected by the now prevailing aquatic, atmospheric, and animal means of transport.

Similar climates and countries, even when altogether favorably placed for receiving colonists from each other, and with conditions suitable to their reciprocal exchange, do not, as a rule, interchange species. Causes now in operation will not account for the fact that only 200 of the New Zealand flowering plants are common to Australia, and still less for the contrasting one that the very commonest, most numerous, and universally distributed Australian genera and species, as *Casuarina*, *Eucalyptus*, *Acacia*, *Boronia*, *Helichrysum*, *Melaleuca*, etc., and all the Australian *Leguminosæ* (including a European genus and species), are absent from New Zealand. Causes now in operation cannot be made to account for a large assemblage of flowering plants characteristic of the Indian peninsula being also inhabitants of tropical Australia, while not one characteristic Australian genus has ever been found in the peninsula of India. Still less will these causes account for the presence of Antarctic and European species in the Alps of Tasmania and Victoria, or for the reappearance of Tasmanian genera on the isolated lofty mountain of Kina-Balou, in Borneo.

* I find that there is a remarkable difference between the floras of the New Hebrides and Caledonia on the one hand, and those of the Fiji islands and those to the east of them on the other. In the former, New Zealand and Australian types abound; in the latter, almost exclusively Indian forms. The differences between the floras of Fiji, Samoa, Tonga, Tahiti, and that of India, are in species and not in genera, and many species are common to all.

† Mr. Darwin has left Aurora Island (another of the group) uncolored, on account of the doubtful evidence regarding it, which however is in favor of its being in the same condition as Elizabeth's Island. From a list of species communicated by Mr. Dana, it appears to contain no peculiar plants.

‡ *Fitchia*. See Lond. Journ. Bot. 1845, iv, p. 640, t. 23, 24. [A specimen of this plant was gathered by Prof. Dana on the mountains of Tahiti.—Eds.]

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These and a multitude of analogous facts have led to the study of two classes of agents, both of which may be reasonably supposed to have had a powerful effect in determining the distribution of plants; these are changes of climates, and changes in the relative positions and elevations of land.

26. Of these, that most easy of direct application is the effect of humidity in extending the range of species into regions characterized by what would otherwise be to them destructive temperatures.

I have, in the 'Antarctic Flora,' shown that the distribution of tropical forms is extended into cold regions that are humid and equable further than into such as are dry and excessive; and, conversely, that temperate forms advance much further into humid and equable tropical regions than into dry and excessive ones; and I have attributed the extension of Tree-ferns, Epiphytal Orchids, Myrtaceæ, etc., into high southern latitudes, to the moist and equable climate of the south temperate zone. I have also shown how conspicuously this kind of climate influences the distribution of mountain plants in India, where tropical forms of Laurel, Fig, Bamboo, and many other genera, ascend the humid extratropical mountains of Eastern Bengal and Sikkim to fully 9000 feet elevation; and temperate genera, and in some cases species, of Quercus, Salix, Rosa, Pinus, Prunus, Camellia, Rubus, Kadsura, Fragaria, Æsculus, etc., descend the mountains even to the level of the sea, in lat. 25°. In a tropical climate the combined effects of an equable climate and humidity in thus extending the distribution of species, often amount to 5000 feet in elevation or depression (equivalent to 15° Fahr. of isothermals in latitude), a most important element in our speculations on the comparative range of species under existing or past conditions; and when to this is added that the average range in altitude of each Himalayan tropical and temperate and alpine species of flowering plant is 4000 feet, which is equivalent to 12° of isothermals of latitude, we can understand how an elevation of a very few thousand feet might, under certain climatic conditions, suffice to extend the range of an otherwise local species over at least 28° parallels of latitude, and how a proportionally small increase of elevation in a meridional chain where it crosses the Equator, may enable temperate plants to effect an easy passage from one temperate zone to the other.

27. To explain more fully the present distribution of species and genera in area, I have recourse to those arguments which are developed in the Introductory Essay to the New Zealand Flora, and which rest on geological evidence, originally established by Sir Charles Lyell, that certain species of animals have survived great relative changes of sea and land. This doctrine, which I in that Essay endeavored to expand by a study of the

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distribution of existing southern species, has, I venture to think, acquired additional weight since then, from the facts I shall bring forward under the next head of Geological Distribution, and which seem to indicate that many existing orders and genera of plants of the highest development may have flourished during the Eocene and Cretaceous periods, and have hence survived complete revolutions in the temperature and geography of the middle and temperate latitudes of the globe.

28. Mr. Darwin has greatly extended in another direction these views of the antiquity of many European species, and their power of retaining their *facies* unchanged during most extensive migrations, by his theory of the simultaneous extension of the glacial temperature in both hemispheres, and its consequent effect in cooling the tropical zone. He argues that, under such a cold condition of the surface of the globe, the temperate plants of both hemispheres may have been almost confined to the tropical zone, whence afterwards, owing to an increment of temperature, they would be driven up to the mountains of the tropics, and back again to those higher temperate latitudes where we now find most of them. I have already (New Zealand Essay) availed myself of the hypothesis of an austral glacial period, to account for Antarctic species being found on the alps of Australia, Tasmania, and New Zealand; and if as complete evidence of such a proportionally cooled state of the intertropical regions were forthcoming as there is of a glacial condition of the temperate zones, it would amply suffice to account for the presence of European and Arctic species in the Antarctic and south temperate regions, and of the temperate species of both hemispheres on the mountains of intermediate tropical latitudes.

On the other hand, we have sufficient evidence of many of what are now the most tropical orders of plants having inhabited the north temperate zone before the glacial epoch; and it is difficult to conceive how these orders could have survived so great a reduction of the temperature of the globe as should have allowed the preglacial temperate flora to cross the Equator in any longitude. It is evident that, under such cold, the most tropical orders must have perished, and their re-creation after the glacial epoch is an inadmissible hypothesis.*

* The question of the state of the mean temperature of the globe during comparatively recent geological periods is yearly deriving greater importance in relation to the problem of distribution. Upon this point geologists are not altogether clear, nor at one with the masters of physical science. Lyell (Principles, ed. ix, chap. vii) attributes the glacial epoch to such a disposition of land and sea as would sufficiently cool the temperate zones; and he implies that this involves or necessitates a lowering of the mean temperature of the whole globe. Another hypothesis is, that there was a lowering of the mean temperature of the globe wholly independent of any material change in the present relations of sea and land, which cold induced the glacial epoch. A third theory is that such a redisposition of land and sea as would induce a glacial epoch in our hemisphere need not be great, nor necessitate a decrement of the mean temperature of the whole earth.

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29. It remains then to examine whether, supposing the glacial epoch of the northern and southern hemispheres to have been contemporaneous, the relations of land and sea may not have been such as that a certain meridian may have retained a tropical temperature near the Equator, and thus have preserved the tropical forms. Such conditions might perhaps be attained by supposing two large masses of land at either pole, which should contract and join towards the Equator, forming one meridional continent, while one equatorial mass of land should be placed at the opposite meridian. If the former continent were traversed by a meridional chain of mountains, and so disposed that the polar oceanic currents should sweep towards the Equator for many degrees along both its shores, its equatorial climate would be throughout far more temperate than that of the opposite equatorial mass of land, whose climate would be tropical, insular, and humid.

30. The hypothesis of former mountain chains having afforded to plants the means of migration, by connecting countries now isolated by seas or desert plains, is derived from the evidence afforded by geology of the extraordinary mutation in elevation that the earth's surface has experienced since the appearance of existing forms of animals and plants. In the Antarctic Flora I suggested as an hypothesis that the presence of so many Arctic-American plants in Antarctic America might be accounted for by supposing that the now depressed portions of the Andean chain had, at a former period, been so elevated that the species in question had passed along it from the north to the south temperate zone;* and there are some facts in the distribution of species common to the mountain floras of the Himalaya and Malay Islands, and of Australia and Japan, that would well accommodate themselves to a similar hypothesis. Of such submerged meridional lands we have some slender evidence in the fact that, in the meridian of Australia and Japan, we have, first, the north-west coast of Australia sinking, together with the Louisiade archipelago to its north; then, approaching the line, the New Ireland group is sinking, as are also the Caroline Islands, in lat 7° N. Beyond this, however, in lat. 15° N., are the Marianne Islands (rising) of whose vegetation nothing is known; in 27° N., the

* The continuous extension of so many species along the Cordillera (of which detailed evidence is given in the Antarctic Flora) from the Rocky Mountains to Fuegia, is a most remarkable fact, considering how great the break is between the Andes of New Granada and those of Mexico, and that the intermediate countries present but few resting-places for alpine plants. That this depression of the chain has had a powerful effect in either limiting the extension of species which have appeared since its occurrence, or in inducing changes of climate which have extinguished species once common to the north and south, is evidenced by the fact that a number of Fuegian and South Chili plants extend northward as alpines to the very shores of the Gulf of Mexico, but do not inhabit the Mexican Andes, whilst as many Arctic species advance south to the Mexican Andes, but do not cross the intermediate depression and reappear in the Bolivian Andes.

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Bonin Islands (also rising); and in 30° is Japan, with which this botanical relationship exists.

It is objected by Mr. Darwin to this line of argument (as to that on p. 15, concerning the Pacific Islands), that all these sinking areas are volcanic islands, having no traces of older rocks on them. But I do not see that this altogether invalidates the hypothesis; for many of the loftiest mountains throughout the Malayan Archipelago, New Zealand, and the Pacific Islands, are volcanic; some are active, and many attain to 14,000 feet in elevation, whilst the lower portions of some of the largest of these islands are formed of rocks of various ages.

(*To be continued*)

ART. II.—*Some General Views on Archæology;* by A. MORLOT.*

A CENTURY scarcely has elapsed since the time when it would have been thought impossible to reconstruct the history of our globe, prior to the appearance of mankind. But, though contemporary historians were wanting during this immense pre-human era, the latter has not failed in leaving us a well-arranged series of most significant vestiges: the animal and vegetable tribes, which have successively appeared and disappeared, have left their fossil remains in the successively deposited strata. Thus has been composed, gradually and slowly, a history of creation, written, as it were, by the Creator himself. It is a great book, the leaves of which are the stratified rocks, following each other in the strictest chronological order, the chapters being the mountain-chains. This great book has long been closed to man. But science, constantly extending its realm and improving its method of induction, has taught the geologist to study those marvellous archives of creation, and we behold him now unfolding the past ages of our world, with a variety of details and a certainty of conclusions well calculated to inspire us with grateful admiration.

The development of archæology has been very similar to that of geology. Not long ago we should have smiled at the idea of reconstructing the by-gone days of our race, previous to the first beginning of history properly so called. The void was filled up, partly by representing that ante-historical antiquity as having been only of short duration, and partly by exaggerating the value and the age of those vague and confused notions which constitute tradition.

* This article is an introduction to a paper entitled, Geologico-Archæological Studies in Denmark and Switzerland, appearing in the *Bulletin de la Société Vaudoise des Sciences Naturelles*, for 1859, and of which a separate edition, comprising the present pages, will be published.

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It seems to be with mankind at large as with single individuals. The recollections of our earliest childhood have entirely faded away, up to some particular event which had struck us more forcibly, and which alone has left a lasting image amidst the surrounding darkness. Thus, excepting the idea of a deluge, which exists among so many nations, and therefore appears to have originated before the migration of those same nations, the infancy of mankind, at least in Europe, has passed without leaving any recollection, and history fails here entirely: for what is history but the memory of mankind?

But, before the beginning of history, there have been life and industry, of which various monuments still exist, while others lie buried in the soil, much as we find the organic remains of former creations entombed in the strata composing the crust of the globe. The memorials of antiquity enact here a part similar to that of the fossils; and if Cuvier calls the geologist an antiquarian of a new order, we can reverse that remarkable saying, and consider the antiquarian as a geologist, applying his method to reconstruct the first ages of mankind, previous to all recollection, and to work out what may be called pre-historical history. This is archæology pure and proper. But archæology cannot be considered as coming to a full stop with the first beginning of history. For the further we recede in our historical researches, the more incomplete they become, leaving gaps which the study of the material remains helps to fill up. Archæology therefore pursues its course in a parallel line with that of history, and henceforth the two sciences mutually enlighten each other. But, with the progress of history, the part taken by archæology goes on decreasing, until the invention of printing almost brings to a close the researches of the antiquarian.

To pursue geological investigations we must first examine the present state of our planet and observe its changes; that is, we must begin by physical geography. This supplies us with a thread of induction, to guide us safely in our rambles through the passed ages of our earth, as Lyell has so admirably set forth. For the laws which govern the organic creation and the inorganic world are as invariable as the results of their combinations and permutations are infinitely varied; science revealing to us every where the perfect stability of the causes with the diversity of the forms.

So, to understand the past ages of our species, we must first begin by examining its present state, following man wherever he has crossed the waters and set his foot upon dry land: the different nations, which inhabit our earth at present, must be studied with respect to their industry, their habits and their general mode of life. We thus make ourselves acquainted with the different degrees of civilization, ranging from the highest summit

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of modern development to the most abject state, hardly surpassing that of the brute. By that means ethnography supplies us with what may be called a contemporaneous scale of development, the stages of which are more or less fixed and invariable, whilst archæology traces a scale of successive development with one moveable stage, passing gradually along the whole line.*

Ethnography is consequently to archæology what physical geography is to geology, namely a thread of induction in the labyrinth of the past, and a starting point in those comparative researches of which the end is the knowledge of mankind and of its development through successive generations.

In following out the principles above laid down, the Scandinavian savans have succeeded in unravelling the leading features in the progress of pre-historical European civilization, and in distinguishing three principal eras, which they have called the *stone-age*, the *bronze-age*, and the *iron-age*.†

This great conquest in the realm of science is due chiefly to the labors of Mr. Thomsen, director of the ethnographical and archæological museums at Copenhagen‡ and to those of Mr. Nilsson, professor at the flourishing University of Lund in Sweden.§

These illustrious veterans among northern antiquaries have ascertained that our Europe, at present so civilized, was at first inhabited by tribes to whom the use of metals was totally unknown, and whose industry and domestic habits must have borne a considerable analogy to what we now see practised among certain savages. Bone, horn and especially flint were then used instead of metal for manufacturing cutting-instruments and arms. This was the *stone-age*, which might also be called the first great phase of civilization.

The earliest settlers in Europe apparently brought with them the art of producing fire. By striking iron-pyrites (sulphuret of iron) against quartz, fire can be easily obtained. But this method can only have been occasionally used, and seems to have been confined to some native tribes in Tierra del Fuego.‖ The usual mode has evidently been that of rubbing two sticks together. But on further reflection it is easy to perceive that this was a

* Some naturalists see a correspondence of the same sort between embryology and comparative anatomy; for they consider the human embryo as passing during its development through the different stages of the scale of animal creation, or, at least, as passing through the different states of the embryos of the different stages of that scale.

† The history of Danish archæology has been sketched by T. Hindenburg. See *Dansk maanedsskrift*, i, 1859.

‡ *Ledetraad til nordisk Oldkyndighed, Kjoebenhavn*, 1836. Published in English by Lord Ellesmere, under the title, "A Guide to Northern Antiquities. London, 1848."

§ Nilsson: *Scandinaviska Nordens Urinvonare.* Lund, 1838—1843.

‖ Weddell: A Voyage towards the South Pole in 1822–1824. London, 1827, p. 167

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most difficult discovery, and must at all events have been preceded by a knowledge of the use of fire, as derived from the effects of lightning or from volcanic action.

The stone-age was therefore probably preceded by a period, perhaps of some length, during which man was unacquainted with the art of producing fire. This, according to Mr. Flourens, indicates that the cradle of mankind was situated in a warm climate.*

The art of producing fire has been perhaps the greatest achievement of human intelligence. The use of fire lies at the root of almost every species of industry. It enables the savage to fell trees, as it allows civilized nations to work metals. Its importance is so great, that deprived of it man would perhaps scarcely have risen above the condition of the brute. Even the ancients were sensible of this, as is witnessed by the fable of Prometheus. As to their sacred perpetual fire, its origin seems to lie in the difficulty of procuring fire, thereby rendering its preservation essential.

In Europe the stone-age came to an end by the introduction of bronze. This metal is an alloy of about nine parts of copper and one part of tin.† It melts and moulds well; the molten mass in cooling slowly acquires a tolerable degree of hardness, inferior to that of steel, it is true, but superior to that of very pure iron. We therefore understand how bronze would long be used for manufacturing cutting-instruments, weapons and numerous personal ornaments. The northern antiquaries have very properly called this second great phase in the development of European civilization the *bronze-age.*

The bronze articles of this period, with a few trifling exceptions, have not been produced by hammering, but have been regularly cast, often with a considerable degree of skill. Even the sword-blades were cast, and the hammer (of stone) was only used to impart a greater degree of hardness to the edge of the weapon.

The bronze-age has therefore witnessed a mining industry, which was completely wanting during the stone-age. Now the art of mining is so essential to civilization, that without it the world would perhaps yet be exclusively inhabited by savages.

* Flourens: *De la longévité humaine.* Paris, 1835, p. 127. "Man, from the construction of his teeth, his stomach, and his intestines, is primitively *frugivorous*, like the monkey. But the frugivorous diet is the most unfavorable, because it constrains its followers perpetually to abide in those countries which produce fruit at all seasons, consequently in warm climates. But, when once the art of cooking was introduced, and applied both to vegetable and animal productions, man could extend and vary the nature of his diet. Man has consequently two diets; the first is primitive, natural and *instinctive*, and by it he is *frugivorous*, the second is artificial, being due entirely to his intelligence, and by it he is *omnivorous*."

† Bronze is still used for casting bells, cannon, and certain portions of machinery. It must not be confounded with common brass, which is a compound of copper and zinc, much less hard, and appearing only in the iron-age.

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It is then worth our while to enquire more closely into the origin of bronze.

Copper was not difficult to obtain. In the first place, virgin copper is not particularly scarce. Then, the different kinds of ore which contain copper combined with other elements, are either highly colored, or present a marked metallic appearance, and are consequently easily known; they are besides not hard to smelt, so as to separate the metal. Finally, copper-ore is not at all scarce; it is met with in the older geological series of most countries.

Virgin tin is unknown, but tin ore is heavy, of dark color and very easy to smelt. However frequent copper may be, tin is of rare occurence. Thus the only mines in Europe which produce tin at the present day are those of Cornwall in England, and of the Erzgebirge and Fichtelgebirge in Germany.

But the question arises, whether, previous to the discovery of bronze, man, owing to the great rarity of tin, may not have begun by using copper in a pure state. If so, there would have been a copper-age between the stone-age and the bronze-age.

In America this has been really the case. When discovered by the Spaniards, both the two centres of civilization, Mexico and Peru, had bronze, composed of copper and tin, and used it for manufacturing arms and cutting-instruments in the absence of iron and steel, which were unknown in the New World. But the admirable researches of Messrs. Squier and Davis in the antiquities of the Mississippi valley* have brought to light an ancient civilization of a remarkable nature, and distinguished by the use of raw virgin copper, worked in a cold state, by hammering, without the aid of fire. The reason of its being so worked lies in the nature of pure copper, which when melted flows sluggishly and is not very fit for casting. A peculiar characteristic of the metal, that of occasionally containing crystals of virgin silver, betrays its origin, and shows that it was brought from the neighborhood of Lake Superior. This region is still rich in metallic copper, of which single blocks, attaining a weight of fifty tons, have lately been discovered. There was even found at the bottom of an old mine a great mass of copper, which the ancients had evidently been unable to raise, and which they had abandoned, after having cut off the projecting parts with stone hatchets.†

The date of that American copper-age is unknown. All we know is, that it must reach at least as far back as ten centuries, that space of time being deemed necessary for the growth of the

* Squier and Davis: Ancient Monuments of the Mississippi Valley. Smithsonian Contributions to Knowledge; Washington, 1848. It is one of the most splendid archæological works ever published.

† Lapham: The Antiquities of Wisconsin. Smithsonian Contributions to Knowledge, 1855, p. 76.

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virgin forests now flourishing upon the remains of that antique civilization, of which the modern Indians have not even retained a tradition.

It is finally worthy of remark, that the *mound-builders*, as the Americans call the race of the copper-age, seem to have immediately preceded and prepared the way for the Mexican civilization, destroyed by the Spaniards; for, in progressing southwards, a gradual transition is noticed from the ancient earthworks of the Mississippi valley to the more modern constructions of Mexico, as found by Cortez.

In Europe the remains of a copper-age are wanting. Here and there a solitary hatchet of pure copper is found. But this can be easily accounted for by the greater frequency of copper, while tin had usually to be brought from a greater distance, so that its supply was more precarious.

As Europe did not witness a regular development of a copper-age, it seems, according to Mr. Troyon's very just remark, that the art of manufacturing bronze was brought from another quarter of the world, where it had been previously invented. It was most probably some region in Asia, producing both copper and tin, where those two metals were first brought into artificial combination, and where also traces of a still earlier copper-age are likely to be found.

An apparently serious objection might be started here by raising the question, how mines could be worked without the aid of steel. This however is sufficiently explained by the fact, that the hardest rocks can be easily managed through the agency of fire. By lighting a large fire against a rock, the latter is rent and fissured, so as considerably to facilitate its quarrying. This method was frequently employed when wood was cheaper, and is even practised at the present day in the mines of the Rammelsberg in Germany, where it facilitates the working of a rock of extreme hardness.

That metal of dingy and sorry appearance, but more truly precious than gold or the diamond—iron—at length appears, giving a wonderful impulse to the progressive march of mankind, and characterising the third great phase in the development of European civilization, very properly called the *iron-age*.

Our planet never produces iron in its metallic or virgin state, for the simple reason, that it is too liable to oxydation. But among the aerolites there are some composed of pure iron with a little nickel, which alters neither the appearance nor sensibly the qualities of the metal. Thus the celebrated meteoric iron discovered by Pallas in Siberia was found by the neighboring blacksmiths to be malleable in a cold state.* Meteoric iron has even been

* Pallas: *Voyages en Russie*, Paris, 1793, iv, 595. There was but one mass of this meteoric iron; it weighed 1600 pounds.

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worked by tribes to whom the use of common iron was unknown. Thus Amerigo Vespucci speaks of savages near the mouth of the La Plata, who had manufactured arrow-heads with iron derived from an aërolite.* Such cases are certainly of rare occurrence, but they are not without their importance, for they explain how man may probably have first become acquainted with iron, and they also account for the occasional traces of iron in tombs of the stone-age, if indeed this fact be well established.

It is notwithstanding evident, that the regular working of terrestrial iron-ore must have been a necessary condition of the commencement and progress of the iron-age.

Now, iron-ore is widely diffused in most countries, but it has usually the look of common stones, being distinguished more by its weight than its color. Moreover its smelting requires a much greater degree of heat than copper or tin, and this renders its production considerably more difficult than that of bronze.

But, even when iron had been obtained, what groping in the dark and how much accumulated experience did it not require, to bring forth at will bar-iron or steel! Chance, if chance there be, may have played a part in it. But as chance only favors those privileged mortals who combine a keen spirit of observation with serious meditation and with practical sense, the discovery was not less difficult or less meritorious. We need not then be surprised, if man arrived but tardily at the manufacture of iron and steel, which is still daily improving.

In Carinthia traces of a most primitive method of producing iron have been noticed. The process seems to have been as follows: on the declivity of a hill was dug an excavation, in which was lighted a large fire; when this began to subside, fragments of very pure ore (hydrous-oxyd) were thrown into it and covered by a new heap of wood. When all the fuel had been consumed, small lumps of iron would then be found among the ashes.† All blowing apparatus was in this manner dispensed with; an important fact when we come to consider how much the use of a blast complicates metallurgical operations, because it implies the application of mechanics. Thus certain tribes in Southern Africa, although manufacturing iron and working it tolerably well, have not achieved the construction of our common kitchen bellows, apparently so simple; they blow laboriously through a tube, or by means of a bladder affixed to it.

The Romans produced iron by the so-called Catalonian process, and the remains of Roman works of that description have been discovered and investigated in Upper Carniola in Austria.‡

* Smithsonian Contributions to Knowledge, vol. ii, art. 8, p. 178.

† Communicated to the author by mining engineers in Carinthia.

‡ *Jahrbuch der k. k. geologischen Reichsanstalt.* Wien, 1850, ii, 199. Carinthia and Upper Carniola formed part of the Roman province *Noricum*, celebrated for its iron.

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The Catalonian forge is still used in the Pyrenees, where it yields tolerable results, but it consumes a large quantity of charcoal, requires much wind, and is only to be applied to pure ore, containing but a very small proportion of earthy matter producing scoriæ; for the process consists in a mere reduction with a soldering and welding together of the reduced particles, without the metal properly melting. According to the manner in which the operation is conducted, bar-iron or steel are obtained at will. This direct method dispenses with the intermediate production of cast iron, which was unknown to the ancients, and which is now the only means of producing iron on a great scale.

Silver accompanies the introduction of iron into Europe, at least in the northern parts, while gold was already known during the bronze-age. This is natural, for gold is generally found as a pure metal, while silver has usually to be extracted from different kinds of ore by more or less complicated metallurgical operations—for example, by cupellation.

With iron appear also for the first time in Europe, glass, coined money, that powerful agent of commerce, and finally the alphabet, which, as the money of intelligence, vastly increases the activity and circulation of thought,* and is sufficient of itself to characterize a new and wonderful era of progress. From thence can we date the dawn of history and of science, in particular of astronomy.

The fine arts also reveal, with the introduction of iron in Europe, a new and important element, indicating a striking advance. Already in the stone-age, but more in the bronze-age, the natural taste for art reveals itself in the ornaments bestowed upon pottery and metallic objects. These ornaments consist in dots, circles, and zigzag, spiral, and S-shaped lines, the style bearing a geometrical character, but showing pure taste and real beauty of its kind, although devoid of all delineations of animated objects, either in the shape of plants or animals. It is only with the iron-age that art, taking a higher range, rose to the representation of plants, animals, and even of the human frame. No wonder, then, if idols of the bronze-age, as well as of the stone-age, are wanting in Europe. It is to be presumed that the worship of fire, of the sun and of the moon, was prevalent in remote antiquity, at least during the bronze-age, perhaps also during the stone-age.

The preceding pages constitute a sketch, certainly very rough and imperfect, of the development of civilization. They establish however in a striking manner the fact of a progress, slow,

* "The circulation of ideas is for the mind what the circulation of specie is for commerce, a true source of wealth." C. V. de Bonstetten: *L'homme du Midi et l'homme du Nord.* Genève, 1826, p. 175.

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but interrupted and immense, when the starting point is considered. The physical constitution of man has naturally benefitted by it. The details contained in the treatise, of which the present paper forms the introduction, prove that the human race has been gradually gaining in vigor and strength since the remotest antiquity.* The domestic races also, the dog first, then the horse, the ox, the sheep, have shared in this physical development. Even the vegetable soil has been gradually improving since the stone-age, at least in Denmark.

And yet there are persons who deny all general progress, seeing everywhere nothing but decay and ruin, like that worthy specimen of a northern pessimist, who exclaimed, "see how man is degenerated, he has even lost his likeness to the monkey!"

ART. III.—*On a new genus of Patelliform shells from the Cretaceous rocks of Nebraska*,† by F. B. MEEK and F. V. HAYDEN. (With a plate.)

Genus ANISOMYON, M. & H.

Etym. ἄνισος, unequal; μῦών, muscle; in allusion to the unsymmetrical muscular scar.

PLATE I.

Generic characters.—Shell very thin, patelliform, or obliquely conical, with an ovate, oval, or circular base; margins entire; surface nearly smooth, or only marked by obscure lines of growth, crossed on some species by fine radiating striæ; summit more or less elevated, located between the middle and the anterior end, sometimes nearly central,—immediate apex very small, and abruptly curved backwards, but not spiral; interior without a projecting lamina or other appendage. Muscular scar irregularly horse-shoe shaped, enlarged at the extremities, with the open part directed towards the shorter end of the shell; becoming abruptly attenuate, or broken into a row of minute oval or circular spots on the right posterior side;—anterior extremities connected by a slender line, which usually passes across just in front of the summit.

* This agrees perfectly with the testimony of statistics. See Quetelet, *Sur l'homme et le developpement de ses facultés*. Paris, 1835, ii, 271. This work of first-rate merit is very near akin to archæology. Mr. Quetelet has just published a new work which will certainly be even more remarkable than the first, and which the author of the present paper regrets not having had within his reach.

† The specimens belong chiefly to the collections brought from Nebraska by Lieut. G. K. Warren, U. S. Top. Eng. Full illustrations and descriptions of the species will appear in his report.

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On the left side of the shell, the anterior extremity of the muscular impression (*a*, fig. 2 and 3, of Plate I.) is generally not so much enlarged as on the right, but sometimes extends slightly farther forward; posteriorly it passes around in the form of a a band to the middle of the slope behind (*b*, fig. 2 and 3), where it is abruptly enlarged and curves upwards. From this point to the larger anterior termination on the right side, there is usually only a slender line (*c*, fig. 2), which is not always quite connected with the enlarged extremity of the band-like part coming around from the left side. Generally this slender line is nearly or quite entire, while in other specimens, even of the same species, it is broken into a series of minute scars as seen at *c*, fig. 3, and in some instances it seems to be entirely obsolete, so as to leave the enlarged anterior extremity on the right, quite isolated.

In most instances, the specimens are found with the small apex (*d*, of fig. 1) broken or worn away, in which condition its former existence would scarcely be suspected. In at least one species (*A. borealis*) this small apex seems to be perforated in the end, the minute aperture being circular, and about large enough to fairly receive the point of a pin. This may be due to accident, but the thickened and smooth margin of this little opening, as seen under a magnifier, has very much the appearance of a natural orifice. We are not sure that this exists in the other species, but suspect it does. In two of the species *A. borealis*, and an undescribed form, there are six equidistant impressed hair lines radiating from the summit down the sides, nearly or quite to the margins, but as there are no traces of such lines on some of the others, presenting the same internal characters, we infer they can scarcely be regarded as a generic character.

From the foregoing description it will be seen that the group of shells we propose to include in this genus, although having the form of *Patella*, present striking differences in the unsymmetrical character of the muscular scar, indicating fundamental peculiarities in the structure of the animal, while they are all much thinner and smoother shells than we usually see in that genus. In some specimens, where there appears to be a complete break in the muscular scar on the right posterior side, there would seem to be some analogy to the genus *Siphonaria*, but as we observe no traces of a siphonal groove passing through this gap, nor any fold in the margin opposite it, and the slender portion of the muscular impression usually passes nearly or quite across, it is scarcely possible any organ such as exists in *Siphonaria*, could have been extruded there.

The more convex species, such as *A. borealis*, are somewhat similar externally to some species of *Hipponyx*, but to say noth-

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ing of other differences, the fact that the open extremity of the horse-shoe shaped muscular scar in our shells is always turned towards the shorter end, or in other words, that the apex is placed in front of the middle instead of behind it, shows they have no affinities to that or any allied genus.

They would then seem to be perhaps more nearly related to *Acmæa* and *Gadinia* than to any other of our existing mollusca, since in both these genera the animal is more or less unsymmetrical, the former having the branchial plume exserted from the right side of the neck, and the latter a siphon occupying a groove on the right just in front of the anterior extremity of the muscular scar, which is shorter on that side than on the other. Our shells, however, differ from these genera in the peculiar attenuate or interrupted character of the muscular impression on the right posterior side, and the folding back of the apex.* In the thinness of the shell and the nature of the surface, they are most like *Acmæa*, with which we at first thought them probably identical, but adopting the opinion of M. d'Orbigny that this genus is synonymous with *Helcium* of Montfort, we referred them provisionally to the latter as the older name. Not long afterwards we observed the peculiar character of the muscular impression on an internal cast of one of the species, but at first supposed it merely due to some accident; subsequently however, we ascertained that it exists in five clearly distinct species, and cannot be regarded as an accidental or specific character.

It is probable many of the Cretaceous and Jurassic species that have been referred by different authors to the genera *Patella*, *Acmæa*, *Helcium*, &c., will be found to possess the internal characters of this genus. Judging from the figures of the Cretaceous and Jurassic species of patelliform shells we have seen in published works, specimens showing the muscular scar, have rarely been found. We have observed the characters of this genus in the following Nebraska species:—

ANISOMYON BOREALIS, (=*Hipponyx borealis*, Morton, 1842=*Helcium carinatum*, Meek & Hayden, 1856).
A. SEXSULCATUS, (=*Helcium sexsulcatum*, M. & H.).
A. ALVEOLUS, (=*Helcium alveolum*, M. & H.).
A. PATELLIFORMIS, (=*Helcium patelliforme*, M. & H.).
A. SUBOVATUS, (=*Helcium subovatum*, M. & H.).

Washington, D. C., Nov. 20, 1859.

* Dr. A. A. Gould, the well known conchologist of Boston, to whom we sent sketches of these shells, writes that he concurs with us in regarding them as being clearly distinct from all the recent genera to which such fossil forms are usually referred.

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ART. IV.—*General account of the results of the discussion of the Declinometer observations made at Girard College, Philadelphia, between the years* 1840 *to* 1845, *with special reference to the eleven year period;* by A. D. BACHE, Superintendent of the U. S. Coast Survey.

[Communicated to the American Association for the Advancement of Science, by authority from the Treasury Department.]

IT is proposed to give here in outline the results of an investigation of the magnetic observations made with the declinometer, between the years 1840 to 1845, at the Girard College observatory, with special reference to the eleven year period in the amplitude of the solar-diurnal variation and the disturbances of the magnetic declination. Prof. Henry, Secretary of the Smithsonian Institution, has kindly offered to publish the memoir in full in the Smithsonian Contributions to Knowledge. It is my intention to pursue the discussion by taking up the investigation of the lunar influence on the same magnetic element.

In coöperation with the scheme adopted at the British Colonial Observatories, a series of magnetic and meteorological observations were made at the Girard College observatory with instruments purchased under the direction of the trustees of the College, the observations being made under the patronage of the American Philosophical Society, and finally completed for the use of the Topographical Bureau of the War Department. These observations were made under my direction and superintendence. The series commenced in May, 1840, and with short interruptions terminated in June, 1845, thus furnishing a five years series of magnetic observations taken bi-hourly up to Oct. 1843, and after that date hourly. The readings of each magnetic element were united into means, and were also presented graphically (in the fourth volume of the record). This was done under my direction by J. Ruth, Esq., but owing to other laborious duties the record could not be submitted to a more complete reduction. I have now resumed the subject by the assistance of Charles A. Schott, Esq., Assistant in the Coast Survey, by whom, under my immediate direction, and as my assistant in this special matter, the present paper has been prepared.*

Although other magnetic observatories furnish by their judicious geographical location, a basis for the generalization of their results, it is nevertheless desirable to obtain results from intermediate observatories as confirmations or as corrections. In the investigation of the disturbance-law at Point Barrow, when compared with the same at Toronto, a very remarkable mutual

* It may be proper to state that this work has been performed out of Office hours, and at my own expense.

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relation of the law at these stations resulted from such a comparison, and farther examination may bring to light other dependencies of a mutual character.

According to the latest determination the position of the Girard College observatory is in latitude 39° 58′ 23″ (north), and in longitude 75° 10′ 05″=5^h 00^m 40^s·3 west of Greenwich. From it Toronto bears 38° 45′ west of north (true) and is distant about 334 statute miles.

It is proposed specially to investigate the law of the eleven year period, or, as it is more frequently called, the decennial period, there being yet an uncertainty as to its precise length. It is supposed to have some direct or indirect connection with the solar spot period, which correspondence, according to late investigations by Prof. R. Wolf, is so close as even to exhibit analogous disturbances. The following discussion will afford a contribution towards the determination of the epoch of the occurrence of a minimum in certain phases of the magnetic variation and disturbances, corresponding to a minimum of the solar spots. The method of reduction is substantially the same as that adopted by General Sabine. Earlier investigations of Dr. Lamont and those by Mr. Kreil differ from his in not including the discussion of the disturbances in connection with the period in question.

As long as the magnitude of the deflection remains the only criterion by which a disturbance may be recognized as such, the adoption of any limit of deviation from the normal value of the same hour, month and year, must necessarily remain in some measure arbitrary, or, in other words, there must always remain after the separation of the disturbances a certain small amount of their effect in the remaining regular diurnal progression. To effect the separation, Peirce's criterion has been used with entire success. After a preliminary investigation as to the number of disturbances separated, the limit, as pointed out by the criterion, or a deviation of 8 scale divisions (or 3′·6 of arc) has been adopted in the present discussion, as constituting a disturbed observation. Accordingly all observations differing by that amount or more from the mean monthly value of their respective hour were marked by a pencil line. Next a new hourly mean was taken, omitting values so marked, and each observation was again examined in reference to deviation from this new mean. This process was repeated when necessary, so that in all cases, values differing eight scale divisions or more from the mean were excluded. The last mean thus obtained for each observing hour and each month has been called "the normal." These values have been tabulated and are given for each month and year separately, together with such corrections as the omissions or interruptions demanded. The bi-hourly, and afterwards the hourly readings (and their means) were made 19½^m after the hour so as to correspond to an even Göttingen hour (diagram A).

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For the purpose of comparing the annual means of the normals, or the mean march of the regular solar diurnal variation for each year, the results have been expressed analytically by means of Bessel's formula, and by the application of the method of least squares.

Probably owing to the several accidental changes in the suspension of the bar, and consequent uncertainty in the precise amount of scale correction, the mean readings of each year, when compared with one another, exhibit differences not actually due to irregularities occasioned by declination changes. Though this question does not directly bear upon the present investigation, which mainly depends on differences of readings, it will be proper to remark that the observed increase, giving the weight $\frac{1}{2}$ to the mean of 1840 and 1845 (on account of incomplete record) is under the supposition of a uniform annual change between these years, equal to 4′·50. According to Mr. Schott's latest investigation of the secular change of the declination in Philadelphia supported by observations between the years 1701 and 1855·7, the annual increase between the years 1840 to 1845 is 4′·98, a result which accords tolerably well with actual observations. According to this formula the declination on the 1st of January, 1843, the mean epoch of the present series, is 3° 32′ West with a probable error of ±10′. This declination corresponds to the scale reading 560·31, which has been deduced by taking into account the weights of the annual means.

The expressions have been thrown into curves (diagram B), and the agreement between computed and observed values is shown by the introduction of dots giving the observed reading. The probable error of any single representation is ±0′·1. By means of the formulæ the following values were computed.

For	Epoch of maximum eastern deflection.	Correspond. scale reading	Epoch of maximum western deflection.	Correspond. scale reading.	Amplitude (in arc.)
	h m	*d*	*h m*	*d*	′
1840	7 26 A.M.	595·67	1 34 P.M.	575·71	9·08
1841	7 49	577·96	1 49	560·21	8·06
1842	7 36	571·24	1 37	553·96	7·83
1843	7 40	569·54	1 24	553·06	7·46m
1844	7 32	556·50	1 18	539·99	7·51
1845	7 34	536·65	1 16	517·81	8·53
Mean	7 36 A.M. ± 3m		1 30 P.M. ± 4m		

The inequality constituting the ten or eleven year period is plainly exhibited in the last column, the progression of the numbers being quite regular; the year 1843 is directly indicated as the year of the minimum range of the diurnal fluctuation. By means of a special formula, deduced by least squares, and

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representing a single value within $\pm 0'.11$, the month of May, 1843, is indicated as the epoch of minimum amplitude.

The discussion of the disturbances, as far as they bear on the decennial inequality, next follows, taking in also some collateral results.

The total number of observations for changes of declination recorded and discussed amount to 24,566; of these, according to the preceding investigation, 2,357 were separated as disturbances. There is one disturbed observation in every 10·4 observations. The discussion of the disturbances is divided into two parts, that of the number, and that of the amount of the deflections. Omissions in the record have been supplied by the use of proper ratios showing the law as given by the full periods, and interpolated values are enclosed within brackets. The number of disturbances in each month of the year or the annual inequality in the distribution of the disturbances has been made out for each year, and the means and ratios are also given. The principal maximum occurs in October (at Toronto in September), the secondary in April; the two minima, nearly equal in amount, occur in February and June (the first one in January at Toronto). The ratios of the number of monthly disturbances to the average number are given in the following table, showing the same also divided into westerly and easterly values.

	RATIOS.				RATIOS.		
	W.	E.	Total.		W.	E.	Total.
January,	1·27	0·42	0·77	July,	0·58	1·18	0·86
February,	0·70	0·46	0·52m	August,	2·00M	1·14	1·59M
March,	0·83	0·57	0·68	September,	0·93	1·86	1·36
April,	0·95	0·80	$0·91M_2$	October,	1·58	2·50M	2·12M
May,	0·55	0·60	0·58	November,	0·96	1·12	1·08
June,	0·44	0·61	0·53m	December,	1·21	0·74	1·00
				Mean,	1·00	1·00	1·00

The ratios show a general correspondence in the numbers of westerly and easterly deflections; the westerly seem to occur most frequently in August, while the easterly predominate in October; the secondary maximum of both series is in April. For the total number the minima occur in February and June. The following table contains the number of disturbances in each year.

1840	483	weight ½
1841	539	
1842	446	
1843	275	weight ½
1844	308	
1845	264	weight ½

Proportional number of western disturbance 987, of eastern 912; at Toronto the eastern predominate over the western in the proportion of 1·17 to 1.

These numbers do not indicate the law of the eleven year period as plainly and systematically as found by the investigation

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of the diurnal amplitude, yet giving proper weight, (on account of deficiencies,) the minimum number of disturbances falls in the year 1843.

If we distribute the disturbances, 1,942 in number for the even hours, according to their respective hours of occurrence, we find the following ratios:

Philadelphia mean time.	RATIOS. W.	RATIOS. E.	Total number.
h *m*			
0 (19½)	0·82	1·20	162
2 "	1·18	1·16	189M
4 "	1·08	0 96	168
6 "	1·35	0 80	173
8 "	1 29	0·70	161
10 "	1·32	0·88	178
Noon "	1·13	0·71	150
14 "	0 95	0·67	133m
16 "	1·07	0·78	148
18 "	0 87	0 90	143
20 "	0·40m	1·66M	167
22 (19½)	0·54	1·58	170

The numbers in each column show a regular progression; the disturbances, irrespective of their direction, have a minimum at 2 P. M., and a maximum at 2 A. M., (at Toronto the respective hours are 2 P. M. and 22 P. M.). The principal contrast is between the hours of the day and the hours of the night. In the table given above the most striking result is that the westerly disturbances have their minimum precisely at the hour (8 P. M.) when the easterly have their maximum, and the exact coincidence of this result with that deduced by General Sabine for Toronto is not less remarkable. In connection with this subject, it may be remarked that the same distinguished magnetist found a singular mutual relation to subsist between the phenomena at Toronto and Point Barrow, on the shores of the Arctic sea,—the laws of the easterly deflection at one station being found to correspond at the same local hours with those of the westerly deflections at the other station, and vice versa. This contrast therefore holds good for Philadelphia as well as Toronto.

If we classify the disturbances according to their amount, we obtain the total aggregate and mean values of a single disturbance in the different years as follows:

	Aggregate amount in scale divisions.	Mean value.	Mean value. W.	Mean value. E.
	d	′	′	′
1840	7155·5	6·70	6·52	7·20
1841	7844·4	6·61	5 93	7·07
1842	6019·1	6·11	5·53	6·70
1843	2932·2	4·85	4·85	4·85
1844	4227·3	6·21	6·21	6·21
1845	3521·4	6·02	5·25	6·84

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The eleven year period is well marked in the aggregate as well as in the mean values, and the precise epoch of the minimum was found by a special formula. It took place in August, 1843, and as a resulting epoch from this and the previous determination, June, 1843 may be adopted. This is graphically represented on diagram C.

The following table gives the ratios of the aggregate amount of disturbances in each month of the year:

	Total.	W.	E.		Total.	W.	E.
January,	0·72	1·21	0·33	July,	0·87	0·46	1·29
February,	$0·54m_2$	0·59	0·46	August,	1·61	1·95	1·28
March,	0·66	0·73	0·56	September,	1·56	0·99	2·12
April,	$0·94M_2$	1·04	0·84	October,	$2·06M_1$	1·67	2·46
May,	0·56	0·52	0·56	November,	1·06	1·09	1·02
June,	$0·42m_1$	0·37	0·47	December,	1·00	1·38	0·61

In reference to the first column, the maximum amount of disturbances occurs in October (at Toronto in September); the minimum in June (as at Toronto); the secondary maximum occurs in April (the same at Toronto); the secondary minimum occurs in February (and at Toronto in January). If separated into east and west deflections, maxima occur in September (mean of August and October) and April; and minima in June and January (same as at Toronto).

The arrangement according to the hours of the day gives the following ratios, to which is added, in the last column, the diurnal disturbance variation obtained by dividing the excess of aggregate westerly over easterly values by the total number of days of observation.

Philadelphia mean time.	RATIOS.			Disturbance, variation in arc.
	W.	E.	Both combined.	
h m				′
0 (19½)	0·83	1·24	1·04	−0·16
2 "	1·16	1·10	1·13	−0 01
4 "	1·16	0·92	1·04	+0·05
6 "	1·46	0·67	1·06	+0·24
8 "	1·39	0·67	1·03	+0·22
10 "	1·22	0·77	0 99	+0·13
Noon "	1·03	0·63	0·83	+0·11
14 "	0·98	0 63	0·80m	+0·10
16 "	0·99	0·72	0·85	+0 07
18 "	0 84	0·89	0 84	−0·02
20 "	0·88m	1·88M	1·15	−0·53
22 (19½)	0·56	1·88	1·25M	−0·47

If we compare these ratios with the corresponding numbers in a preceding table showing the bi-hourly distribution in regard to number, we find, irrespective of the direction of the deflections, the 2 P. M. minimum preserved. The maximum is earlier and occurs at 10 P. M. (At Toronto these hours are respectively 1 P. M. and 9 P. M.) At Philadelphia as well as at Toronto the

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ratios are nearly invariable from 10 A. M. to 6 P. M., and again from 8 P. M. to 8 A. M. The easterly maximum and westerly minimum at 8 P. M. form again a marked feature.

The law governing the disturbances during a solar day is clearly shown, and is of a systematic character. The diurnal variation caused by the disturbances, if superposed on the regular diurnal variation, would produce what is known by the name "mean diurnal variation." If we plot the disturbance curve on the same scale, or actually superpose it on the curves of the regular diurnal variation, it would hardly show to the eye, and the compound curve of the mean variation would keep within the maximum distance of a dot from the regular curve in the diagram (D). The disturbance variation has but one maximum and one minimum. Its most prominent feature is the easterly deflection at 8 o'clock ($+19\frac{1}{2}^m$) P. M. (at Toronto it is at 9 P. M.); the maximum deflection amounts at that hour to 32″ of arc, and to 45″ at Toronto. The greatest westerly deflection occurs at 6^h ($19\frac{1}{2}^m$) A. M. and amounts to but 14″ (at Toronto the hour is 8 A. M. with 6″, and from a five years series of observation, with 31″ of deflection). The range of the disturbance variation equals 46″. From 3 in the morning till 5 in the afternoon, the mean effect of the disturbances is to deflect the north end of the magnet to the west, and during the remaining hours (principally night hours) to the east. The westerly and easterly disturbance deflections, during a day, balance within 0′·02.

The annual inequality in the amplitude of the diurnal disturbance variation cannot satisfactorily be shown on account of the short and partly interrupted series of observations.

It is my intention to continue the discussion of the observations made at the Girard College Observatory.

After the above was written No. 1185 of the Astronomische Nachrichten came to hand, containing Prof. R. Wolf's interesting results on the close connection of the variation in the frequency of the solar spots, and the corresponding inequality in the amplitude of the diurnal variation of the declination. He deduces for Munich the simple formula, $\beta = 6'{\cdot}273 + 0'{\cdot}051\,\alpha$, where α represents a relative number expressive of the frequency of the solar spots, directly derived from observation, and β the amplitude of the diurnal variation. He found a close correspondence between these phenomena, showing the observed and computed amplitude for the Munich observations between 1835 and 1850. The average length of the solar spot period is reaffirmed to be 11·11 years $\pm$ 0·04 years. For Philadelphia we obtain $\beta = 7'{\cdot}080 + 0'{\cdot}039\,\alpha$, which formula represents the observations as follows:

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Year.	a From solar spot observations.	Amplitude derived from a.	Observed amplitude.	Difference.
		′	′	
1840	51·8	9·10	9·08	−0·02
1841	29·5	8·23	8·06	−0·17
1842	19·2	7·83	7·83	0·00
1843	8·4	7·41	7·46	+0·05
1844	12·2	7·55	7·51	−0·04
1845	32·4	8·24	8·58	+0·19

The correspondence between the observed diurnal amplitude and the same computed from observations of the solar spots is further exhibited in the annexed diagram (E). The dotted curve is the approximate Toronto curve from observation specially introduced to show the agreement at the epoch of the maximum in 1848. By computation from the solar spot observations, the amplitude at that time would amount to 11′·00, the whole range of the inequality of the diurnal variation would therefore be 11′·00−7′·46=3′·54.

It is much to be desired that this interesting branch of physical enquiry be further studied, as it forms one of the links connecting terrestrial with cosmical phenomena.

ART. V.—*A Visual Method of effecting a Precise Automatic Comparison of Time between distant stations;* by JONATHAN HOMER LANE. (With a plate.)

THE visual apparatus of which I here give a general description was invented several years ago, and is intended to supply the place, under certain circumstances, of the electric telegraph, in the determination of differences of longitude. Although the wires of the electric telegraph, when suspended in the air, appear to leave nothing to be desired, at least for distances of a few hundred miles, in situations where they are available, yet it has appeared to me that the visual method I propose may prove useful in many cases where the stations to be compared, particularly the astronomical stations in a trigonometrical survey, are removed to considerable distances from lines of electric telegraph. In those situations, also, where submarine or subterranean lines take the place of air lines, the visual method, on account of the comparatively slow velocity of the electric signal along the wires of such lines, and the open and irresolvable question whether the signal time might not be greater in one direction than the other, would be capable of furnishing a useful check upon the indications of the electric telegraph.

The general features of the method are the following:

First, an intense light shown at one station, A, and viewed at the other station, B, as a star.

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Secondly, a uniform automatic movement at A, made to interrupt the light at regular minute periods of say one sixteenth of a second from the middle of one interruption to the middle of the next.

Thirdly, a uniform automatic movement at B, by which the star of light is made optically to travel around a circle at the rate of one revolution in the same period of one sixteenth of a second, the consequence of which is that the periodical interruption becomes visible to the eye as a break in the luminous circle produced by the motion of the star, and according as such break is seen upon one part or another of the circumference of the luminous circle, the relation of the movement at A to that at B becomes, inside of the recurring period of one sixteenth of a second, known in like manner as if they had been connected by a coupling shaft extended from one to the other.

Fourthly, a supplementary flash of the light at A, occurring at each whole second and during the interval of one of the interruptions before mentioned, which supplementary flash finds itself subject, in the movement at B, to an action by which it would be carried optically around a circle once only in one whole second, and by the position at which it occurs on the circumference of that circle indicates the sixteenths of the whole second. Also, a still further signal, by which account may be taken of the whole seconds, in ways too obvious to require special notice, and which will complete the knowledge of the relation of the movements at A and B to each other, or of the quantity by which one may be in advance of the other in its motion.

Fifthly, any of the known methods of effecting automatic comparison of the movement at either station with the clock at that station, by which means the comparison of the clocks at the two stations will be made to the minutest fraction of a second. Or, a single clock may be used, say at A, and any observation at B can by known methods be automatically referred to the movement at that station, and thus compared at once with the clock at A.

Further, if the automatic movement at B, besides giving optical motion to the star of light shown from A, is simultaneously made to produce periodical interruption of another intense light shown at B and seen at a third station, C, provided with a movement like that at B, the comparison from A to B may be extended directly onward from B to C, and from C onward to a fourth station, and so on, and such is the degree of precision which seems, so far as we can judge without direct experiment, attainable in the comparison between contiguous stations, that the probable error of a single comparison between the extremes of a line of twenty stations may, I believe, be made smaller than the hundredth part of a second.

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An arrangement suitable for carrying the above plan into effect is illustrated by a sketch in the accompanying plate. At the station A, the rays of the signal light, diverging from their source L (Plate II, fig. 1, *a*), are converged by a lens C so as to meet and cross each other in the focus F of a telescope. Diverging from this point they traverse the object glass O, and issue from it in a nearly parallel beam directed to the station B. The light thus transmitted is periodically intercepted at the focus F by the projecting teeth of a rotating disc, or signal wheel, S, which is made to rotate uniformly at the rate of one revolution in about one second.

The telescope used at the station B for viewing the light shown at A, is furnished with a terrestrial eye-piece, and at that point where occurs the first image of the object glass, or crossing of all pencils of light that can pass through the telescope, is introduced a refracting glass prism P, shown in section in Fig. 1, *b*. The pencil formed by the light from A, on traversing the refracting prism, is turned aside so that the star image, which otherwise would be formed at *s*, is formed at *s'*, and the displacement is observed by the eye at E. Since this displacement is always in a plane at right angles to the edge of the refracting angle of the prism, if the latter be made to rotate on an axis *zz*, parallel to the axis of the telescope, the displaced star image *s'* will travel in a circle around *s* as a center. If the period of revolution be shorter than the duration of the luminous impression on the eye, and the light be unintermitting, the circle described by *s'* will appear to the eye as a continuous circle of light. The period of one sixteenth of a second may perhaps be taken as sufficiently small for continuity of luminous impression. Accordingly, if the prism be made to revolve about sixteen times per second, and precisely sixteen times during each revolution of the signal wheel S, and if the primary division of the latter be made by sixteen equidistant slotted openings in its border, then the luminous circle, which but for the interposition of the signal wheel would appear continuous and entire, will be seen in part obliterated, as shown in Fig. 3, the luminous part S' D S' having the same ratio to the obliterated part S' G S', that the width of one of the slotted openings of the signal wheel has to the width of a tooth. And as the luminous arc S' D S' appears on one part or another of the circumference, its angle of position, which may be observed by bringing the wire of a position micrometer into coincidence with the extremities of the arc, will determine the angle of position at which the prism arrives simultaneously with the arrival of the signal wheel at a given point of reference.

But for counting the whole sixteenths of a second it will be required to know also the simultaneous angle of position of a

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wheel geared with the prism P so as to revolve only once in the time of one revolution of the signal wheel. The circular prismatic piece is of a diameter many times greater than that of the pencil of light, and the latter traverses the former at its border. Around the prismatic piece is fastened a toothed ring, into which gears the driving wheel H from which the prism takes its motion. Between this wheel H and the regulator from which it derives its uniform motion, is interposed a satellite wheel arrangement, by means of which the observer, without disturbing the invariable velocity of the regulator, can set the wheel H, and the parts to which it gives motion, forward or back in their course, and then allow them to proceed *at once* with the same correct velocity as before. In this way the observer will have absolute control of the angle of position of the luminous arc S′ D S′, and it may be agreed that as this angle of position slowly changes in consequence of the want of *perfect* unison between the movements at the two stations, he shall, from time to time, bring the luminous arc back to near coincidence with a standard position, that for instance which is shown in Fig. 3, the exact angle of position to be measured, however, in the manner above mentioned. Provided, then, the arc be not allowed to stray far from its standard position, it will be obvious that one part of the border of the prismatic piece P will never be traversed by the light which passes the sixteen primary openings of the signal wheel and forms that arc. The part thus unused is made with parallel faces, as shown in the figure, and then any supplementary flash of light occurring midway between the primary ones, will pass through the parallel part of P unrefracted, and may be refracted by a second prism P′, that moment interposed. This second prism is made to revolve once in the period of the sixteen revolutions of P, and in the best mode of construction that occurs to me is one of sixteen prismatic pieces P′, P′, P′, &c., so cut out, and attached to the border of a wheel or disc K, made to revolve in that period, that the edges of the refracting angles of all of them shall be parallel to each other, the whole forming the equivalent of a single prism cut into a large toothed wheel. This wheel K, like P, takes its motion from H, and is so geared that during all the intervals of time in which a passing pencil would encounter the refracting part of P, it will have free passage through one of the spaces between the pieces P′, P′, P′, &c., which, during the alternate intervals of time, will in their turn be interposed in the path of the pencil. Any flash of light, therefore, that escapes through any supplementary opening, as *t*, in the middle of one of the sixteen primary teeth of the signal wheel, will, in traversing the telescope at B, be refracted by one of the prisms P′ alone, and not by P. And if it be recollected that the several prisms P′ are in effect parts of one prism, as dis-

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tinctly indicated in fig. 1, *b*, and that the action of this differs in no respect from that of P except in its longer period of revolution, and that this period of revolution is the same with that of the signal wheel, it will be obvious that if several supplementary openings be made in the signal wheel, as for instance two, *t* and *u*, in two teeth diametrically opposite to each other, and two others, *v* and *w*, in teeth adjacent to one of these on each side of it, the flashes of light through these openings will be seen by the eye at E to occur at points t', u', v', w', fig. 3, distributed around the circumference of a circle concentric with S′ D S′ G, and at angular intervals from each other identical with those between the corresponding openings in the signal wheel. It will be further obvious that the observed angle of position of the diameter $u'\,t'$ will depend on the angle of position of the wheel K relatively to that of the signal wheel. The diameter $u'\,t'$ becomes, then, by aid of the more exact indication of S′ D S′, an index by which we know the required angle of position at which the wheel K arrives simultaneously with the arrival of the signal wheel S at a given point of reference. As the most convenient mode of procedure in practice, the observer at B may operate the satellite wheel until the index diameter $u'\,t'$ is brought to a position, for instance the vertical one in the figure, made to denote zero, and the position micrometer for taking the angle of position of S′ D S′ may be graduated to thousandths and ten thousandths of a second.

Instead of the above described arrangement there is a modification of it which I am disposed to prefer, the type of which is a pair of telescopes at station B, placed side by side so that one may contain the rapidly revolving prism and the other the more slowly revolving one, each prism being in this case unintermitting in its action, and the supplementary openings of the signal wheel being replaced by the filling up of a single one of the primary openings. The omission of the flash of light from this one would be observable through the slow prism and give the required indication, while it would not probably injure in any material degree the distinctness of the arc of light seen through the fast prism. Instead of a pair of complete telescopes, the equivalent of a pair of eye-pieces with a sliding object-glass to alternate between them at pleasure, would answer the same purpose. In this arrangement no rectification of the prisms by the observer would be necessary, it being always possible to observe the total deviation. This would be a great advantage on a line of very numerous stations, in which case it would, on the first described plan, be a somewhat critical matter to bring all the instruments on the line into the required correspondence for simultaneous observation.

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As before intimated, it would be possible to employ but a single clock on the whole line of stations, but as this would require signal observations for every time observation at any other than the clock station, it would be more convenient to employ a clock at every astronomical station.

The question of the feasibility of the process described in this paper will depend primarily on the practicability of securing, with telescopes of moderate aperture, a sufficiency of light for such distances as from fifty to eighty miles, and next on the attainment of sufficient precision of rate in the uniform motion employed. I do not anticipate serious difficulty in either of these things. For the uniform motion, considering especially the light work it will have to do, the Fraunhofer regulator would I presume be everything that is required, or an electromagnetic regulator, similar to that described by me in a paper presented to the American Association at their meeting at Montreal, may be used if found reliable. From what a scientific friend has told me of his experience with distant lights, I think we are justified in anticipating the easy attainment of sufficiency of light.

A similar optical means can also be used for comparing a mean time clock at one station with a sidereal clock at another, by the method of coincidences, without other mechanism than the clocks themselves, though with diminished power of precision on a long line of stations. The pendulum of the one clock is made to carry in the focus of the telescope at its station, an opaque disc with a narrow slit, through which, at each oscillation, a flash of light is allowed to escape to the other station, and through the focus of the telescope at that station oscillates a wire carried by the pendulum of the other clock, which eclipses the flash of light at each coincidence of the two pendulums. Or the pendulum at the observing station may carry a mirror, in which either a flash or an interruption of light from the other station may be observed by reflection, and the coincidence noted when the flash or the break is seen at the same point of the field of view where it is observed with the pendulum at rest.

I have already observed that the visual method proposed in this paper might prove useful as a check, at least, upon the indications of submarine or subterranean lines of electric telegraph. But it seems less liable to uncertainty in its indications than even the air lines, the signals of which occupy a very appreciable and more or less ambiguous time in passing, and therefore on very extensive surveys it would be very instructive at least, and might be found to give increased accuracy, to add to the comparisons made by the telegraph wires, further comparisons by means of a sufficient number of the visual instruments to reach across the whole extent of the survey. In case it should

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ever be undertaken, as has been proposed, to measure an extensive arc of the equator, the idea of such a visual method for the accurate determination of the differences of longitude, would be well worth considering.

I will close by suggesting one more obvious application of the method, and that is, the determination of the velocity of light, which, with a sufficiently high velocity of revolution of the prism and signal wheel, might be done with considerable accuracy by transmitting, in the same manner as before described from a second station to a third, a return signal from the second station to the first.

ART. VI.—*On Osmious Acid, and the position of Osmium in the list of Elements;* by J. W. MALLET, Prof. of Chemistry, &c., Univ. of Alabama.

IN most chemical text-books it is stated, on the authority of Berzelius, that there are five oxyds of osmium—OsO, Os_2O_3, OsO_2, OsO_3, and OsO_5—of which however the second and fourth have not been isolated, although compounds containing them are known. To these may be added a blue substance, first obtained by Vauquelin and supposed by Berzelius to consist of OsO united to either Os_2O_3 or OsO_2, and the highest oxyd, probably OsO_5, the existence of which was announced by Frémy in 1854.

While preparing osmium from some black platinum residues I have accidentally obtained, a substance which there is some reason to believe may be osmious acid—the hitherto unisolated teroxyd—mixed indeed with osmic acid, but still permitting certain of its properties to be observed.

Three or four ounces of the platinum residue were treated by a modification of the original process of Wollaston, now seldom adopted. The powder was mixed with three times its weight of nitre, the mixture was fused for some time in an iron crucible, and then poured out upon an iron plate. While still warm the fused cake was broken into fragments and put into a flask fitted with a cork, through which passed a tube two feet long, bent at right angles, and a funnel-tube, the latter drawn out to a very small bore at the lower end, and reaching to the bottom of the flask. The bent tube was well cooled, and *undiluted* oil of vitriol was very cautiously poured, by a few drops at a time, into the funnel.

The acid produced intense heat on coming in contact with the cake of potash salt, and oily drops of a bright *yellow* color began to make their appearance in the cooled tube. These drops very slowly congealed to a solid resembling unbleached bees-wax.

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By the time the sulphuric acid had been added in slight excess a considerable quantity of this yellow substance had collected in the tube and in a receiver attached. By gentle heating the whole was obtained in the receiver, and united under a little water to a single mass. Towards the end of the distillation *colorless* needles and fused drops of the well known osmic acid came over, and doubtless a considerable portion of the yellow mass in the receiver consisted of the same.

At first it seemed probable that the yellow color of the latter was due merely to some impurity, and it was therefore cautiously resublimed, but it again collected of the same tint as before. It appeared to be even more fusible and volatile than osmic acid; it took a long time to congeal under a stream of cold water flowing over the outside of a tube in which it had been melted.

The water in which it was fused acquired a bright yellow color, and gave off fumes, the odor of which seemed to me somewhat different from that of osmic acid, and which irritated the eyes so insufferably that it was scarcely possible to finish work with the acid and put it up for preservation. It was removed as a single cake from the water, and sealed up hermetically in a glass tube which had been previously cleansed with care from all traces of dust or other organic matter. The water in which it had been fused was mixed with caustic potash, and gave a solution of very dark brown-red color, such a tint as would probably result from a mixture of the red* *osmite* of potash discovered by Frémy with the orange-brown *osmiate* of potash.

The sealed tube containing the fused cake or stick of yellow acid was allowed to remain upon a table exposed to the direct rays of the sun. The acid immediately began to sublime upon the sides of the tube, not in long needles and prismatic crystals like osmic acid (which seems to be monoclinic), but in feathery crusts like sal-ammoniac, which under a lens had somewhat the appearance of minute octahedrons grouped together. The color was still bright yellow, but in a short time the sublimed acid began to turn black, and in twenty-four hours the whole inner surface of the tube was perfectly black and opaque. A tube containing pure colorless osmic acid has been exposed in a similar way to the sun for three weeks without any such blackening taking place. A tube closed by a cork, or one from which dust has not been carefully removed will often cause osmic acid to turn dark, but never exhibits anything like the absolute blackness and opacity of the whole tube noticed in the present instance.

* A rose-red color is also characteristic of the salt supposed by Berzelius to be the ammonio-*terchlorid* of osmium, corresponding in the chlorine series to *osmite* of ammonia.

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It is easy however to imagine the cause of this change undergone by the yellow acid if it be in fact the teroxyd of osmium (mixed with osmic acid). The teroxyd probably broke up into osmic acid and one of the lower oxyds of osmium or perhaps the metal itself. We might have

$$2OsO_3 = OsO_4 + OsO_2$$
$$5OsO_3 = 3OsO_4 + Os_2O_3$$
$$3OsO_3 = 2OsO_4 + OsO,$$

or,
$$4OsO_3 = 3OsO_4 + Os.$$

In order to ascertain, if possible, which of the above changes had taken place, the tube was opened two or three months after it had been sealed, and the contents were examined. The fused stick of acid was found to be black and partially friable; on heating in another glass vessel most of it sublimed, leaving a little black powder behind, and condensed in needles, still slightly yellowish, but differing little in appearance from common osmic acid. The inner surface of the original tube was found coated with a thin filmy, adherent crust, of a black color and considerable lustre. This was scraped off, and a portion of it gently heated in a stream of dry carbonic acid gas until all traces of adherent osmic acid were driven off. After cooling, the carbonic acid was replaced by dry hydrogen, and heat was again applied. Water condensed on the tube beyond the heated part, thus proving that an oxyd of osmium, not the metal, was under examination. Replacing again the hydrogen by oxygen, osmic acid was produced and carried off with the stream of gas. The black powder scraped off from the original tube was heated with hydrochloric acid, and seemed to be but slowly acted on; the acid however assumed a green color, and hence it is probable that the osmium existed as protoxyd.

It is not easy to see, without further investigation, how osmious acid could have replaced in part osmic acid in the attempt to prepare the latter as above described. Is there a particular stage of the decomposition of nitre by heat at which osmium may replace nitrogen in nitrite of potash (KO, NO_3)? From the relations of the two elements, to be noticed presently, this would seem probable, and in fact Frémy has noticed the crystallization of osmite of potash from a solution in hot water of the fused cake of nitre and iridosmium. A reason for osmic acid (OsO_4) being usually obtained from the latter, instead of osmious (OsO_2), might perhaps be found in the fact that the chemists who of late years have worked upon osmium recommend the use of nitric or nitro-muriatic acid to neutralize the potash—sulphuric acid, to which Wollaston had recourse in his early experiments, is now seldom employed. Thomson, in his "Chemistry of Inorganic Bodies," published many years ago, observes that osmic acid has sometimes a tint of yellow.

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It does not seem likely that the cork closing the neck of the flask used for distillation had anything to do with the production of osmious acid, if such took place; the cork itself did not show any appearance of being acted on, and there was no blackening of its surface until some time after the experiment was ended.

The reduction of osmic acid generally results in the formation of the basic oxyds; Berzelius, however, observed that on adding sulphurous acid to a solution of osmic acid the latter passed through various shades of color—*yellow*, *orange-yellow*, brown, green, and at last blue; he attributed these tints to the successive formation of sulphates of the bin-oxyd, sesqui-oxyd, and blue oxyd; but may not the first step in the reduction have been osmious acid, giving the yellow color?

Another and altogether different view of the nature of the volatile yellow substance above described was suggested as possible by some remarks of Claus in a recent paper on the tendency to reduction of salts of iridium (Ann. d. Chem. u. Pharm., Aug., 1858, S. 129). This author has shown that the platinum metals fall naturally into these groups, in each of which are contained two metals resembling in general habit and relations each other more closely than members of the remaining groups. Platinum and palladium constitute the first of these pairs, iridium and rhodium the second, osmium and ruthenium the third. The atomic weight of the first-mentioned member of each pair is higher than (nearly double) that of the second.

In the paper quoted Claus remarks that the metal of lower atomic weight in each of these groups is much more easily reduced than the other from superior to inferior grades of combination with chlorine; thus the bichlorid of palladium is reduced with much greater ease to proto-chlorid than is the corresponding compound of platinum; and, for the same season, probably, the bi-chlorid of rhodium is not known, but only the sesqui-chlorid, while both salts of iridium can be easily obtained. On this same principle Claus explains the fact that no oxyd of ruthenium homologous with osmic acid has been obtained, while he gives the following reasons for suspecting the existence of such an oxyd: "This opinion is based upon the fact, that in my preparation of compounds of ruthenium, which can be obtained only by energetic processes of oxydation, the material worked upon, notwithstanding all my care and economy, gradually diminished, and yet I have never succeeded in collecting a volatile product. Once only, when I had fused ruthenium, perfectly free from osmium, with caustic potash and nitre, dissolved the mass in water, and decomposed it with nitric acid, I observed a peculiar odor, quite distinct from that of osmic or nitrous acid; and afterwards, having covered the beaker, which was smeared on the edge with tallow, with a plate of glass, I

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remarked an unmistakable blacking of the tallow, caused by the reduction of a volatile metallic compound."

It seemed possible that the volatile yellow substance to which the present paper refers might have been an acid oxyd of ruthenium*—RuO_3, RuO_4, or RuO_5—and reducible with extreme facility, Claus and others having already noticed the reducing effect of light upon salts of the platinum metals. A portion of the crust from the sides of the tube of yellow acid was carefully examined for ruthenium, the various tests given by Claus as well as that recently proposed by Dr. Gibbs being made use of, but no proof of the presence of this metal could be obtained.

The properties of osmium and its compounds are very remarkable, and render it a matter of no little interest to trace the analogies of this rare substance and fix its place among the other elements. It is described in most chemical works along with platinum and its associated metals, mainly on the ground of community of origin, for in many respects it is unlike the platinum, palladium, rhodium, &c., with which it always occurs in nature. All these metals are commonly thought of as very infusible, of great density, very slightly affected by reagents, and very easily reduced from their compounds to the metallic state; when more closely examined they are found to differ from each other in many of their other properties. The arrangement by Claus of the platinum metals in these groups, each containing one metal of high and one of low atomic weight, viz.

Platinum,	Iridium,	Osmium,
Palladium,	Rhodium,	Ruthenium,

has been alluded to above; the two members of each group are more closely related to each other than to any of the rest. Osmium and ruthenium are clearly the most electro-negative of the series. Graham has inferred the isomorphism of platinum, palladium, iridium and osmium, from the fact that their potassio-chlorids all crystallize in the form of the regular octahedron; the corresponding compound of ruthenium has since been added to the list, while that of rhodium is still unknown. The occurrence of two salts under the same form, *in the regular system*, of

* If such a compound exist, an explanation may be found for the process by which Frémy has obtained a lower oxyd of ruthenium—probably the bin-oxyd—in crystals. He roasts the powder of platinum-residue in a stream of air drawn through a porcelain tube at a bright red heat; osmic acid volatilizes, and is said to carry with it *mechanically* the oxyd of ruthenium, which deposits upon fragments of porcelain placed in the cooler part of the tube. But the oxyd is in distinct crystals, and can therefore scarcely be conceived of as a powder borne along in a merely mechanical way by a stream of vapor; and, moreover, there is no reason for oxyd of ruthenium only being so borne along, while other substances of no greater density remain behind. Is it not more likely that a volatile and very easily reducible homologue of osmic acid is formed, and almost immediately afterwards decomposed, depositing the bin-oxyd of ruthenium?

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course does not of itself suffice to establish the relation of isomorphism between them; iridio-chlorid of potassium seems however to be capable of crystallizing in all proportions with the platino- and osmio-chlorids.

The interesting fact has been discovered by Claus that osmio-cyanid and ruthenio-cyanid of potassium are strictly isomorphous with the well-known ferro-cyanid, crystallizing with it in all proportions, and even giving very similar precipitates with various metallic solutions; so that, in these double cyanids, osmium and ruthenium are capable of taking the place of *iron*.

In the greater number of its relations, however, osmium presents itself as a member of the *arsenic group* of elements. This has been noticed by some recent authors, as by Prof. Dana in the arrangement of the elements adopted in his System of Mineralogy, and by Prof. Miller, who says in his lately published Elements of Chemistry that "it presents more analogy with arsenic and antimony than with the noble metals." Frémy too compares osmium in platinum ore to arsenic in the native arseniurets.

Nitrogen, phosphorus, arsenic, antimony, and bismuth are generally recognized as forming a distinct and natural group of elements, and into this group it seems from many considerations that osmium, and probably ruthenium, ought to be introduced. They have some analogies with other natural families, just as arsenic is allied to sulphur in native sulph-arseniurets, and nitrogen and chlorine exhibit some resemblance in the nitrates and chlorates, but here appear to lie their closest relations. It may be interesting to notice some of the principal points of resemblance to or difference from this group.

Iridosmine occurs in crystals closely related in form to those of arsenic, antimony, and bismuth in the metallic state. The analyses of iridosmine are not yet sufficiently numerous or accurate to enable us to decide upon its normal composition, but it seems probable that the two metals occur in variable proportions, and are in this mineral isomorphous, thus establishing, as noticed by Dana, a connection between the arsenic group and that of the distinctly basic metals, as the arsenic and sulphur groups are united through homœomorphous bismuth, tetradymite, and tellurium. Dana places iridium in the same section with iron, among the metals whose most stable grades of oxydation are the prot-oxyd and sesqui-oxyd, but the statement of Claus that the *bin-oxyd* of iridium is the most stable and easily prepared compound with oxygen would remove this metal, as also perhaps platinum and palladium, from the iron section to that containing tin and titanium, and the propriety of this transfer may be supported by the relationship of Frémy's crystallized oxyd of ruthenium (doubtless the bin-oxyd) examined

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by Sénarmont: This was found to be homœomorphous with stannic and (the rutile form of) titanic acid. The bi-chlorid of tin and potassium too is reported as crystallizing in regular octahedrons, like the corresponding salts of iridium, platinum, and palladium.

The arsenic section, as given by Dana, includes nitrogen, phosphorus, arsenic, antimony, bismuth, osmium, and tellurium. The last-named is marked as doubtful, and should decidedly be placed with sulphur and selenium, to which it is analogous in by far the greater number of its compounds.

In one of the interesting memoirs lately published by Dumas on the numerical relations subsisting among the atomic weights of the elements, the arsenic series is thus given:

	Atomic weights.
Nitrogen,	14
Phosphorus,	14+17=31
Arsenic,	14+17+44=75
Antimony,	14+17+88=119
Bismuth,	14+17+176=207

and the parallelism of this series with that of chlorine, iodine, &c., is supposed to be shown in the following lines:

F (19)	Cl (35·5)	Br (80)	I (127)
N (14)	P (31)	As (75)	Sb (122)

in which a common difference of 5 is assumed between the two members in each of the vertical columns (a difference not strictly brought out in the case of phosphorus and chlorine), and in which antimony is given a higher atomic weight than in the preceding table. Osmium is not included, but in a supplemental note since published we find it placed, with an equivalent somewhat higher than that usually adopted, in the *sulphur* group, serving to complete the following two lines of equivalents:

Mg (12·25)	Ca (20)	Sr (43·75)	Ba (68·5)	Pb (103·5)
O (8)	S (16)	Se (39·75)	Te (64·5)	Os (99·5)

between the paired members of which a common difference of 4 is supposed to exist.

Let osmium and ruthenium be brought into the arsenic group, and the series of atomic weights will then stand thus:

	Atomic weights.
Nitrogen,	14
Phosphorus,	14+17=31
Ruthenium,	14+17+22=53
Arsenic,	14+17+44=75
Osmium,	14+17+66=97
Antimony,	14+17+88=119
Bismuth,	14+17+176=207

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The atomic weights of ruthenium and osmium are here assumed as 53 and 97, numbers not differing more widely from those commonly received—52·2 (Claus) and 99·6 (Berzelius)—than do several of those assumed by Dumas. Our knowledge of these two equivalents is based upon very limited data, and can but be looked on as approximative merely. As regards osmium, Frémy says that in several experiments he has obtained an equivalent number lower than that given by Berzelius, and the vapor-density of osmic acid, which we shall notice presently, points to an equivalent close to 97. A re-determination of this equivalent is very much to be desired.

Taking the series as given above, we find ruthenium and osmium to fall in between phorphorus and arsenic—arsenic and antimony; the numbers from phosphorus to antimony increasing by 22—44—66—88, just as in the following group given by Dumas:

	At. wts.
Chromium, - - -	26
Molybdenum, - - -	26+22=48
Vanadium, - - -	26+44=70
Tungsten, - - -	26+66=92

and we may arrange the two series in parallel lines,

P (31)	Ru (53)	As (75)	Os (97)
Cr (26)	Mo (48)	V (70)	W (92)

These numerical relations are of very little importance in themselves, when we employ the small numbers of the hydrogen scale of equivalents, and especially when we permit ourselves to alter the numbers themselves to any extent, however small, but they acquire more interest when they present us with groupings of elements which we acknowledge on other grounds to be naturally related. In such cases, when the homology is distinctly marked, we may even be justified in taking some liberties for the moment with the numbers standing, often with but slender evidence to support them, for the equivalents of the less-known elements; and we may, perhaps, thus be directed to errors of determination which future experiments will clear away.

The bodies named in each of the two lines just given are homologous in many respects besides that of atomic weight, and a connection between the two series, through vanadium, has lately been shown by Schafarik. There is a clear resemblance running through the formulæ and properties of their oxyds. In the chromium series—a very natural one—the most important oxyds are the metallic acids of the composition MO_3; we have also in each case a bin-oxyd, MO_2; but the sesqui-oxyd is prominent only in the case of chromium itself, and indicates the relation of this metal with iron.

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In the arsenic series the known oxyds are the following:

	P_2O					
NO	PO	RuO	AsO(?)	OsO	SbO(?)	BiO(?)
		Ru_2O_3(?)		Os_2O_3(?)		
NO_2		RuO_2		OsO_2		
NO_3	PO_3	RuO_3	AsO_3	OsO_3	SbO_3	BiO_3
NO_4	PO_4(?)			OsO_4	SbO_4	BiO_4
NO_5	PO_5		AsO_5	OsO_5	SbO_5	BiO_5(?)

The prominent compounds in the table are the acids, MO_3 and MO_5; with respect to the separate columns the following facts are noticeable.

The oxyds of nitrogen are well known; the regularity observable in this column causes it to be frequently used as an illustration of the "law of multiples." NO and NO_2 are usually said to be neutral, but the latter plays the part of a base in contact with sulphuric acid, as in the crystals of the oil of vitriol chambers, and possibly the former may do so too in the nitrosulphates (KO, NO, SO_3 and NH_4O, NO, SO_3?) obtained by Davy by bringing nitric oxyd in contact with an alkaline sulphite. NO_3 and NO_5 are well-known acids. It is doubtful whether hypo-nitric acid (NO_4) is capable of combining with bases and forming salts; in contact with the alkalies it yields a mixture of nitrites and nitrates, yet, *when out of contact of bases, it seems to be a body of more stability than either* NO_3 *or* NO_5 (*anhydrous*).

In the column of the oxyds of phosphorus, we have first the very anomalous sub-oxyd (P_2O), which is probably the only marked exception to the homology running through the whole table. Before the discovery of red (amorphous) phosphorus by Schrötter this substance was, no doubt, to some extent confounded with phosphoric oxyd, and may even now throw some doubt upon the cases in which the latter seems to have been obtained pure and to have yielded a formula supported by trustworthy analyses. PO, unlike the other protoxyds of the series, is usually considered an acid, but as it has not been obtained in the separate state, and all the hypophosphites contain water, it may be reasonably assumed that the formula of the acid should include hydrogen. PO_4 is doubtful; this may, perhaps, be the composition of Pelletier's phosphorous acid, produced by the slow combustion of phosphorus, a body which undergoes no further oxydation by prolonged exposure to the air, and which, in contact with bases, yields mixed phosphites and phosphates. The last term in the column—phosphoric acid—is well known.

The existence of a distinct protoxyd of arsenic, as of antimony and bismuth, is doubtful. Arsenious acid is *a feeble, volatile, me-*

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tallic acid—feebler in its relations as an acid than arsenic acid, and volatilizing at a lower temperature than the latter. Arsenious acid, moreover, volatilizes at a temperature below that required by metallic arsenic.

In the antimony column, the oxyd SbO_3 is usually viewed as a weak base, but seems also to be capable of uniting as a feeble acid to the alkalies, and even of expelling carbonic acid from their carbonates (Liebig). The iso-dimorphism of SbO_3 and AsO_3 is well established. SbO_3 is volatile at quite a moderate temperature, while metallic antimony requires at least a white heat to vaporize it. SbO_5 is a body of distinctly acid properties. *Both* SbO_3 *and* SbO_5 *are converted by heating in the air into* SbO_4 —the so-called antimonious acid—which seems therefore to be the most stable oxyd when strong bases and acids are not present. It is most probable that, as Frémy maintains, SbO_4 is not itself an acid, but that a so-called alkaline antimonite is in fact a mere mixture of an antimoniate with the compound of antimonic oxyd and alkali ($2SbO_4 = SbO_3 + SbO_5$).

In the bismuth column, the teroxyd is homologous as a base with teroxyd of antimony, but shows little tendency to play the part of an acid with even the strongest bases. This oxyd and the metal itself are volatile at high temperatures. BiO_4 also seems to be devoid of acid properties, but the compound BiO_5 probably exists, and is homologous with SbO_5, forming alkaline salts of little stability.

Comparing now ruthenium and osmium with the above recognized members of the arsenic group, we find first that both metals form protoxyds, which are feeble bases, as are probably the corresponding compounds of the other members of the group. We next meet with the sesquioxyds, whose formula is exceptional in the series, but for neither metal has this grade of oxydation been obtained in the free state and pure, and in the case of osmium its existence may be gravely doubted. Anhydrous Ru_2O_3 is supposed by Claus to be formed during the roasting of metallic ruthenium in the air at a high temperature, but only on the ground that the absorption of oxygen slackens when *about* enough has been taken up to form this compound, and that the proportion necessary for the binoxyd is never fully attained. Claus, however, describes a sesquichlorid with which double salts are formed by the chlorids of potassium and ammonium, and we must therefore assume a sesquioxyd also. Sesquioxyd of osmium is quite unknown in the separate state, and the belief in its existence is founded solely upon the preparation by Berzelius of a dark brown substance, supposed to consist of the sesquioxyd united to ammonia, which, dissolved in hydrochloric acid, yields a brown compound, supposed to be the sesquichlorid of osmium and ammonium. Neither of these, however,

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can be crystallized, nor has the constitution assigned to either been supported by an analysis. The so-called ammonio-sesquioxyd detonates when heated, (sometimes with much violence, as I have noticed in removing by heat the deposit of this substance which forms on the end of a retort-neck during the distillation of osmic acid into a receiver containing ammonia,) and hence is probably analogous to fulminating platinum, containing perhaps the binoxyd of osmium. The binoxyd itself is a feeble base, the characteristic color of whose salts in solution is yellow as is the case with the corresponding compounds of iridium. Similar remarks apply to the binoxyd of ruthenium—probably the body obtained, as we have shown, by Frémy in crystals. The teroxyd of osmium is the body supposed to have been isolated in the experiment described at the beginning of this paper. Its position as a feeble acid, capable, however, under some circumstances, of playing the part of a base, its fusibility and volatility (greater, apparently, than those of osmic acid, as nitrous acid is more fusible and volatile than hypo-nitric), its probable crystallization in octahedrons of the regular system, in which arsenious acid and teroxyd of antimony are also found, all tend to indicate homology with the other teroxyds of the arsenic group. The general relations of ruthenic acid, so far as these are known, place it in a similar position. Just as we find hyponitric acid (NO_4) and antimonious acid (SbO_4) to be the most stable of the higher oxyds of nitrogen and antimony, so the well known osmic acid (OsO_4) seems to be the grade of oxydation which osmium most readily assumes and retains when not in contact with bases. OsO_3 and OsO_5 (the latter as described by Frémy) seem scarcely capable of existing in the separate state; when set free from their salts they soon pass into OsO_4; while it may as well be doubted that the latter ever exists as a distinct acid in combination with bases as that NO_4 or SbO does so. No so-called osmiate has ever been analyzed; the saturating capacity of the acid, if it be such, is unknown; when free and in solution in water it has no acid reaction, it does not displace carbonic acid from the carbonates, and it is itself expelled by heat from most of its supposed compounds, and is separated in part by water even from potash and soda. No compound of OsO_4 with a base has been obtained in crystals, while Frémy states that he has crystallized the alkaline salts of both OsO_3 and OsO_5. RuO_4 and RuO_5 are as yet unknown.

The tendency throughout the whole arsenic group is manifestly to the production of the acid compounds MO_3 and MO_5, the former the more fusible and volatile body, the latter the stronger acid. In addition we have some cases of the protoxyd (MO), a feeble base, and the binoxyd (MO_2), a body of still more feebly basic properties, verging upon the acids. All other grades of

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oxydation, so far as they exist at all, may perhaps be correctly viewed as compounds of the preceding *inter se*. The stability of the oxyd (MO_4) in the separate state is remarkable—its formula is one of rare occurrence.

The affinity of all the elements of the group for oxygen is considerable; it is so even in the case of osmium and ruthenium, usually placed among the noble metals. Dumas (*Traité de Chim. app.*) states that osmium does not oxydize at common temperatures, nor even at 100° C., but I have obtained conclusive evidence that oxydation may go on slowly even at the ordinary atmospheric temperature. The paper label and the cork of a tube containing pure metallic osmium have in the course of several years become blackened, precisely as organic matter is by the fumes of osmic acid, the black tint on the paper decreasing from the mouth of the tube along the outside. A piece of white paper in which some black platinum residue had been wrapped, was strongly stained in the immediate neighborhood of the powder in the course of a few weeks. *The same effect is distinctly observable even upon the paper label placed inside a tube of native iridosmine* (Siberian) *in the usual coarse grains*—a specimen which has lain among other minerals, and has never been placed near any artificial preparations of osmium. Osmium, like arsenic and antimony, is clearly capable of slowly taking up oxygen at common temperatures. At a red heat, roasting in a current of air affords, as is known, a good method of obtaining osmic acid from the iridosmine of platinum residues—just as by similar roasting arsenious acid is prepared from the native arseniurets.

It would be a matter of much interest to compare osmium with its supposed homologues under circumstances in which we should expect it to play an electro-negative part. Frémy has announced his belief in the existence of an *osmiuretted hydrogen*, but such a body has not yet been isolated and described. Compounds of the metal with ethyl, methyl, &c., would be well worth examination, and it is not unlikely that such might be prepared from a body which in some states of combination exhibits such a high degree of volatility.

The earlier experiments of Deville and Debray upon the platinum metals seemed to have shown that both osmium and ruthenium could be volatilized, at exceedingly high temperatures, without previous fusion; if this were confirmed, a strong point of resemblance with arsenic would be made out, but it appears from a more recent paper that osmium at least may be fused and obtained as a perfectly compact mass, the apparent volatility of the metal being due doubtless to previous oxydation, the crucibles used being permeable to air. We have seen, as regards arsenic and antimony, that their oxyds are more volatile than the metals themselves.

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It is lately stated that osmium may be obtained in crystals by the same means as those used for boron and silicon, but I have as yet seen no account of the form which it assumes.

Deville has furnished another interesting fact with respect to osmium, by determining the density of the vapor of osmic acid, which he has found = 8·88. This, if we take the generally received atomic weight for osmium, gives the atomic volume $\frac{131·6}{8·88}$=14·82, indicating a condensation to 2 vols. If we now calculate back to the theoretical atomic weight we get (14·57× 8·88)−32=97·38, a number closely approaching 97, which, as we have seen, brings the equivalent of osmium into simple and harmonious relation with those of the other elements of the arsenic group.

The specific gravity of fused metallic osmium having been lately determined by Deville =21·4, there can be little doubt that all the metals of the platinum family possess the same atomic volume when in the free state, about 4·6 or 4·7; the specific gravity of ruthenium is not yet known with accuracy, but such experiments as have been made render it improbable that it will prove an exception. This number is about one-fourth the mean of the at. vols. of the long recognized members of the arsenic group, but these latter differ so widely among themselves* that the comparison is of little or no value. It would be desirable to get a good determination of the density of osmic acid in the solid state, so that its at. vol. might be calculated and compared with that of antimonious acid.

The specific heat of osmium, so far as its value as a physical character goes, *opposes* the introduction of this element into the arsenic group. It has been determined by Regnault =·03063; multiplying now by the equivalent 97, we have the product, 2·9711, thus placing osmium in the list of the elements (including the majority) for which the product of sp. ht. by at. wt. is nearly 3, while for phosphorus, arsenic, antimony and bismuth the product thus obtained is twice as great, or about ·6. In this respect, however, osmium probably resembles nitrogen—the latter examined, as it necessarily is, in the gaseous form.

It is to be hoped that the conducting power for heat and electricity of compact osmium will soon be examined; nothing is as yet known of these characters.

* Phosphorus, - - - - $\frac{31}{1·83}$ (*Schrötter*) =16·94

Arsenic, - - - - $\frac{75}{5·67}$ (*Herapath*) =13·23

Antimony, - - - - $\frac{119}{6·7}$ (*Karsten*) =17·76

Bismuth, - - - - $\frac{207}{9·8}$ (*Marchand & Scheerer*) =21·12

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Lastly, as regards the magnetic relations of the element—it is placed, with some doubt, by Faraday in the paramagnetic class; the metal and its protoxyd were found to act feebly in this sense, while pure osmic acid is said to have shown itself clearly *diamagnetic*. The strongly diamagnetic character of phosphorus, antimony and bismuth would render a re-examination of this point interesting. Arsenic, however, is said to be very feebly diamagnetic, and is placed by Faraday close to osmium in the list of metals examined, though on the opposite side of the line of magnetic neutrality or indifference.

Reviewing, now, the united physical and chemical characters of osmium, and comparing them with those of the generally recognized members of the "arsenic group," we are, I think, justified in concluding that here this curious metal should be placed in a natural arrangement of the elements—while important distinctions seem to separate it from some, at least, of the platinum metals, with which it is usually associated and described.

Tuscaloosa, Ala., Nov. 1, 1859.

ART. VII.—*The Comas and Tails of Comets;* by Prof. W. H. C. BARTLETT, U. S. Military Academy at West Point.

COMETS have, at all times, been objects of curiosity and wonder; and the question in regard to the nature of their luminous appendages, has exercised the speculative ingenuity of philosophers from the earliest records of astronomy. Everything written about them is read with interest, and the most extravagant theories in respect to their constitution and the laws of their being find a ready favor with the public. They are still among the enigmas of the heavens. Among the recent and remarkable efforts at solution, is one by the ablest mathematician of the country, perhaps of the age: and granting the premises, there is no avoiding the conclusions of the comprehensive and searching analysis for which this eminent man is so remarkable. But the assumption, that the attractive energy which summons a comet from the depths of space to the presence of the sun, retains its nature unchanged and strengthens with the diminution of distance for a part of the approaching mass, and yet reverses its character and becomes repulsive for another part, in order to obtain material to build up the tail, appears so unsupported by the analogies of nature as to give to his results the taint of improbability. Indeed, a theory which demands such an exercise of faith in matters of science, and from such friends, can only inspire doubt, and should yield the place it has too long occupied

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to some other, founded in better ascertained laws of matter. The question is not one of pure mathematics, but of physics.

The material elements of all bodies of which we have any knowledge, are united by some conditions of aggregation, determined by the reciprocal action of molecular forces; and the circumstances of their relative motion will come from the equation

$$\Sigma m.\left[\left(\frac{d^2x}{dt^2}-X\right)\delta x+\left(\frac{d^2y}{dt^2}-Y\right)\delta y+\left(\frac{d^2z}{dt^2}-Z\right)\delta z\right]=0;$$

in which m is the mass of an element, xyz its coördinates of place, X Y Z the sums of the components of impressed accelerations in the direction of the axes $x\,y\,z$, respectively.

The conditions of aggregation may be expressed in some functions of the coördinates of molecular places. As three coördinates determine the place of a single molecule, there will be three times as many coördinates as molecules; and if μ be the number of molecules and λ the number of equations that give the conditions of aggregation, then will $3\mu-\lambda=n$ be the number of coördinates which, if given, would reduce the number of unknown coördinates to the number of equations. These unknown coördinates could then be found, and the places of the molecules at the corresponding instant determined.

Denote the λ coördinates by $x\,y\,z$, $x'\,y'\,z'$, &c., and the n coördinates by $\alpha\,\beta\,\gamma$, $\alpha'\,\beta'\,\gamma'$, &c. The former of these coördinates, as also the forces, may be expressed in functions of the latter, and both eliminated from the general equation of motion. And if $\xi\,\eta\,\zeta$, $\xi'\,\eta'\,\zeta'$, &c., be the increments of $\alpha\,\beta\,\gamma$, $\alpha'\,\beta'\,\gamma'$, &c., at any instant and due to any transmitted initial disturbance, it is easily shown that

$$\begin{aligned}
\xi &= \Sigma\, R.N_{\xi}.\sin\left(t.\sqrt{\varrho}-r\right),\\
\eta &= \Sigma\, R.N_{\eta}.\sin\left(t.\sqrt{\varrho}-r\right),\\
\zeta &= \Sigma\, R.N_{\zeta}.\sin\left(t.\sqrt{\varrho}-r\right),\\
\xi' &= \&c. \qquad \&c. \qquad \&c.
\end{aligned}$$

In which there are n terms comprehended by the sign Σ, and in which ϱ will, in general, have different values from one term to another. When these values of ϱ are real and positive, the different terms in the values of $\xi\,\eta\,\zeta$, &c., will disappear periodically, the precise times of disappearance being given by

$$t.\sqrt{\varrho}-r=a\pi;\quad t'\sqrt{\varrho'}-r'=a'\pi;\quad \&c.,\ \&c.$$

or

$$t=\frac{a\pi+r}{\sqrt{\varrho}};\qquad t'=\frac{a\pi+r'}{\sqrt{\varrho'}};\quad \&c.,\ \&c.$$

in which a is any whole number. The intervals of disappearance will be

$$\frac{\pi+r}{\sqrt{\varrho}};\quad \frac{\pi+r'}{\sqrt{\varrho'}};\quad \&c.\quad \&c.;$$

and if these intervals be commensurable, all the terms will dis-

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appear simultaneously, and $\xi\,\eta\,\zeta$, &c., reduce to zero, at equal intervals of which the duration is

$$t_i = \frac{\pi + r}{\sqrt{\varrho}} \times \frac{\pi + r'}{\sqrt{\varrho'}} \times \&c. \times \&c.$$

The vast atmosphere of ether which pervades all space is ever busy transmitting luminiferous waves from the sun and other heavenly bodies. Its molecules are ever on the move with velocities and in orbits determined by the relative places and intensities of existing wave sources. Any cause which will perturbate the etherial molecules of any limited volume of ether from these orbits, regarded as initial, and by the quantities $\xi\,\eta\,\zeta$, $\xi'\,\eta'\,\zeta'$, &c., will make such volume *self-luminous;* and if the perturbation be great enough, it will be visible from all directions.

Comets are known to exist in a state of great tenuity, their densities being almost insignificant in comparison with that of the fleecy clouds that float in the upper atmosphere. The luminiferous waves from the sun, entering such bodies with great ease, their intromitted greatly preponderate over their reflected components. The former of these components modify and determine the internal motions of comets, and make them *self-luminous.* The internal cometary elements become so many centres of disturbance. They throw their waves in all directions, and are simultaneously sources of molecular perturbations to the surrounding ether, each giving rise to a term $\mathrm{R.N.}Sm\,(t.\sqrt{\varrho} - \gamma)$, in the general value of the perturbating functions $\xi\,\eta\,\zeta$, &c., and thus making the ether also *self-luminous.* The degree of illumination will vary with the maximum values of the perturbating functions. These will result, in any case, from the extent of the initial disturbance and the distance, at right angles thereto, over which the disturbance may have been propagated; decreasing, according to the principle of wave divergence, as the square of this distance increases. The components of the initial disturbance perpendicular to the line drawn from the comet to the sun, is, from the principle of wave propagation, much greater than in any other direction; and hence the much greater extent of the illumination on the side of the comet opposite the sun. The comet's head can have no phases, from its self-luminosity; neither can the coma and tail have sharply defined outlines, from the gradual degradation of molecular perturbations towards their borders.

This view denies the presence of cometary material in the coma and tail altogether, and regards these appendages but as phenomena due to the reciprocal action of the etherial and cometary molecular forces. According to it, the coma and tail become, as it were, *a luminous shadow,* a part of which is literally "cast before," and the dark cap which envelopes the head and

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stretches away through the tail, a region of *wave interference*. No wonder, then, that comets turn their tails from the sun, and, at perihelion, whisk them, though of enormous lengths, through celestial arcs well nigh equal to a semi-circumference, in a few hours. This is no more surprising than that opaque bodies throw their shadows from the luminous sources whose light they intercept. The curvature, which is so remarkable a feature in the tail, is but the simple effect of the comet's orbital velocity, and the progressive motion of light.

If the principles here cited be well founded, then will the zodiacal light find an easy solution; and the great oblateness of its spheroidal figure must be taken as evidence that the component molecular motions in the sun are greater in the direction of the solar axis than in any other.

West Point, Oct. 25th, 1859.

ART. VIII.—*On Sodalite and Elæolite from Salem, Massachusetts;* by J. P. KIMBALL, Ph.D.

FOR a knowledge of this locality of the occurrence of the two rare silicates, sodalite and elæolite, we are greatly indebted to Gilbert L. Streeter, Esq., of Salem, as well as to several other gentlemen of the same city. Fortunately, Mr. Streeter very carefully observed their mode of occurrence, and, together with G. F. Cheever, Esq., and Rev. S. Johnson, Jr., collected choice specimens of them. The best of these are in the possession of the Essex Institute, Salem, to the curator of which, Dr. Henry Wheatland, I owe in a great measure the privilege of examining them.

The locality in which the minerals were found is "a pit or quarry, a short distance below the Almshouse upon the road passing along the northern side of the Neck, towards Hospital Point."* They were first noticed in a "block of compact syenite resting upon the bank, the end of which presented a beautiful coloring of blue and greenish white, with specks of black. Upon examination these conspicuous minerals were seen to be in a vein, a portion of which was connected with the block of syenite."†

Mr. Streeter subsequently discovered what undoubtedly was the continuation of the same vein. This traversed an erratic block of the same rock, imbedded in the drift, of which the small block, just mentioned, was a fragment. The vein is described to have been about six feet in width, and to have dimin-

* G. L. Streeter: Essex Institute Proceedings, ii, 153. † Ib.

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ished in thickness "wedge-like to a mere line at the termination." Although this vein-stone was identical in its character with that of the smaller block, its yield of fine specimens of sodalite and elæolite was less abundant than that of the latter. Unfortunately enough, the discovery of the minerals was not made in time to rescue this precious vein-stone from the hands of the quarrymen. Large masses of it including, it is believed, the best specimens, had been carted away and buried deep beneath a littoral road along Collins' Cove. The quarry was opened in the autumn of 1855. I visited it last spring when it was not being worked, and found amongst the debris of the quarry very good specimens of both minerals. At that time a portion of the boulder which contained the vein was still left. This I identified as a syenitic porphyry (Quarzfreier Porphyr of Senft). It is characterized by remarkably entire crystals of oligoclase of a greenish color, which make up the base of the rock. Thickly disseminated through the base are minute grains of hornblende and scales of mica.

Besides the sodalite and elæolite, the vein-stone is composed of orthoclase for the greater part; biotite in black tabular prisms; small crystals of zircon in octahedral prisms; fine stellate brownish-yellow flakes of xanthosiderite; and (probably) albite in small, irregular, reddish, granular masses.

At Litchfield, Me.—the only other known locality in America where sodalite and elæolite occur together—these minerals are further associated with cancrinite and, as at Salem, with zircon; but instead of occupying a vein as in the Salem instance, exist as accidental constituents of a granitic rock composed of quartz, feldspar and black mica,* thus constituting a miascite analogous to that of the Ilmen mountains.† The Litchfield rock, to be sure, is found only as erratic blocks; but the absence of cancrinite in the Salem boulder, and the dissimilarity between this and the Litchfield rock as to petrographic character, tend to preclude the possibility of the two having a common source.

Sodalite.—The sodalite from Salem has quite the same character as that from Litchfield and the Ilmen Mts., with specimens of which I have been able to compare it, excepting that the former in common with the elæolite, is contaminated with minute particles of what appears to be mica, thus rendering it very difficult to glean perfectly pure mineral for analysis. It is in crystalline, sub-translucent masses having an indistinct cleavage. Its lustre is greasy, and its color beautiful lavender blue.

Three separate determinations of its specific gravity were made with different portions of the mineral, giving the results as follows: 2·294, 2·303, 2·314.

* J. D. Whitney: Poggendorff's Annalen, lxx, 434.
† Senft: Classification und Beschreibung der Felsarten, 218.

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Two portions of the mineral were used for the analysis. The one portion was treated with nitric acid, and the chlorine determined as chlorid of silver. The other portion was treated with diluted hydrochloric acid, whereby the silica, alumina, lime and soda were determined according to the customary methods.

Prof. J. D. Whitney,* in his analyses of the sodalite and its associated minerals from Litchfield, has so fully observed their chemical properties as to render superfluous here any remarks on the same subject.

Calculating all the sodium as soda, the following results are obtained:

Silica,	37·33
Alumina,	32·70
Oxyd of iron,	*trace.*
Soda,	24·31
Chlorine,	6·99
	101·33

But on the other hand, assigning to the percentage of chlorine enough of sodium to form chlorid of sodium in accordance with von Kobell's formula of this mineral ($\dot{N}a^3\ddot{S}i+3\ddot{A}l\ddot{S}i+NaCl$), we have 18·17 for the percentage of soda in combination with silica.

Hence the analysis will stand thus:

Silica,	37·33	
Alumina,	32·70	
Soda,	18·17	
Sodium,	4·57	NaCl
Chlorine,	6·99	
	99·76	

These results are sufficiently in agreement with the established formula.

Elæolite.—The elæolite from Salem possesses all the constant physical properties of this variety of nepheline. Its color is dull green, its lustre greasy, and its fracture sub-conchoidal. It is sub-translucent, and in structure massive. Its specific gravity is 2·629. Its chemical composition I find to be as follows:

	I.	II.
Silica,	44·31	44·07
Alumina,	32·80	
Peroxyd of iron,	*trace*	
Lime,	·40	
Soda,	16·43	
Potash,	5·50	
Ignition,	1·47	
	100·91	

Schenectady, N. Y., September, 1859.

* Poggendorff's Annalen, lxx, 433.

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ART. IX.—*Description of Nine new species of Crinoidea from the Subcarboniferous Rocks of Indiana and Kentucky;* by SIDNEY S. LYON and S. A. CASSEDAY.

PTEROTOCRINUS. *Lyon and Casseday.*

Asterocrinus, Lyon, Geol. Rep. Ky., vol. iii, p. 472.

SINCE our description of Asterocrinus was published in the 3d vol. of the Kentucky Report, we find the name to have been appropriated previously. We therefore propose Pterotocrinus as the name of our genus, which has now four well authenticated species.

Generic formula.

Basal pieces,	2	Mouth central,	1
Radial pieces, 1st series,	5	Column round, (?)	
" " 2nd series,	10	Arms ciliated and single,	20
" " 3rd series,	20	Wings or lobed pieces,	5 variously formed.
Anal piece,	1 or more.		

Pterotocrinus depressus, n. s.

Body, depressed, subconical, twice as wide as high; below the free arms it presents the form of a very shallow, flattened cup, the pieces composing it smooth and of equal thickness, in some cases nearly a plane, the margin curving suddenly upward at the junction of the arms; the vault rises from the arms at a very low angle, rapidly increasing towards the centre, where it is nearly perpendicular. *Column*—very small, formed near the body of circular pieces of unequal size and thickness. *Basal* pieces, two, similar in size and shape; not prominent. When joined, they together form an irregular pentagon, raised a little above the general surface of the cup; slightly indented at their junction with the column, which has one or more pieces buried in the pit in which it is attached.

Radial pieces of the first series, five; subquadrangular, similar in form and size, nearly twice as long as high. Radial pieces of the second series, ten, subquadrangular, differing slightly both in form and size, resting near the centre of the outer margin or the first radials, a little more than half covering them. The radials of the third series are twenty in number, differing slightly in size, subquadrangular, nearly twice as wide as high, ten of them resting upon the radials of the second series, the other ten resting partly on these and partly on the first radials. The radials of the third series support from four to five brachials of quadrangular form, four times as wide as high; from the summits of this last series the arms, which before had been horizontal, become quite erect, and are composed of a double row of pieces which join by angular sides in the center of the arms.

Anal piece, one; obtusely angular below, fitting into a depression of the basal pieces, rounding to a point above, rising above the first radials.

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The vault is divided into five triangular spaces by the wings, five in number; each space is covered with seven pieces of a compressed hexagonal form, three rising from the arms from the first row; upon these rest two; in the angular space at the summit of these rests the sixth, similar in size to those below it; it is squarely truncated above, thus becoming pentangular; its upper margin supports a small quadrangular piece; there are some quite small pieces above this last, in our specimen, probably one row which may form the mouth; between each of the five fields of the vault is a long lanceolate piece as wide as the other pieces of the vault, and about three times as long as those in the fields; the wings are attached to these long pieces by an articulating joint, and extend beyond the arms which embrace the vault, diminishing in thickness from their attachment outward, and terminating in a thin knife-like edge, equal in width to twice the height of the vault, the upper and lower margins nearly parallel; obtusely rounded at their outer extremity.

The arms are single, twenty in number, lying in sets of four between the wings, which, being placed immediately above the centre of the first radials, divide them into pairs, two from each of the adjacent radials falling into the spaces between each pair of wings. The arms are provided with a row of cilia for each side, formed of short joints, placed immediately in contact one above the other, filling the length of the arms which rise above the wings, about one sixth of their length. The dermal coating has been lost from all the specimens we have seen. The sides of the wings are strongly marked by a muscular attachment, by which they were moved. (?)

By reference to the figures it will be evident that this differs from all heretofore described species; by the bipartite basal pieces it is related to *Dichocrinus*.

Dimensions of Specimen of Medium Size.

Height of base, - - - -	·20 inches.
" " vault, - - - -	·30 "
Diameter of arms, - - - -	·75 "
Length of " - - - -	·70 "
Greatest width across the wings, - -	1·69 "
Height from base to top of wings, - -	·85 "

Geological Position and Locality.—Several specimens have been found in the third intercalated limestone, of the Millstone grit, Grayson Springs, Grayson County. Same horizon, near Dr. Baker's furnace, Edmonson County, Ky.

Pterotocrinus pyramidalis, sp. nobis.

Body without the arms and winged appendages:—the vault is a pyramidal pentagon, nearly twice as high as the greatest diameter at its junction with the calyx. The calyx is vasiform, the rim of the vase slightly reflected downwards: four times as wide as high. Columnar pit deep, irregularly oval, the longest diameter transversely disposed with reference to the anal side. Columnar facet round, perforation not visible (in any specimen that has come under our observation).

Basal pieces, two; prominent, outer margins thick and rounded, joined by a straight line to each other, the posterior margin having a deep angular notch, while the anterior side is but slightly indented at the

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junction of the pieces; both pieces are irregularly pentagonal, nearly of the same form; that on the right of the anal piece being a little the largest.

First radial pieces, five; broad, sides diverging from below upwards, three times as wide as high, the ends of the upper margins parallel to the lower; about one-half of the length of the upper margin circularly depressed for the reception of two of the second radials to each.

Second radial pieces, ten; small, obscurely quadrangular, resting in the circular depression at the summit of the first radials, each pair being separated one from the other by a very minute anomalous piece, which rests on the center of the first radials between them; it is nearly round, and rises under the suture which marks the separation of the second radials, and is about one fourth the height of those pieces.

Third radials, twenty; small, differing both in size and form; nearly as high as wide, divided into five groups, disposed on a curved line, the exterior pair of each group resting partly on the first radial and partly against the oblique outer margins of the second radials. The central pair of all the groups rest upon the second radials, against those on either side and against each other. The articulating surfaces to which the arms were attached are slightly concave; the upper side deeply indented by a perforation into the body, partly in the arms and partly in the pieces immediately above them.

Anal piece, one; large, as compared with other species of this genus, long, hexagonal, obtusely pointed below, resting in the deep angular notch in the basal pieces; diminishing gradularly upward, supported on either side by the first radials; acutely pointed above, reflected toward the body, and supporting on either side one of the third radials.

Our specimens are weathered to such an extent that all surface markings (if they ever existed) have been removed.

Summit.—The summit above the arms is divided into five fields, nearly of the same form and size, that above the anal side being a little the largest; they are covered by hexagonal or pentagonal pieces higher than wide. The largest field has four pieces in the first row, the other fields having only three each; the outside of each of the first series is articulated with a piece at the base of the wings, the second range and the superior margins of the third radials. Dividing the first range of pieces are five projecting angular pieces inserted between the groups of the first range, touching the third radials at one of its angular points, rising from them by a line slightly curved outward; they articulate with the first and second range of the five fields and with the wings, in the same manner as *P. depressus* (nobis). The second range consists of three long pentangular pieces, in the field above the anal piece, and two to each of the others. The third range consists of one lanceolate piece to each of the regular and two (?) to the irregular side. Between the fields of the summit is a broad articulating surface, about as wide as the pieces covering the fields, formed by the reflected margins of those pieces, and the supporting piece at the base of the wings. *Wings*—none have been found attached; great numbers are found loose, which we refer to this species. They are of various forms. The articulating surface applied to our specimen precisely fills and fits the articulating surface upon it; they are thick near the junction with the body, curved both above and below, gradually running to a point, five times as long as thick, broad on the upper surface,

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gradually thinning downward; the outer end round and pointed; they are frequently found bifurcate near the end and double pointed: affixed to the specimen they radiate regularly and horizontally, the points being about as high as the summit of the specimen.

Mouth, central.

Arms, twenty; form unknown.

Column, unknown.

Dimensions.

Greatest diameter of calyx,	·80 inch.
" " " basal pieces,	·49 "
" " " columnar facet,	·05 "
Height of calyx,	·18 "
Height of specimen, small part lost,	·75 "
Length of wing articulations, with body,	·45 "
Width " " " "	·12 "
Height of first radial pieces,	·12 "
Width " " "	·35 "

Geological Position and Localities.—Found only in beds near the top of the third limestone of the Millstone grit series of Edmondson, Grayson and Breckinridge counties, Kentucky.

Fragments very abundant, good specimens rare. Beds from one to two feet thick are found composed of a mass of the remains of this crinoid cemented together, forming a distinctive and characteristic bed of the 3rd limestone.

Pterotocrinus rugosus, nobis.

The condition of our specimen is such that a particular description cannot be made: the arrangement of the parts, however, is evidently quite similar to that of *P. depressus*. The basals, first, second and third radials, are present, together with parts of the wings and a portion of one of the arms. This species differs remarkably from *P. depressus*, in the greater thickness of the pieces, prominence of the base, the knobby protuberances upon it and upon the first radials, the depth of the columnar pit, as well as by its roughness and more robust appearance.

Geological Position and Locality.—A single crushed and imperfect specimen was found in the lowest siliceous mud bed,* at the Falls of Rough Creek, Breckinridge county, Ky. Fragments of this species are quite abundant.

The beds at Rough Creek, Grayson Springs, Grayson county, and Baker's Furnace, Edmondson county, are doubtless the equivalents of each other. In the western edge of Breckinridge county, they are separated by a thick sandstone, where the upper division of the limestone appears to be the equivalent of the beds above enumerated. The size and proportions of this species is about the same as *P. depressus*.

Zeacrinus ovalis, sp. nobis.

Body—When the arms are closed the body is ovoid, the length being equal to about twice the diameter; concave at the base.

Basal pieces, five; minute, forming a pentagon, slightly indented at the sutures marking the division of the pieces. When the column is present, the basals and about half of the subradials are concealed. *Sub-*

* Equivalent to limestone No. 3, Millstone grit.

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radial pieces, five; four of which are of the same form and size; lanceolate, obtusely pointed at the superior extremity; the fifth is large, irregularly pentagonal; one of its sides supports two of the anal pieces. *First radials*, five; differing considerably in size; the anterior one being symmetrical and the largest, the other four are unsymmetrical, the inferior side toward the anal field being longer than the inferior anterior side; they diminish regularly from the anterior to the posterior side. The *second radials* in all but the anterior ray are pentagonal, twice as wide as long, on the upper beveled sides of each supporting two arms. The second radial of the anterior ray is twice as wide as high, supporting on its upper side a third radial similar in size and form to the second radial of the other four rays, and like them it also supports a pair of arms. The *arms* are ten in number. The antero- and postero-lateral rays are composed of three quadrangular and one pentagonal piece each; one of the antero-lateral rays has the same form and number of pieces as the other and the anterior rays are composed of two quadrangular and one pentagonal piece each, somewhat larger than the pieces of the other rays. The inner upper margin of the last piece in all the rays support each one division of the ray, which is composed of about forty-five quadrangular pieces each, gradually diminishing in size to the end of this division of the ray, which is thus terminated in a point. The outer margins each support a branch which is bifurcated on the fourth, fifth or seventh piece, similar in form and arrangement to the first branch of the rays, the pieces being nearly as high and about half as wide as those below. The inner beveled upper margin of each last piece supports a branch which is of the same form as the inner branch below, composed of quadrangular pieces, terminating at the same height as the other branches. The outer margin of the second bifurcation of the anterior ray has in like manner an undivided branch of the same form and length; all the other rays are again bifurcated on the fifth or sixth piece, each division bearing two branches similar in form to the upper part of the inner divisions below; the anterior ray in its last division consisting of six fingers, all the others having eight fingers each, 46 fingers in all. The *fingers* are ciliated their entire length with a row on their inner margins.

Anal pieces, eight; the two inferior pieces rest partly on the subradials and partly against the first radials on either side, one rising nearly as high as the upper margin of the first radial against which it rests; upon these two rest two of the second range, and on the left side one quite minute, making the upper margin of the field nearly horizontal; the last two support one subquadrangular piece (the largest in the anal field) above which are three of the same form rapidly diminishing in size, thus running the anal field to a point.

Columns delicate pentagonal (near the body), composed of alternately larger and smaller thin pieces.

Dimensions.

Total length, - - - -	1·25 inches.
Greatest diameter, - - -	·62 "
Height of calyx, - - - -	·05 "
Greatest diameter of calyx, - -	·50 "

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Length of finger, first bifurcation,	-	-	·94 inch.
" " second "	-	-	·80 "
" " third "	-	-	·45 "
Depth of columnar pit,	-	-	·05 "

Geographical Position and Locality.—Found in the third limestone of the Millstone grit beds of Grayson, Breckinridge, Edmondson and Todd counties, Kentucky.

It has not been found in any other geological horizon in the Millstone grit beds of Western Kentucky, and may be classed amongst the distinguishing fossils of this particular bed of limestone.

Remarks.—Our species, in the arrangement of the calyx, approaches *Z. Worthenii* (Hall), from which it differs in having three instead of four primary radials; also in the number of pieces in the first branches of the rays. It differs from *Z. magnoliæformis* (Troost) in the arrangement of the calyx, the first radials and subradials being much shorter in our species.

Cyathocrinus dekadactylus, nobis.

Calyx, vasiform, spreading rapidly from the base, comprising all the pieces up to the second radials. The pieces forming it are thick, tumid, with long strong arms. Vault unknown. Column obtusely pentangular, covering nearly one half of basals; articulating surfaces crenulated on their borders.

Basal pieces, five; pentangular, forming together a marked stellate figure; upper facets prolonged into nearly acute angles; under surface slightly excavated.

Sub radials, five; hexagonal or heptagonal, somewhat irregular in size; they alternate with the basals; very tumid and thick.

Radials, five; the first are large, various in their shapes, generally septangular, sometimes broader than high, in other instances as high as broad; they support each two other radials, the first of which are thin, parallelogramic pieces much rounded on their outer surfaces; the last radial pieces are pentagonal, rather acuminate, more than twice as wide as high, and support on their bevelled edges two arms each, making $5 \times 2 = 10$ arms in all.

Arms.—The arms are apparently without any subdivisions throughout their whole length. They come off immediately from the radials; the first two armpieces are anchylosed, the arms becoming free and separate on the second armpieces. They are composed of thick pieces, rounded on their backs, about equal in size, and regularly superimposed upon each other. They are about one half as large as the last radials, and have a shallow sulculus (ambulacral groove,) (?) on their inner surface.

Anal piece, one; trapezoidal in shape, the upper margin reflected, pentangular, the middle portion of the piece intumescent. In two well preserved specimens we cannot trace any pieces superposed upon the anal piece.

At the juncture of any three pieces composing the calyx, there is a depression, which attains its greatest depth along the suture lines of the pieces. These depressions are an easily distinguishable character of this

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species. The surface markings and proboscis (if one existed) are not preserved in any examples before us.

The spaces between the radials supporting the arms are quite extended and present the form of a Doric arch.

Geological Position and Locality.—Sub-Carboniferous beds of Montgomery county, Indiana. Rather rare.

Cyathocrinus hexadactylus, sp. nob.

Body, compressed, crateriform, robust, with long arms; surface covered with minute granulæ, arranged apparently in rows parallel to the margins of the pieces. The column is comparatively small, composed of alternating thick and thin pieces; the articulating surfaces are crenated, the larger and thicker pieces project over the thinner ones, the projecting edges being distinctly rounded.

Basis—Composed of five sublanceolate pieces forming together an irregular pentagonal figure. The columnar pit is small, occupying about one-third of the basis, slightly depressed, with a large central opening.

Subradials—Three of them are nearly equal in size, hexagonal, generally higher than wide; the remaining two are larger than the others, not constant in their shape, being quadrangular, pentangular and hexangular in different individuals. They alternate with the basals, are very convex, from their centres project two obscure folds which proceed to the radial pieces, where they join similar folds, thus enclosing a triangular depression, which is considerably deepened at their junction with any two radial pieces. These plicatures are faintly marked, and can be seen only on specimens when in a good state of conservation.

Radials—The first are large, wider than high, fitting into the retreating angles of the subradial pieces, equal in size; near the middle of the upper surfaces of each, and at the juncture of the two adjoining folds from any two adjoining subradial pieces, come off the remaining radials, which are two in number, small, the last axillary. The second radials fit into a facet which is scooped out of the upper part of the face of the first radials, and are bordered by a small emarginate plicature, slightly raised and reflected outwards.

Anal pieces, two; the first is obscurely pentangular, the fifth facet being hardly perceptible; it is hemmed in entirely by the surrounding pieces, much smaller than they are; the second is irregularly shaped, about the size of the subradials, and bears upon its upper edge an arm.

We have never been able to procure a specimen which had a vault preserved, and can therefore give no description of it. We doubt the existence of a proboscis. The figure* is drawn the size of nature.

Arms.—Following the large first radials are three small, rounded pieces, two quadrangular, the third axillary, giving off two branches, which are long, tapering, composed of a single row of stout pieces; three branches give off at intervals on either side, long filamentous pinnulæ, which extend the full length of the arms. These are composed of small pieces resembling those forming the arms, and are similarly arranged. There are five pairs, rising from the regular rays. From the anal field rises a single, long, tapering arm, not bifurcated as the others are.

* To be given hereafter.

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None of the family of Cyathocrinidæ have yet been described in which the anal field always bore an arm. We supposed at first that this sixth arm was only an anomalous condition of the individual, but having in our possession six or seven specimens, all showing this arrangement, its actuality is placed beyond cavil.

Hall has described (Iowa, vol. ii, p. 625, pl. 18, fig. 7, 8,) *C. spurius*, to which our species is most closely related; it differs in the sixth arm, which alone constitutes it a distinct species. See also *P. Meekianus*, Shumard (Geol. Rep. Missouri, p. 188, pl. A, fig. 7, a, b).

Geological Position and Locality.—Found in sub-carboniferous beds of Clear Creek, Hardin county, Ky., and Montgomery county, Indiana.

Actinocrinus Indianaensis, nobis.

Calyx, subglobular, ornamented with hieroglyphic sculpture. Proboscis long and thin, arms very long, articulations similar in size.

Basal pieces, three; large, extending beyond the supra columnar oint. The margin of the basis projected into a flange.

Radial pieces, 5×3; the first is large, heptagonal, its central portion marked by a double sigmoid carina; its upper facet is angularly notched, into which depression the second radial pieces fit; they are longer than wide, pentagonal, supporting the third radials, which are axillary. These last give off on either facet two radials of the second series, the last of them axillary, and support two rows of three brachials each, the last ones of which are large, tumid, and quite protuberant; here the free arms come off. This is the case except in the antero-lateral ray, where the radials of the second series support only one row of brachials on each beveled edge, thus giving $4\times4+2=18$ pairs of arms.

A prominent carina commencing on the first radial piece extends over the middle of all the pieces, distinctly marking the ramifications of each ray.

Interradials.—Three are interposed between each two rays, except of course, on the anal side, (a large hexagonal one followed by two smaller ones) their middle portions rise into mammiform protuberances.

Anal pieces, seven; hexagonal, of nearly the same size. First one, followed by a row of three, then two; above them a single one, which completes the pyramid. The ornature of these pieces is the same as that of the interradials. We have one specimen showing elaborate hieroglyphic markings; doubtless this was the character of the original ornature of the surface.

Vault—about equal in size to the calyx, composed of many small polygonal pieces, which are intumescent, sometimes prolonged into small sharp thorns. The whole is surmounted by a thin, long proboscis, composed of very minute pieces, and closed at its extremity, where it widens out into a fusiform shape.

Arms.—When in a normal condition there are eighteen pairs of arms, long, dividing into two branches almost immediately after leaving the brachials, each branch running out to the end without further bifurcations. They are formed of a double row of pieces, which at their juncture present a serrated appearance; towards their outer extension they become gradually smaller, until at their end they are quite attenuate and generally curved inward. They are deeply sulcate on their inner surfaces,

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and bear on their lower outer edges long filamentous frimbriæ, which are set closely together, one to each piece forming the arm; these are composed of five or six joints, each having a direction outwards or downwards. *De Koninck and Le Hon* figure (Recherch. Crinoid. foss., Pl. IV, fig. 3,) a bundle of arms which so nearly resemble the arms (with head attached) of one of our specimens, that we cannot but regard them as perfectly identical. They refer them to *Actinocrinus stellaris*, to which species they certainly do not belong. A *stellaris* has 20 arms, (our species 18) arranged in 5 rays of 4 each, with comparatively large interstitial spaces, while the arms figured are thickly crowded together, coming off as they did in almost a closed circle, without any interstices of moment to divide them into separate bundles of arms.

Geological Position and Locality.—Sub-carboniferous beds of Montgomery county, Indiana. It is quite an abundant fossil, and generally found in a good state of preservation.

Actinocrinus Coreyi.

Body, globular; the vault about one third higher than the calyx, tumid; vault surmounted by a large knob.

Basal pieces, three; forming together an irregular hexagon, scooped out, producing a depression occupying about one-half of the superfices.

Radials.—The first are large, irregularly hexagonal, wider than high; their upper surfaces are hollowed, receiving the second radials, which have a corresponding convexity; they are thin but wide, flabelliform, and bearing the third radials, which are flattened pentagons, axillary, and support on each beveled edge three unsymmetrical brachial pieces, five or six times wider than high; thence the free arms.

Interradials, three; the first is very large, generally an elongated octagon; above this are two irregular, thin, high pieces, each having a small depression near the center of their lower surfaces, and interposed between the arms.

Anal pieces, seven; the first is large, having a depression corresponding to that described in the radials; in a row with the first radials, but a little larger than them, hexagonal, its vertical diameter is greatest on its three upper facets, bearing three pieces; they are smaller than the first anal piece; the one on the superior facet is irregularly hexagonal, the lateral ones are long, rather unsymmetrical, and adjoining the neighboring radials and brachials, which nearly enclose them; upon these are three considerably smaller polygonal pieces, which complete the calyx. All of the pieces described above are tumid, without any visible external markings.

This intumesence renders the sutures of the pieces very marked. The arms are ten in number, very strong and thick, coming off in five pairs; we cannot describe them farther, as they are not preserved, and this is the only example we have of this species.

Vault.—The vault is surmounted by a very prominent knob, from which are projected in the directions of the arms, five plicatures, formed of four or five (generally four) massive tumerous pieces; they are much larger than any of the other pieces of the vault, with the exception of the central knob. Between each two of the plicatures is an interradial field, containing from eight to ten polymorphous pieces; they are generally ar-

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ranged in a pyramidal form; often the vertex being a piece considerably larger than any of the others; the basal pieces of the pyramid are in most cases thinner and longer than any of the others; these, as well as the remainder of the pieces of this species, are turgid and massive, and like those of the calyx, are destitute of ornament.

On the side of the vault, above the anal field, is a considerable ovoid intumescence, composed of nearly thirty small pieces, whose surfaces are quite plain and level in contrast to the other pieces of the vault; they are arranged in nearly parallel rows, as follows, commencing with the lowest: 1–3–4–5; on the fifth row (which nearly completes this field—there are but two more rows) supervenes an ovoid opening, about one line in length, without a proboscis. This is the only opening upon the vault.

We have named this elegant species after Mr. O. W. Corey, to whom we are much indebted for many favors.

Dimensions.

Diameter of base, - - - -	·82 inch.
Greatest diameter, - - - -	1·30 "
Height to arms, - - - -	·35 "
Total height of body, - - -	1·10 "
Diameter of axillary articulation, - -	·30 "

Geological Position and Locality.—Rare in sub-carboniferous limestone near the top of the knob stone bed, Hardin and Allen counties, Ky., and same geological horizon, Washington county, Indiana.

GENUS ONYCHOCRINUS, nov. gen. *Lyon & Casseday.*

Generic Formula.

Basal pieces, - - - - -	3
Subradial, - - - - -	5
Radial, - - - - -	5×5
Brachial, - - - - -	4 or 6×5
Interradial, - - - -	20 or 25×4
Anal, - - - - -	5 to 7
Interaxillary, - - - -	1 to 3×5
Arms, - - - - -	5 pairs.

Generic description.—This genus, in the shape of some of the pieces and in its general form, resembles more closely *Forbesiocrinus* than any other; yet they are so widely different in other respects that it will require no great perception to distinguish between them. The column near the calyx is cylindrical, large, composed of very thin articulations similar in size to each other; perforation small.

The radials are large, and form, together with the brachials, a continuous line: the arms are quite robust, furnished with strong pinnulæ.

Interradial fields, triangular in general shape; an anomalous one on the anal side. Anal field—long and narrow: one to three small interaxillary pieces on each ray.

Basal pieces small, subradials large, pentangular, alternating with the radials.

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Onychocrimus exculptus, n. s.

Calyx vasiform, spreading rapidly to the base of the free arms, together with the arms resembles much the talons of a bird, whence its generic name; surface ornamented with minute granules.

Basal pieces, three; their under surfaces concave, forming a saucer-shaped depression, which was wholly filled by the column; they are low, rather thick, upper surfaces prolonged into an obtuse angle; the column facet marked by small short striæ on the outer margin.

Subradials, five; large, four are pentagonal, two of which are larger than the remaining two; their upper articulating surfaces form quite sharp angles; the fifth is hexagonal, its superior surface parallel to the inferior, smaller than the remaining pieces.

Radials, generally five in each ray; the first row are very large, heptagonal, except in the postero-lateral rays, where they are hexagonal, having but one facet on the side next the first anal piece instead of two; their lower surfaces rest on the retreating angles formed by the subradials, with which they alternate; their superior margins are horizontal, on which are imposed the second row of radial pieces; these are smaller than the first, hexagonal, nearly twice as wide as high. The third and fourth rows are similar in form, but become gradually smaller; the fifth rows are heptagonal, axillary, and support on each beveled edge a row of two or three brachials, which are smaller than the radials, obscurely hexagonal, bounded by wave-like lines; near the center of their inferior borders they are prolonged into minute uvulæ, or little tongue-like projections, similar to those found in some species of *Forbesiocrinus*, (and described by Hall as patelloid pieces,) which fit into corresponding depressions in the adjoining pieces.

Upon the last brachial pieces rest two arms; they are as long as the body, robust, composed of exceedingly stout pieces, grooved by a deep sinus, decreasing gradually in size to their outer extension; they are similar in form to the brachials described above; from either side of these arms, alternately disposed, are stout, short pinnulæ, composed of thick pieces stretching in a direction outwards and upwards, without tentaculæ. Commencing with the first two pieces of the arms, they come off in two pairs, then on the next two pieces come off again two pairs, but on the alternate side from the first two; this arrangement is continued throughout their whole length, the pinnulæ becoming smaller and more closely crowded together toward the termination of the arms.

Interradials.—These vary from twenty to twenty-five, according to the age of the individual. The first is large, septagonal, situated between the first two radials of each row, followed by three pieces about half the size of the first; the middle piece of the three is an elongated pentagon, smaller than the other two, which are hexagonal, upon the outer upper facet of each of which, and lying against the radials, are one or two smaller pentagonal pieces; then follow (in the specimen we have before us) fifteen yet smaller pentagonal pieces; they are arranged in the form of a hemispherical arch, depressed towards the disc of the stomach, the sides of the arch being extended up along the radials and brachials as far as the commencement of the free arms.*

* See also under description of Vault.

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Interaxillaries.—In a line with the radial, and between the two opposite arm pieces, is one to three quite small pentagonal interaxillary pieces; sometimes two yet smaller pieces occur.

Anal pieces, one; small, quadrangular; one is superimposed on the subradial, followed by three pieces of like form and size, and similarly disposed.

Vault.—The vault of this remarkable crinoid resembles so much some of the *asteriada* that we may consider it as one of the connecting links between the crinoidea and the star-fishes. The specimens in our possession do not show distinctly the whole of the vault, so that a description must necessarily be imperfect until better examples are found. The plates which we have described above as interradials, form the greatest portion of the perimeter of this upper surface; the remainder of the perimeter is formed of a row of pieces lying on the anal side; their relative position is as follows: viewing it from the anal side, in the position in which the animal grew, we see a row of small pieces (interradials?) which extend up along the radials and brachials of the left ray, fitting into the serrated depressions existing at the junctures of any two pieces. This is found only on the left ray, the pieces forming the right being squarely truncated and without other pieces attached in any way to them.

The general surface of the vault is depressed, the edges being raised and curved inwards. From the centre, and in the direction of the arms, extend five rays, composed of two rows of large granular pieces, one row alternating with the other. We cannot discover pores in any of these pieces, which are most probably analagous to the *ambulacra* of the *asteriada*.

The interstitial pieces lying in the fields bounded by the five rays of larger ones are very small, granulose, and arranged without any apparent order.

It is impossible, from the fragmentary portions in our cabinets, to trace the farther similarity between this genus and the star-fish, as the central portions of all are so concealed that we cannot make them out clearly.

Geological Position and Locality.—Found at Clear Creek, Hardin county, Ky., in sub-carboniferous rocks near the upper part of the sandy mud-beds of the knobstone. In beds of similar age in Montgomery county, Ind. Good specimens are rare.

Louisville, Ky., Nov. 1, 1859.

Art. X.—*Theoretical Determination of the Dimensions of Donati's Comet;* by Prof. W. A. Norton.

I have recently undertaken to bring the theory developed in a previous number of this Journal,* in an article entitled "Dynamical Condition of the Head of a Comet," to the test of numerical computations, by determining, by calculation, the theoretical dimensions of the great Comet of 1858. The more important results may be briefly stated; the complete discussion is reserved for a subsequent number of the Journal.

* Vol. xxvii, [2], 86.

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It appears from the investigation that, confining our attention to the outer bright envelope, the process of ejection of nebulous matter from the nucleus was mostly confined to a certain portion of its illuminated side lying nearest to the sun, and that the limiting angle of inclination of the jets to the line connecting the centre of the nucleus with the sun, was 25°. From this circumstance it resulted that the envelope had nearly a circular form. The lateral dispersion of these luminous jets, as they were flowing away into space, under the influence of the sun's repulsion, or, in other words, the breadth of the tail, was partially due to the directions, more or less inclined to the radius vector, in which they originally issued from the nucleus; but another and highly efficient cause coöperated to produce that result. If, as we conceive, certain portions of the cometic matter, at the surface of the nucleus, were brought by some action of the sun into the condition to be repelled by both the nucleus and sun, we may make two suppositions with regard to the forces of repulsion thus developed; that the force exerted by either body was of the same intensity for all the particles acted upon, or that it varied from one particle to another. It is not easy to decide, upon *à priori* grounds, which of the two suppositions is the most probable. The latter is certainly no less so than the former. If we adopt this as a fundamental hypothesis, we have an efficient cause in operation adequate, in connection with that already mentioned, to the development of the tail of the comet, in all its vast proportions; and which may incidentally have produced the special phenomena observed,—as the supernumerary tails, and the alternate bright and dark bands seen to traverse a certain portion of the principal tail. The ejected particles that are unequally repelled, by both the nucleus and sun, do not part company in consequence, while they are in the vicinity of the nucleus, nor materially while within the limits of the envelope, for the reason that the ratio of the repulsive forces of the two bodies remains constantly the same; but as soon as they pass out of the sphere of influence of the nucleus they are analyzed by the solar repulsion, and driven off by it into space, in separate and diverging paths. The various susceptibilities to repulsion possessed by the particles have accordingly no sensible effect upon the dimensions or form of the envelope, but may give rise to a wide lateral dispersion of the flowing streams that make up the tail of the comet. The particles that are most energetically repelled go to make up the preceding or convex side of the tail.

I have made the calculations, regarding the solar repulsion as varying between certain prescribed limits. The determinations of the breadth of the tail, at various points of its length, accord with the results of observation; at the same time that the tail is found to have the form and positions actually observed.

The supernumerary tails observed were but lines of receding

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particles subject to much greater forces of repulsion than the other particles ejected from the nucleus. All such collections of matter would, of necessity, be in advance of the principal tail, and lie in a curve that would approach more nearly to a straight line. Their position makes known the intensities of the repulsive forces to which they owe their separate existence.

An interesting result of the investigation is that the alternate bright and dark bands so distinctly seen to traverse a certain portion of the tail of Donati's comet, in nearly parallel directions, on the evening of Oct. 10th, had each the position of a line connecting particles which started from the region of the nucleus at a certain previous date, and at the same instant of time. They accordingly find their natural explanation in corresponding alternations in the quantity of nebulous matter given off simultaneously from the nucleus. The most probable cause for such alternations of discharge that can be conjectured is that the nucleus turns about an axis, and so presented periodically different sides to the sun, which were unequally influenced by his inciting action. If this be the true explanation of the phenomenon, we have in the observed distance between contiguous bright bands, the means of determining the period of rotation; or, at least, the shortest interval of time in which the rotation can be completed. If we take this distance at 1°, the period of rotation comes out about 24 hours.

There was a special cause of lateral dispersion at work in the case of the cometary particles that, on their return, came very near the nucleus. Such streams of particles must have been repelled off obliquely, and may very well have presented the appearance of luminous jets issuing from the sides of the nucleus, and have formed curved terminations to the inner envelope. From the dispersion thus produced resulted an absence of matter, or a dark space, behind the nucleus, whose varying boundary was determined by the intersections of lines of particles unequally repelled by the sun.

The indications are, that the formation and gradual expansion of one envelope after another, may have arisen from the process of ejection beginning in all instances high up in the photosphere surrounding the nucleus, and gradually extending downward to the vicinity of the solid surface. It appears, upon investigation, that if this descending action were to proceed according to a certain uniform law, the outline of the envelope would recede from the nucleus at a uniform rate. This process of evolution of cometary matter, in whatever it may consist, is probably auroral in its origin and character, and has its counterpart on both the earth and sun.*

New Haven, Nov. 30, 1859.

* To render the investigation more complete, I have considered the case of the cometary matter being projected from the nucleus, without experiencing any repulsion from its mass.

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Art. XI.—*Geographical Notices.* No. X.

The Mountains of Western North America.—Having previously spoken of the publication of Warren's memoir to accompany the map of the western territory of the United States, and also of the map to which it refers, we now copy the following paragraphs from the conclusion of the Memoir, in order to meet the erroneous opinions which are prevalent in respect to the "Rocky Mountains," and the erroneous presentation of their direction which is given in most of the popular maps.

"The mountains in our territory west of the Mississippi river, from where they rise above the horizontal strata of recent geological formations on the east to their disappearance under the waters of the Pacific Ocean, form a nearly continuous mass of upheaved ridges, with occasional intervening level plateaus. The direction of the central line of this mass between the 32d and 49th parallels of north latitude, is about north 20° west. The greatest width perpendicular to this direction is along the line passing from the vicinity of San Francisco through that of the Great Salt Lake to Fort Laramie. This distance is about 1,000 miles, or, if we include the Black Hills of Nebraska, 1,125 miles.

"The great mountain mass, of which that in our territory forms but a part, extends with varying breadth nearly on the line of a great circle of the globe from Cape Horn north to Behring Straits, and thence south along the western part of Asia to the island of Sumatra. Its length is about 240 degrees, or 18,560 miles, being two-thirds of the circumference of the earth.

"The area occupied by and included in this mountain mass in our territory, is about 980,000 square miles. Large as this is, it is probably only a small portion of the upheaved formations between the 32d and 49th parallels. A few ridges and peaks projecting above the surface of the Pacific as islands, or above the level tertiary and cretaceous strata of the eastern plains, give evidence of the existence of vast areas whose extent must forever remain unknown. Throughout the portions now visible, proofs are abundant of great abrasions; in some cases whole ridges even, having been swept away or broken into separate portions.

"Already enough has been learned to establish the existence in these mountains of the equivalents of many of the geological formations; and it is probable, when investigations have been carried to the same extent as in the civilized portions of the earth, that the geologist will find here new and still more complex fields for research.

"The classification of the separate parts of this mountain mass, so as to present its physical characteristics clearly to the

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mind, is a great desideratum. It has in part been attempted at various times, but as yet unsuccessfully from the want of sufficient information; the theorist's idea being often proved to be wrong by new discoveries almost as soon as uttered." * * *

"The publication of the Pacific Railroad maps will probably change some of the former ideas of these mountains, and give rise to new speculations as to their directions, equivalents, and connexions of different parts. Every one knows how easy it is to generalize ideas where facts are few, and in accordance with this, those who have travelled most in the region have theorized the least, having seen the immensity of the subject and the difficulties which must be overcome to comprehend it. Those who have investigated merely the travels of others, have had only the imperfect representations of the latter on which to theorize.

"It may not be inappropriate here to give some of the general ideas which have successively prevailed in regard to these mountains.

"In the earlier periods of North American discovery it was known that there were mountains in the interior at its northern and southern parts, and rivers flowing from them to the two great oceans east and west. It was natural to connect these mountains by hypothesis, and to consider them as one great chain, separating the sources of these streams. Such an idea prevailed at the time of Humboldt's New Spain. Even now many well informed persons consider that a road has but one mountain summit to cross from the Mississippi river to the Pacific Ocean.

"When, after the publication of the charts of Vancouver, map makers became aware of the extent of the mountains near the Pacific coast, nothing seemed more natural than to suppose two great mountain chains—one near the Pacific and one in the interior. If this theory were true, we should find a great longitudinal valley between the ranges similar to that separating the interior mountains from the Alleghanies, and we should have but two mountain summits to pass between the Mississippi and the Pacific. This idea is practically as erroneous as that of one summit, although it still prevails. Such a prominent place did this longitudinal valley hold, in the opinions of geographers of earlier times, that we find in Humboldt's New Spain: 'M. Malte Brun has started important doubts concerning the identity of the Tacouche Tesse and the river Columbia. He even presumes that the former discharges itself into the Gulf of California: a bold supposition, which would give the Tacouche Tesse a course of an enormous length. It must be allowed that all that part of the west of North America is still but very imperfectly known.'

"The explorations of Lewis and Clarke proved that the Tacouche Teche did not empty into the Gulf of California, and

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that it was probably the source of the Columbia. Without considering the character of the pass of the Columbia river through the Cascade range, the belief now became general that the overland route in this latitude crossed but one summit, and was therefore more favorable than any other. This erroneous idea with some still prevails.

"The idea of rivers traversing great mountain chains, now known to be so common in the mountains west of the Mississippi, was so repugnant to the opinions of even philosophers in earlier times, that we find Humboldt saying, 'every geographer who carefully compares Mackenzie's map with Vancouver's will be *astonished* that the Columbia, in descending from the Stony mountains, which we cannot help considering as a prolongation of the Andes of Mexico, should traverse the chains of mountains which approach the shore of the great ocean, whose principal summits are Mount St. Helen and Mount Rainier.'" * *

"The distinguished explorers, Lewis and Clarke, having determined that the Columbia river broke through the Cascade range, considered from the size of the Willamette at its mouth, that it also broke through this chain, having its source in the Rocky mountains, near the position of the Great Salt lake. We then see the American maps representing mountains surrounding the valleys of the Columbia and Colorado, and separating them from that of the San Joaquin and Sacramento. On the English maps of that date, the Sierra Nevada is not represented, and two or three great rivers are made to flow from large lakes in the interior to the Pacific; nearly all of their compilers making false applications of the principles of hydrography laid down by Humboldt.

"The first map which represented these rivers and lakes correctly was that of Captain Bonneville, of which I have given a reduced copy. There we see the Great Salt lake and Bear river and Utah lake forming one basin; to the west lies the Mary or Ogden's river, with its lakes forming another enclosed basin; the San Joaquin and Sacramento rivers in their right position; and the Willamette reduced to its proper length. The positions given on this map are not geographically correct, nor are their many mountains indicated; but it gives the first correct idea of the hydrographic character of the country; and by giving too little rather than too much, escapes the errors into which others had fallen.

"The explorations of Captain Frémont fixed these great rivers and basins in their proper geographical positions; but his maps have given rise to many erroneous impressions in regard to the mountain ranges. Still, making a 'false application of the principles of hydrography,' he represented all the basins as if surrounded with mountains or 'rims,' and thus introduced mountain chains which have no existence in nature.

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"Since Frémont's expedition began, a large portion of the area of these mountains in the territory of the United States has been examined, and many new attempts have been made to systematize the knowledge acquired. The most important theory advanced is that of parallelism in the ranges, the foundation of which I shall briefly indicate.

"On the map of Lewis and Clarke the Rocky mountain ranges are represented parallel to each other with a northwest trend. That this was their theory is evident, from the fact that they indicated the Black Hills about the source of the Shyenne as having this same trend, though they never saw them, and only knew of their existence from hearsay.

"The maps of Captain Frémont showed a parallelism and general north and south direction of the mountain ranges from the Wasatch, east of the Salt Lake, to the Sierra Nevada, including all the numerous intermediate ranges.

"The maps made by Major Emory, near the 32d parallel, and in New Mexico, showed again a remarkable parallelism of the mountain ridges, those in this latitude having a northwest trend nearly parallel to the Rocky mountains, as shown by Lewis and Clarke.

"The maps of Lieutenants Abert and Peck, of Lieutenant Simpson, of Lieutenant Beckwith, Lieutenant Williamson, and Lieutenant Parke, have all shown a local parallelism to exist in different parts of the mountains. The systems of ridges have courses varying from a few degrees north of east to north 45° west.

"The idea has lately begun to prevail that this local parallelism is the characteristic of the great mountain mass throughout its whole extent. Whether this idea has been true or not it has been attended with some practical advantages. Instead of one or two main summits for an overland road to pass, it shows us that we must expect many. On every route explored across the continent, at least four well-defined summits have been discovered, and on some of them many more. Some of these ridges enclose interior hydrographic basins. Others are traversed by rivers, but the passes thus made are generally impracticable, and, for the purposes of travel, might almost as well never have existed.

"In many places, however, the mountain ridges have not this local parallelism, of which a few instances will be cited. The Uintah mountains, east of the Great Salt Lake, trend nearly east and west; the Wind River mountains about north 45° west; and the Humboldt range about north 20° east; these three ranges being comparatively near to each other.

"Humboldt, in speaking of the Sierra Nevada, says, 'it soon separates into three branches.'

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"Lieutenant Abbot, in the sixth volume of the Pacific Railroad Reports, says: 'Shasta Butte, although generally considered a peak of the western chain of the Sierra Nevada, is, in truth, the great centre from which radiate, beside several smaller ridges, the Cascade range, the Coast range, and the western chain of the Sierra Nevada.'

"There are many other portions of this mountain region from which the ridges seem to radiate. Such as Long's Peak, the junction of the Sierra Nevada, and Coast ranges in Southern California, &c., as is evident on an inspection of the map. The parallel system of ridges has been considered a matter of importance, as being in accordance with some supposed laws of mountain formation, but that of centres of upheaval are not less consistent with those laws. At any rate it does not appear that we are at liberty to assume a parallelism of ridges till examination has shown this to be the case.

THE SOURCES OF THE NILE.—At a recent meeting of the Royal Geographical Society of London (May 9), Captain Speke gave the following narrative of his journey with Capt. Burton into the interior of Africa. His remarks were called out by a paper of Mr. Macqueen's, the object of which had been to prove that the mountain Kilimandjaro is actually snow covered but that it has no connection with the River Nile.

Capt. Speke's remarks are thus reported in the Proceedings of the Society, vol. iii, No. 4:

"After arriving at Zanzibar, we had to wait a considerable time, some months, until the masika, or rainy season, would be over, before we could penetrate into the interior. It was generally advised that we should do so. During the interim Captain Burton and myself made a short coast tour, first to Mombas, and then proceeded down the coast to Pangani. Leaving that place, we ascended the Pangani river, and arrived at Chongwe, a small military station belonging to Prince Majid. Here we were supplied with a small escort of Belooch soldiers, who accompanied us across some hills, by an upper route, to Fuga, the capital of Usambara, where we were hospitably entertained by King Kimwere, a great despot reigning there. After visiting him for one day, the shortness of our supplies compelled us to retrace our steps by a forced and rapid march, following down close along the banks of the same river until we again arrived at Pangani. Thus ended our initiatory tour in Eastern Africa. The rainy season or masika was spent by us at Zanzibar, in constructing the equipment of a caravan. There is a singular tribe of negroes in the interior of Africa, called Wanyamuezi—the literal translation of which signifies people of the moon. These strange people are professionally voluntary porters: they annually bring down ivory to the coast in barter for themselves, or otherwise

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for the Arabs. At the close of the rainy season Captain Burton and myself left Zanzibar, with a caravan mustering about eighty men, having previously sent on some supplies in anticipation of our arrival. Unable to collect a sufficient caravan for the conveyance of our kit, we purchased a number of donkeys (about thirty). Thus completed, and with an escort of twelve Belooch soldiers, given us by Prince Majid, we commenced our journey westward, and arrived (by slow degrees travelling over a low alluvial plain, up the course of the Kingani river) at Zungomero, a village situated under the coast range, which struck us as bearing a good comparison with the western ghauts of India. We might call this range the Eastern Ghauts of Africa. There we were detained by severe illness a considerable time. Afterwards we crossed these eastern ghauts, the maximum altitude of which I ascertained to be about 6000 feet. On the western side of this longitudinal chain of hills we alighted on an elevated plateau, an almost dead flat, ranging in level from 3000 to 4000 feet above the sea. Here we had cold easterly winds, continuing through the entire year. Proceeding onwards, we arrived at the Tanganyika Lake, called by the Arabs Sea Ujiji, a local name taken from the country on the eastern margin of the lake, whither they go to traffic for ivory and slaves. This lake is in a singular synclinal depression; I found its elevation to be only 1800 feet; whereas the surrounding country (the plateau), as I said before, averaged from 3000 to 4000 feet. The lake is encircled at its northern extremity by a half-moon shaped range of hills, the height of which I estimated (for I could not reach its summit) to be at least 6000 feet. They may extend to a height much greater than that; however, we could not take any observations for determining it. After exploring this lake we returned by the former route to Unyanyembe, an Arab depôt, situated in latitude 5° south, and about 33° east longitude. My companion, Captain Burton, unable to proceed farther, remained here; whilst I, taking just sufficient provisions for a period of six weeks, made a rapid march due north, to latitude 2° 30′ south; and there discovered the southern extremity of the Nyanza, a lake, called by the Arabs Ukerewe, a local name for an island on it, to which the merchants go in quest of ivory. The altitude of this lake is equal to the general plateau (4000 feet), even more than the average height of all the plateau land we traversed. In reverting to the question asked, why I consider the Lake Nyanza to be the great reservoir to the Nile, my answer is this: I find, by observation, that its southern extremity lies in east longitude 33°, and south latitude 2° 30′. By Arab information, in which I place implicit confidence, I have heard that the waters extend thence, in a northerly direction, certainly from five to six degrees. Notwithstanding they can account for

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a continuous line of water to this extent, no one ever heard of any limit or boundary to the northern end of the lake. A respectable Sowahili merchant assured me that, when engaged in traffic some years previously to the northward of the line and the westward of this lake, he had heard it commonly reported that large vessels frequented the northern extremity of these waters, in which the officers engaged in navigating them used sextants and kept a log, precisely similar to what is found in vessels on the ocean. Query, Could this be in allusion to the expedition sent by Mahamad Ali up the Nile in former years? Concerning the rains which flood the Nile, the argument is simple, as I have said before: a group of mountains overhang the northern bed of the Tanganyika Lake. The Arabs assure us that from the north and northeastern slopes of these hills during the rainy season immense volumes of water pour down in a northeasterly direction, traversing a flat marshy land, intersected by some very large, and many (they say 180) smaller streams. Again, on the western side, we hear from Dr. Krapf, that the snow-clad mountain, Kœnia, is drained by rivers on its western slopes in a direction tending to my lake.

"During the rainy season, which I know, by inspection, commences in that region on the 15th of November, and ends on the 15th of May, the down-pour is pretty continuous. Supersaturation, I should imagine, takes place later on the northern than on the southern side of the aforesaid moon-shaped mountain, systematically in accordance to the ratio of seasonal progression; but this, in so mean a distance, could not be very great. Suffice it to say, that I saw the Malagarazi river, which emanates from near the axis of these hills, to be in a highly flooded state on the 5th of June. The Nile at Cairo regularly swells on the 18th of June.

"Farther, it would be highly erroneous to suppose that the Nile could have any great fluctuations from any other source than periodical rains. Were the Nile supplied by snow, as some theorists suppose, its perennial volume would ever be the same. There would be no material fluctuations observable in it, in consequence of its constant and near approximation to the path of the sun.

"By these discoveries, the old and erroneous hypothesis of a high latitudinal range of mountains extending across the continent of Africa from east to west, in the vicinity of the line, and known as the Mountains of the Moon, is therefore now annihilated. However, it is worthy of remark, that the crescent-shaped mountain, which we visited to the northward of the Tanganyika, lies in the centre of the continent of Africa, immediately due west of the snowy peaks Kilimanjaro and Kœnia, and is west beyond the Unyamuezi, or Country of the Moon.

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The Wanyamuezi tribe has from time immemorial been addicted to journeying, and at all periods has constantly visited the eastern coast of Africa. It would not be beyond legitimate and logical supposition, to imagine that these hills, lying beyond their Moon Country, should have given rise to the term Mountains of the Moon, and from misunderstanding their relative position with the snowy Kœnia and Kilimanjaro, should have been the means of misguiding all ancient inquirers about that mysterious mountain.

"My positions were fixed by astronomical observations, certainly under painful and considerable difficulties, owing to my constantly impaired general state of health: weakness and blindness not being the least of these difficulties which I had to contend with. My latitudes were taken by the altitude of stars, at nearly every stage, on an average from ten to fifteen miles apart. I also fixed some crucial stations, the principal points for delineating the country by lunars, on which I place great reliance, as the means of the masses of them which I took show so little deviation. The intermediate distances I compassed very closely; the altitudes of the country I traversed I determined by boiling thermometer, on which I also place very great reliance. We had a thermometer and pedometer, and several chronometers. The performance of these instruments was anything but satisfactory; indeed, finally, I had to rig up a string and bullet pendulum to beat time whilst taking my lunars in the latter stage of the journey. There now can be no doubt that this great lake, the Nyanza (Captain Speke now pointing to the map) is the great reservoir of the Nile, and that its waters indubitably extend northwards from the position visited by me on its southern extremity to $3\frac{1}{2}°$ north latitude, lying across the equator, and washes out that supposed line of mountains, called the Mountains of the Moon, which stands so conspicuously in all our atlases."

Baikie's Niger Expedition.—The latest intelligence we have received from this important expedition is contained in the following extract from Sir R. I. Murchison's late anniversary address before the Roy. Geog. Soc. of London:

"The unfortunate shipwreck of the *Pleiad* on the rocks near Rabba, and the check given to the expedition under Dr. W. B. Baikie, which left England early in 1857, were alluded to in my last year's address. I now learn from Mr. D. T. May, R.N., who has returned to England, that less than twenty miles above Rabba the River Niger, or Quorra, divides into several rocky, intricate channels. Consul Beecroft in the *Ethiope*, in 1845, safely navigated the most available of these passages; but the voyagers of 1857 were not so fortunate, and the steamer was totally lost on the rocks. Most of the property was, however,

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saved, and the neighboring bank became the head-quarters of the expedition for a whole year. The rocks forming the banks of the river where the shipwreck took place are composed of highly-inclined strata of hard sandstone. All the specimens of this rock which I have examined, whether brought home by Mr. May or sent by the Admiralty, belong to the same light-colored, hard, sub-crystalline, pinkish sandstone, with very fine flakes of white mica; the successive layers (which are much foliated) being strikingly covered by thin elongated crystals of black tourmaline. The rock has altogether the appearance of having undergone considerable metamorphosis, and much elevation and disturbance. Geodes of pure white quartz, with large micaceous coatings, also occur. As soon as the party had become somewhat settled, it was determined to make a direct overland communication by Yóruba with Lagos, and Mr. May offering himself for this service, accomplished it satisfactorily, as explained in a notice laid before the Society. In the mean time Lieut. Glover made journeys up the river, visiting Wawa and Busa, and definitely ascertained the impracticability of navigating the river for a few miles beyond the spot of the encampment, a waterfall at Waru being an impassable barrier even for canoes in any season.

"Mr. May having waited on the sea-coast, expecting another steamer from England, at last returned to the encampment through Yóruba, and then set out on a more extended journey, with a view to exploring the country, and of establishing postal communication in a line from Lagos to the confluence. Having first travelled to Hadan (the road between Lagos and Hadan being well known and used), he passed eastward, and journeyed for many weeks through the previously unvisited districts of Ife, Ijesha, Igbouma, Yagha, &c., being warmly received, and observing everywhere that the people were quiet, orderly, and industrious; though these good qualities are here and there broken in upon by marauding or slave-catching armies, sent into the Yóruban country by powerful neighbors. The details of this journey were communicated to the Foreign Office in January last, and will, I presume, soon reach the Society.

"Approaching to within fifty or sixty miles of the confluences of the Quorra and Chadda Rivers, Mr. May was compelled to alter his route, and proceed northwards, visiting the ruined famous town Ladi, crossing the Quorra at Shaw, and journeying thence on the north side of the river through Núpe to Rabba.

"Lieutenant Glover had during this time also visited the coast by Mr. May's first route, and was now there waiting to pilot up the river the steamer which was at last coming to the relief of the party. Dr. Baikie and the other members of the expedition had been chiefly employed during the year in culti-

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vating a good understanding with their neighbors, reducing their language, &c., whilst the energies of Mr. Barton were amply occupied on the botany of this part of Africa. In October, 1858, just a twelvemonth after the settlement of the expedition at the spot in question, the *Sunbeam* steamer arrived, the whole party were then embarked, and proceeded down the river to Fernando Po, there to recruit the health of the officers and men, and make arrangements for farther exploration. During the twelvemonth's residence in Núpe the most friendly relations were maintained with the king, his brother, and chiefs, and the natives generally; supplies being often received overland from Lagos.

"At Fernando Po (November, 1858), a re-organization having taken place, and the preparations being completed, the party again set out, now in the steamer *Rainbow*, built and sent for the purpose, and endeavored to re-ascend the river. But it was then found that this vessel, which draws four feet of water, could not ascend the Niger even in the month of January; the waters subsiding until June, when they increase. In consequence, the party was obliged again to return to the sea, and since have set out upon the land-journey from Lagos to Rabba (upon the route opened up by Mr. May), whence it is purposed to proceed with an expedition, the friendly objects of which must by this time have made a due impression on the native chiefs, and from which we may anticipate the gain of much knowledge when all the acquisitions of Dr. Baikie and his associates are unfolded."

KHANIKOFF'S EXPEDITION IN CENTRAL ASIA.—At a recent meeting of the American Oriental Society in New York, the Corresponding Secretary, Prof. Wm. D. Whitney of Yale College, presented a letter from the Chev. N. Khanikoff, dated Kerman, Persia, April 7–19th, 1859, in which he speaks as follows of the journey which he has just made.

"I have just completed, or nearly so, a very interesting journey through Khorassan, Western Afghanistan, and Northern Seistan. The whole region traversed by the scientific expedition which I have had the honor to conduct has been carefully surveyed, the situation of its principal points has been fixed astronomically, for more than thirty points ascertainment has been made of the magnetic coördinates, and of the intensity of magnetism corresponding therewith, and the profile of the territory has been determined by barometrical observations and trigonometrical measurements. The botanical researches have been made by Prof. Bunge, and the geological investigations by M. Göbel; the oriental literature, archæology, and numismatics have fallen to my share, and I hope soon to have the pleasure of communicating to the Society a succinct view of the results at which I have arrived." D. C. G.

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Art. XII.—*The Great Auroral Exhibition of August 28th to September 4th*, 1859.—(2d Article.)

In our last Number* we gave some observations of this grand auroral exhibition, from a number of stations widely distant from each other. We now put on record some facts observed respecting the influence of the Aurora upon the wires of the electric telegraph. We hope in our next Number to be able to communicate additional intelligence respecting this Aurora.

1. *Observations made at Boston, Mass., and its vicinity by* George B. Prescott, *Telegraph Superintendent.*

My attention was first called to the possibility of the Aurora Borealis affecting the telegraph wires in 1847, while operating the Morse (electro-magnetic) telegraph at New Haven; but I was not fortunate enough to observe it until the winter of 1850. At this time I became connected with Bain's (electro-chemical) telegraph in this city, and observed some effects of the aurora; but, owing to the feeble displays, only to a limited extent.

In September, 1851, there was a remarkable display of the Aurora Borealis, which completely took possession of all the telegraph lines running out of the city, and effectually prevented any business being done over them during its continuance.

The following winter there was another remarkable display, which occurred upon the 19th of February, 1852. I furnish from data recorded in my journal at that time the following particulars in regard to this phenomenon.

The system of telegraphing used upon the wires during the observations of February, 1852, was Bain's electro-chemical. The circuit was what is known as the open circuit,—that is, the key, which throws the current from the battery upon the line, was always open when a message was being received from a distant station, and the current passed through the chemically prepared paper to the earth without uniting with the home battery. Each station was furnished with its own battery, the negtive pole of which was invariably connected with the earth, and the positive, by the depression of the key, with the line.

The line extended in a direction nearly northeast and southwest. The paper was prepared with a solution of cyanid of potassium, made after the following recipe. Six parts prussiate potassa dissolved in water; two parts nitric acid; two of ammonia. This solution will scarcely color the paper, while it will render it quite sensitive to the action of the electric current. The stylus was made of No. 30 iron wire. A battery of ten

* Vol. xxviii, p. 385–408.

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cups Grove, with the line well insulated, will decompose the salts, and uniting with the iron stylus, leave a bright blue mark upon the paper, at a distance of 230 miles.

The positive pole only produces a colored mark; the negative bleaches the paper.

When there is no electric current upon the wires, the pen leaves no impression upon the paper; but the slightest current will produce decomposition; and the color of the mark depends upon the strength of the current.

Free or common electricity produces no color upon the paper. It emits a bright spark in passing from the stylus to the moistened paper; produces a quick, sharp noise, like the snapping of a pistol and disappears. This effect is totally unlike that of the Aurora Borealis, as will be seen from the following.

Thursday, February 19, 1852. Towards evening a blue line appeared upon the paper, which gradually grew darker and larger, until a *flame of fire* followed the pen, and burned through a dozen thicknesses of the prepared paper. The paper was set on fire by the flame, and produced considerable smoke. The current then subsided as gradually as it came on, until it entirely disappeared, and was then succeeded by a negative current, which bleached instead of colored, the paper; this also gradually increased, until, as with the positive current, it burned the paper, and then subsided, to be followed by the positive current again. This state of things continued during the entire evening, and effectually prevented any business being done over the wires. The current came in waves of varying intensity—light at first, then stronger, until having attained to the volume and intensity of at least two hundred Grove cups, it subsided, and was followed by a current of the reverse polarity. This *invariably occurred*, and may be set down as an established fact, that the currents from the Aurora Borealis always change their polarity during every wave.

I have seen the auroral current produce magnetism, decompose chemicals, and produce heat and fire.

The effects of the magnetic storm of August 28th, 1859, were apparent upon the wires during a considerable portion of Saturday evening, and during the entire day, Sunday. At 6 P. M. the line to New Bedford (60 miles in length, running a little west of south) could be worked only at intervals, although, of course, no signs of the Aurora Borealis were visible to the eye at that hour. The same was true of the wires running east through the state of Maine as well as those running north to Montreal. The wire between Boston and Fall River had no battery connected with it on Sunday, and yet there was a current upon it during the entire day, which caused the keepers of the electro-magnets to open and close as the waves came on and receded.

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Upon the lines which had batteries connected Sunday evening, it was observed that the poles changed during every wave of the aurora—each wave occupying from fifteen seconds to half a minute. When the poles of the Aurora were in unison with the poles of the battery upon the line, the effect was to increase the current; but when they were opposed, to neutralize it. I will give my proofs of this farther on. It is to be observed that the effects I have illustrated in relation to the Aurora of August 28th, 1859, were observed upon the Morse (electro-magnetic) system. The same were, however, observed upon the House and Hughes lines running out of the same office.

It is not true that there is any difference in the effect produced upon the wires by the Aurora Borealis, whether they run east and west, or north and south. Lines running to every point of the compass diverge from the office here and were equally affected. Even the short wire running from the office in State street to the observatory in Cambridge (five miles long) was sensibly affected.

In an article which I published in the Boston Journal, August 31st, I stated that the current from the Aurora Borealis could have been used for telegraphic purposes, but I did not imagine it would be so soon verified by the actual fact.

On Friday, September 2d, 1859, upon commencing business at 8 o'clock A. M. it was found that all the wires running out of the office were so strongly affected by the auroral current as to prevent any business being done, except with great difficulty. At this juncture it was suggested that the batteries should be cut off, and the wires simply connected with the earth. The Boston operator accordingly asked the Portland operator to cut off his battery and try to work with the auroral current alone. The Portland operator replied, "I have done so. Will you do the same?" Boston operator answered, "I have cut my battery off and connected the line with the earth. We are working with the current from the Aurora Borealis alone. How do you receive my writing?" "Very well indeed," rejoined the Portland operator; "much better than with the batteries on. There is much less variation in the current, and the magnets work steadier. Suppose we continue to work so until the Aurora subsides?" "Agreed," said the Boston operator. "Are you ready for business?" "Yes; go ahead," was the reponse. The Boston operator then commenced sending private dispatches, which he was able to do much better than when the batteries were on, and continued to use the wire in this manner for about two hours, when, the Aurora having subsided, the batteries were resumed.

While this singular phenomenon was taking place upon the wires between Boston and Portland, the operator at South Brain-

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tree—Miss Sarah B. Allen—informed me that she was working the wire between that station and Fall River—a distance of about forty miles south—with the auroral current alone. Since then I have visited Fall River and have the statement verified by the intelligent operator upon the railroad line at the dépôt in that village.

The office at the dépôt is about half a mile from the regular office in the village. The battery is kept at the latter place, but the operator at the dépôt is provided with a button or switch, by which he can throw the battery off the line, and put the wire in connection with the ground at pleasure. The battery at the other terminus of the line is at Boston, but the operator at South Braintree is furnished with a similar switch, which enables her to dispense with its use at pleasure. There are no intermediate batteries; consequently if Fall River operator puts his wire to the earth, and the South Braintree operator puts her wire to the earth, the line is without battery, and of course, without an electrical current.

Such was the state of the line upon the 2d of September last, when for nearly two hours, they held communication over the wire with the aid of the celestial batteries alone!

I have restricted myself in this article to facts observed by myself. I have stated nothing which I am not absolutely certain of, and which, if necessary, can be proved by a number of reliable witnesses.

2. *Observations made at White River Junction, Vt., communicated by* J. H. Norris, *Telegraph Superintendent.*

During the forenoon of Sept. 2d, an unusual current of varying intensity was present most of the time on the wires of the Vermont and Boston telegraph. The polarity of this current appeared to change frequently, sometimes being opposite to and nearly or quite neutralizing the battery current when an attempt was made to use the line; at other times much increasing the force of the battery current. The auroral current produced the same marks upon our chemical paper (we use the Bain or chemical system of telegraph) as those produced by the use of the battery. Signals and messages were transmitted between Boston and Manchester *by the sole use of the auroral current.*

3. *Observations made at Springfield, Mass., by* J. E. Selden.

On the evening of Aug. 28th, upon the Boston and New York circuit, at one moment there was a very heavy current on the wire, and the next none at all. On the Albany and Springfield circuit, *a flash* passed across from the break key of the telegraph apparatus to the iron frame, the flame of which was about half the size of an ordinary jet of gas. It was accompanied by

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a humming sound similar to a heavy current passing between two metal points almost in contact. The heat was sufficient to cause the smell of scorched wood and paint to be plainly perceptible.

[The observations at Springfield, as well as those at New York and Washington were communicated by Messrs. Lewis and Lovett of the New York telegraph office.]

4. *Observations made at New York by* J. C. Crosson, *Telegraph operator.*

On the evening of Aug. 28th, at 7½ o'clock, I experienced considerable difficulty in working, on account of the variation of current. I could work south by constantly altering the adjustment of my magnets, but the magnetism on the eastern circuit was so nearly destroyed that I could do nothing. About ten o'clock I could see nothing of the Aurora in the southern hemisphere, yet the same variations of current were manifest upon the line for an hour afterward. There was during this time a very strong turning current from the east, which resembled a reversed current so much that I disconnected my battery and put on a 'ground,' but I could not then get magnetism sufficient to work a simple armature. At $12^h\ 30^m$ the current from the east assumed a new feature, producing enough magnetism to work quite well, yet wavering and varying in intensity.

5. *Observations made at Philadelphia, communicated by* H. Emmons Thayer, *Telegraph Superintendent.*

On the evening of Aug. 28th, about 8 o'clock, we lost current on all our four wires running from Philadelphia to New York, and we had strong circuit as if from a near ground connection; but there was no interruption on wires running south to Baltimore and Washington. At $9^h\ 10^m$ the wires were relieved to a great extent from the influence of the Aurora, giving us our usual working current.

On testing wires at 8 o'clock on the morning of Sept. 2d, I found two of our wires, those running *via* Camden and Amboy to New York, strongly under the influence of an Aurora. The effect was different from that of Aug. 28th. There was an intensity of current which *gave a severe shock* when testing, giving a reversed current, neutralizing our batteries, and destroying magnetism. On removing the batteries, we had a very strong circuit, giving powerful magnetism, but could not raise New York. On the line running from this city to Pittsburgh, the operator, Mr. Steacy, succeeded in transmitting a business message to Pittsburgh *wholly on the auroral current.* The current was changeable, suddenly disappearing and returning at intervals of from five to ten minutes. The signals were distinct and

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the conversation lasted four or five minutes, the operators exchanging remarks as to the singularity of the phenomenon. At 9 A. M. all the wires were relieved from the effects of the Aurora, and worked well as usual.

6. *Observations made at Washington, D. C., by* FREDERICK W. ROYCE, *Telegraph operator.*

On the evening of Aug. 28th I had great difficulty in working the line to Richmond, Va. It seemed as if there was a storm at Richmond. I therefore abandoned that wire, and tried to work the northern wire, but met with the same difficulty. For five or ten minutes I would have no trouble, then the current would change, and become so weak that it could hardly be felt. It would then gradually change to a 'ground' so strong that I could not lift the magnet. The Aurora disappeared at a little after 10 o'clock, after which we had no difficulty. During the auroral display, I was calling Richmond, and had one hand on the iron plate. Happening to lean towards the sounder, which is against the wall, my forehead grazed a ground wire. Immediately I received *a very severe electric shock,* which stunned me for an instant. An old man who was sitting facing me, and but a few feet distant, said that he saw *a spark of fire* jump from my forehead to the sounder.

7. *Observations made at Pittsburgh, Pa., communicated by* E. W. CULGAN, *Telegraph manager.*

During the Aurora of Aug. 28th the intensity of the current evolved from it varied very much, being at times no stronger than an ordinary battery, and then suddenly changing the poles of the magnets it would sweep through them, charging them to their utmost capacity, and compelling a cessation of work while it continued.

On the morning of Sept. 2d, at my request the Philadelphia operator detached his battery, mine being already off. We then worked with each other at intervals as long as the auroral current continued, which varied from thirty to ninety seconds. During these working intervals we exchanged messages with much satisfaction, and *we worked more steadily when the batteries were off than when they were attached.*

On the night of Aug. 28th the batteries were attached, and on breaking the circuit there were seen not only sparks (that do not appear in the normal condition of a working line) but at intervals regular *streams of fire*, which, had they been permitted to last more than an instant, would certainly have fused the platinum points of the key, and *the helices became so hot* that the hand could not be kept on them. These effects could not have been produced by the batteries.

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ART. XIII.—*On Numerical Relations existing between the Equivalent Numbers of Elementary Bodies;* by M. CAREY LEA, Philadelphia. Part I.

(1.)

THE determination of the chemical equivalents of the simple substances seems with each new approach to accuracy, to destroy more and more the numerical relations once supposed to exist between the equivalent numbers of certain series of elements nearly related to each other by their properties. If we except the series formed by oxygen, sulphur, selenium and tellurium, there probably remains none of those usually recognized in which the numerical relation is rigorously exact. Chlorine, bromine and iodine are represented by the numbers 35·5, 80 and 127, where the difference between the first and second term is 44·5, between the second and third, 47. Lithium, sodium and potassium have the numbers 6·95, 23, 39·2; difference of first and second terms, 16·05, of second and third, 16·20. Calcium, strontium and barium have 20, 43·77 and 68·6; difference of first and second, 23·77, of second and third, 24·83. It can be demonstrated that other relations exist, approaching quite as near to exactness as these, and some where the accuracy is perfect.

Few of the so-called elements present more directly marked analogies than nitrogen, phosphorus, arsenic and antimony, and the very interesting discoveries of Cahours and Hofmann respecting the phosphorus bases have shown that phosphorus stands in every respect intermediate between nitrogen and arsenic, forming compounds of the type $3(C_4H_5)PHCl$, &c., like the nitrogen compounds as well as those of the type $3(C_4H_5)PO_2$, &c., like those of the arsenic and antimony groups. These authors further observe that the equivalents of phosphorus, arsenic and antimony differ by nearly the same number (44 to 45), but that nitrogen does not exhibit this relation.*

Beyond the fact of the approximate equality of these two differences, the analogy has never been extended. The following considerations will show that this relation not only extends to nitrogen but may be carried with exactness to other elements.

If we form a descending arithmetical series beginning with antimony =120·3, and diminishing by a common difference of 45 (45·3 in one instance, 44 in several) we shall find that such a series does not cease with the third term, P=31, but gives for a fourth −14, the exact equivalent of nitrogen with a negative sign. The fifth term will be −59, the exact equivalent of tin, with a negative sign. The sixth will be −104, or very nearly the equivalent of lead (also with a negative sign). The seventh

* Kopp u. Will, Jahresbericht für 1857.

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−149, very nearly the double of the equivalent of arsenic, a previous term in the same series. These results are exhibited in the following table.

Differences.	Calculated equivalents.		Received equivalents.
	120		Sb = 120·3
45	75		As = 75
44	31		P = 31
45	− 14		N = 14
45	− 59		Sn = 59
45	−104		Pb = 103·5
45	−149		2As = 150
45	−194		—— ——
45	−239		2Sb = 240·6
45	−284		—— ——
44	−328		= 2×164
44	−372		—— ——
44	−416		2Bi = 416

It will be seen presently that the number 164, the eleventh term in the above table, occurs also in the ascending positive series, and may represent the equivalent of a metal existing but as yet unknown.

If we examine the position occupied by antimony, arsenic, phosphorus and nitrogen in the electro-chemical scale of Berzelius we shall find that in proportion as their equivalent numbers diminish, their properties become more and more electro-negative; a corresponding change is also visible in the organic radicals which these elements are capable of forming by their union with carbon and hydrogen. The passage from the positive to the negative sign in the interval between phosphorus and nitrogen is accompanied by a marked change in the nature of the organic radicals into which these elements enter—$3(C_4H_5)N$ does not possess the power of combining directly with oxygen, chlorine and sulphur which $3(C_4H_5)P$, $3(C_4H_5)As$, $3(C_4H_5)Sb$ exhibit in so high a degree. The methyl compounds show the same differences as the ethyl. Standing between nitrogen and arsenic, phosphorus is every way more closely allied to the latter of

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these substances, not only by the analogies of its organic radicals, but also by the polybasic nature of phosphoric acid.* Although tin and lead represent the further members of the same series in reference to their equivalent numbers, it is evident that the increase of electro-negative relations does not extend to them. Moreover bismuth, antimony, arsenic, phosphorus and nitrogen at their maximum of oxydation combine with five equivalents of oxygen and chlorine, whereas tin unites with but two of each, and lead at most with two of oxygen and one of chlorine.

Again, if we begin with phosphorus, and form an ascending series with a common difference of 44 (except in one instance), we shall find both the number 164, the double of which constituted the eleventh term of the preceding table, and also the equivalent of bismuth, the double of which formed the thirteenth term of the same table.

Differences.	Calculated equivalents.			Received equivalents.
	31	-	-	P = 31
44				
	75	-	-	As = 75
45				
	120	-	-	Sb = 120·3
44				
	164	-	-	— ——
44				
	208	-	-	Bi = 208

These four elements exhibit strong analogies and are all isomorphous with each other.

If, taking mercury as a starting point, we subtract the number 44 from each term to find the following one, we shall obtain the series—

Differences.	Calculated equivalents.			Received equivalents.
	100	-	-	Hg = 100
44				
	56	-	-	Cd = 56
44				
	12	-	-	Mg = 12
44				
	−32	-	-	Zn = 32·6

The salts of the protoxyds of the three last of these metals are isomorphous. It may seem forced to place mercury in the same group, but its analogies with zinc are perhaps as strong as those which it exhibits with silver, next to which it has usually been classed, principally because the oxyds of both metals are reduced by heat. Mercury, like zinc, cadmium and certain other metals, is capable of replacing an atom of hydrogen in hydrid of ethyl, C_4H_5H, and of producing in this manner the conjugate

* See the very interesting paper of Cahours and Hofmann above referred to.

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organic radical mercur-ethyl, C_4H_5Hg, analogous to zinc-ethyl, C_4H_5Zn. This compound has recently been isolated by Buckton.* Silver does not appear to be capable of a similar substitution; when chlorid of ethyl is made to react upon zinc-ethyl the products are ethyl, chlorid of zinc and metallic silver. It has lately been shown by Hallwachs and Schafarik that magnesium forms an ethyl-magnesium.† These metals are likewise all volatile, and it has been shown by H. St.C. Deville that magnesium, like the others, is readily distilled.

It is not a little curious that the numerically negative members of this series lead into the positive members of the foregoing; if we continue the subtraction of 44, we find for the fifth number 76, or nearly the equivalent for arsenic, for the sixth 120, very nearly that of antimony, for the seventh 164, corresponding, as before remarked, with a possible undiscovered metal, and for the eighth 208, or exactly the equivalent of bismuth. The two series thus naturally lead to each other. The equivalent for arsenic given by L. Gmelin, viz. 75·4, accords more nearly with some of these series and renders them more exact than that adopted by the Jahresbericht which has been here employed.

The members of these two analogous series are further united by the fact that all of them, eleven in number, are capable of uniting with the hydro-carbons of the methyl, ethyl, &c. type to form powerful organic metals, and that this capacity appears to be limited to these elements alone.‡

(2.)

The magnesia group includes a well marked natural family of metals, whose oxyds having the constitution RO are related with each other by isomorphism. The equivalents of these metals, according to the most recent determinations, are as follows:

Magnesium,	12	Chromium,	26·7
Manganese,	27·5	Zinc,	32·6
Iron,	28	Cadmium,	56·
Cobalt,	29·5	Copper,	31·7
Nickel,	29·6	Lead,	103·5
Uranium,	60		

[Some of the elements here enumerated are found also in the foregoing or following series. It need scarcely be remarked that no absolute classification into groups is possible; lead for instance is in some of its combinations isomorphous with the barium group, in others with the magnesia, copper in grey cop-

* See this Journal, [2], xxviii, 146. † This Journal, [2], xxviii, 147.

‡ There appears some reason to believe that an ethyl-aluminum may exist, which would form an exception to this law. If so, the same property may possibly be extended to others of the metals of the earths.

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per ore is isomorphous with silver, while its carbonate, sulphate and seleniate are isomorphous with those of the magnesia group.] The equivalents of the above metals are related in the following manner by the number 44:

Cu 31·7	Zn 32·6	Cd 56
Mg 12·	Mg 12	−Mg 12
43·7	44·6	44
U 60	U 60	U 60
Mn 27·5	Fe 28	Co 29·5
87·5, ½=43·75	88, ½=44	89·5, ½=44·75
U 60	U 60	Pb 103·5
Ni 29·6	Cr 26·7	−U 60
89·6, ½=43·5	86·7, ½=43·35	43·5
Cd 56	Cd 56	Pb 103·5
Zn 32·6	Cu 31·7	Fe 28
88·6, ½=44·3	87·7, ½=43·9	131·5, ⅓=43·8

and so likewise by adding to the equivalent of lead, the equivalents of manganese, cobalt, nickel, and chromium, we obtain in each case a number almost exactly equal to three times the number 44.

To sum up: with Cu and Mg, Zn and Mg the sum of each pair is 44 nearly.

With Cd and Mg, Pb and Ur, the difference of each pair is 44 or nearly.

With U and Mg, U and Fe, U and Co, U and Ni, U and Cr, Cd and Zn, the mean term is 44 or nearly.

With Pb and Mn, Pb and Fe, Pb and Ni, Pb and Co, Pb and Cr, the sum of each pair is three times 44 nearly.

It has been before pointed out that the strong analogy existing between Mg, Cd and Zn extends to their equivalents, that of Mg being added to that of Zn gives the number 44 nearly, subtracted from that of Cd, 44 exactly.

(3.)

The following metals may be classed together as tending to form acids:

Tin, 59	Vanadium, 68·8
Titanium, 25	Molybdenum, 48
Tantalum, 68·6	Tellurium, 64
Tungsten, 92	Niobium, 48·82*

Relations depending upon the number 44 exist between these equivalents.

* By the recent determination of H. Rose, Poggendorff, civ, ext. in this Journal, [2], xxvii, 127.

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If from Ta 68·6 we subtract Ti 25, the remainder is 43·6, 44 nearly.

If from V 68·8 we subtract Ti 25 the remainder is 43·8, 44 nearly.

If from W 92 we subtract Mo 48 the remainder is 44 exactly.

If from W 92 we subtract Nb 48·82 the remainder is 43·18.

If to Te 64 we add Ti 25 the sum 89 is twice 44 nearly.

If to Te 64 we add Ta 68 the sum 132·6 is three times 44 very nearly.

If to Te 64 we add V 68·8 the sum 132·8 is three times 44 very nearly.

If the equivalent of Sn=59 be multiplied by $\frac{3}{4}$ the result is 44·25, in other words $3Sn=4\times44{\cdot}25$.

If we add Sn 59, Ta 68·6, W 92, V 68·8 and Te 64, the result is 352·4, $8\times44=352$.

If we add Ta 68·6, Nb 48·82, Sn 59, we have 176·42, $4\times44=176$.

If we add Ta 68·6, Mo 48, Sn 59, we have 175·6, $44\times4=176$.

If we add W 92, 2Te 128, we have 220, $5\times44=220$.

If we add Ta 68·6, Mo 48, 2Sn 118, V 68·8, Nb 48·82, we have 352·22, $8\times44=352$.

These analogies, though many are very complex, approach exceedingly near to absolute exactness; in the last for instance the difference amounts to only $\frac{1}{176}$ of the whole amount.

(4.)

If commencing with gold, Au=197, we form a diminishing series with a common difference of 44·5, we shall find for its terms—

Differences.	Calculated equivalents.			Received equivalents.
	197	-	-	Au=197
44·5				
	152·5	-	-	— ——
44·5				
	108	-	-	Ag=108
44·5				
	63·5	-	-	Cu = 63·4

The equivalent of Cu has been here taken at double that usually employed, or that which results from taking the first oxyd of copper as CuO, a view formerly entertained by Berzelius, L. Gmelin and other distinguished chemists.* It is doubtful if this be not the true equivalent; in grey copper ore, if we consider Cu as 32·7 we must admit that one equivalent of silver is replaced by two of copper, which is improbable. Moreover the two sulphides, AgS and CuS (Cu=63·4) are isomorphous.†

* Thomson, Inorganic Ch., i, 592.

† L. Gmelin, Handbook, i, 95. The recent experiments of Cannizaro give additional reason for doubling the equivalent of copper.

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Viewed in this light the above series approaches very near to exactness, and may probably be wholly so, for the equivalent of copper is by no means positively known. Dumas' recent researches were too unconcordant to permit him to offer a positive opinion. It will be shown further on that these metals, as well as many others here grouped together by relations depending upon a number approximating to 44·5, are also united by analogies of atomic volume.

The second number in the above series, 152·5, does not correspond with the equivalent of any known element, and it, like the number 164, which occurs twice in the nitrogen series, may represent the equivalent of some elementary body as yet unknown. In the second part of this paper, an additional argument will be presented in favor of the possible existence of two elements having these equivalent numbers.

The same relation may be extended with more or less approximation to the platinum group, which naturally subdivides itself into two families: the members of each family have very nearly the same equivalents, and the difference between the equivalents of the two families approaches to 45.

Rhodium,	52·2	Platinum,	98·7
Ruthenium,	52·2	Iridium,	99
Palladium,	53·3	Osmium,	99·6

The difference between the equivalents of platinum and palladium is 45·4, the rest vary a little more.

The elements, carbon, boron and silicium, are united by the number 44 in the following manner: if we take Gerhardt's equivalent for carbon $\Theta=12$, the sum of the equivalents of these three substances amounts to 44 exactly.

Carbon (Θ),	12
Boron,	11*
Silicium,	21·
	44

With respect to the metals of the earths and alkaline earths, magnesium and calcium have been considered elsewhere, yttrium, erbium and terbium, and the cerite metals have not yet been sufficiently examined, the equivalents of the three first have in fact not yet been determined, thorium exhibits a relation which will be hereafter pointed out; there remain therefore glucinum, aluminium and zirconium. The equivalents of these metals, viz. 4·7, 13·7 and 22·4 are evidently too small to follow precisely the same law which governs the various series previously considered, but that it extends also to them becomes evident

* As determined by Deville by analyses of bromid of boron.

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by multiplying their equivalents by 5, when they form a series analogous to the foregoing.

Differences.		
45	23·5 - -	5G = 23·5
45	68·5 - -	5Al = 68·5
	113·5 - -	5Zr = 112·

(5.)

If we take the equivalent of nitrogen, add to it three times 45 and divide by 8, we shall have nearly the equivalent of fluorine, and by continuing nearly in the same manner, we obtain those of chlorine, bromine and iodine.

$$\frac{14+3\times45}{8} = 18{\cdot}62, \quad Fl = 19$$

$$\frac{14+6\times45}{8} = 35{\cdot}5, \quad Cl = 35{\cdot}5$$

$$\frac{14+14\times45}{8} = 80{\cdot}5, \quad Br = 80$$

$$\frac{2\times14+22\times45}{8} = 127{\cdot}25, \quad I = 127$$

(6.)

Several elements, taken in pairs, have equivalents which differ from each other by a number approximating to 45.

Na	23	+	45·6 =	Ba	68·6
Be	4·7	+	43·3 =	Di	48
Ti	25	+	43·6 =	V	68·6
Cl	35·5	+	44·5 =	Br	80
Al	13·7	+	45·9 =	Th	59·6
Zr	22·4	+	46·4 =	Ba	68·6

(7.)

At the commencement of this paper reference was made to the well known numerical relations existing between Li, Na, K; Ca, Sr, Ba; Cl, Br, I. Inexact as are these (with the exception of the first which approaches tolerably near), the strong analogies which exist between the substances themselves, give to their relations an interest which they would not otherwise possess, and it is to be remarked that they all belong to that class which it has been the object of this paper to develop, viz., those depending upon the number 44–45.

1. The equivalent of sodium is well known to be nearly a mean between those of lithium or potassium—if the equivalent

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of lithium be added to that of potassium, or if that of sodium be doubled, a number is obtained in either case approaching nearly to 45.

2. The equivalent of strontium is nearly a mean between those of calcium and barium: Sr=43·75, half the sum of Ba 68·6 and Ca 20 is 44·3; in either case 44 nearly.

3. The equivalent of chlorine, 35·5, subtracted from that of bromine, 80, gives 44·5. The equivalent of bromine subtracted from that of iodine gives 47.

(8.)

With respect to three of the groups already considered, a further relation exists. For greater clearness the groups in question have been arranged in the following table:

Group A.			Group B.		
Differences.			Differences.		
	100	Hg=100		208	Bi =208
44	56	Cd= 56	44	164	— —
44	12	Mg= 12	44	120	Sb =120·3
44	−32	Zn = 32·6	45	75	As = 75
			44	31	P = 31
			45	−14	N = 14
Group C.			45	−59	Sn = 59
44	92	W = 92	44·5	−103·5	Pb =103·5
	48	Mo= 48			

If to the equivalent of the substances forming series A, we add successively to each term the equivalent of fluorine, we shall obtain four corresponding terms of series B.

Mercury	100	Cadmium	56	Magnesium	12	Zinc	−32·6
Fluorine	19	Fluorine	19	Fluorine	19	Fluorine	19
	119		75		31		−13·6
Antimony	120·3	Arsenic	75	Phosphorus	31	Nitrogen	14

Further: If to two corresponding terms of series A we add the equivalent of chlorine, we shall obtain the two terms of series C.

Cadmium	56	Magnesium	12
Chlorine	35·5	Chlorine	35·5
	91·5		47·5
Tungsten	92	Molybdenum	48

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And if to two terms of the same series A we add the equivalent of bromine, we also find the terms of series C.

Magnesium	12	– Zinc	– 32·6
Bromine	80	Bromine	80
	92		47·4
Tungsten	92	Molybdenum	48

If we add respectively the four terms of series A with the four corresponding terms of series B, we shall in each case obtain a number which is very nearly twice forty-four; that is, the mean of each pair of series is 44 nearly.

– Zinc	– 32·6	Magnesia	12	Cadmium	56	Mercury	100
Antimony	120·3	Arsenic	75	Phosphorus	31	– Nitrogen	– 14
	88·7		87		87		86

It will be remarked that where a series leads to the equivalent of an element, but with a negative sign, that negative sign has been in all cases preserved in the further examination of its numerical relations.

(9.)

It is evident that the number 44—45 plays an important part in the science of stoichiometry, and the relations which depend upon it are supported, in some cases at least, in a remarkable manner, by analogies of atomic volume. That such analogies are a support becomes evident from the following considerations.

Solids and liquids are very far from being governed by the laws which determine the combinations of gases, in volumes either equal or having some very simple relation to one another. Therefore, if we find that in some few instances such a relation does hold good with solid substances, we may naturally expect to find a close relationship existing between those substances thus united. We may even be permitted, by way of hypothesis, to advance a step further, and finding that a given volume of silver unites with a given volume of oxygen, and that the same volume of gold unites with precisely the same volume of oxygen, to conjecture that gold may differ from silver only by a third substance, which unites with the silver without increasing its volume, or affecting the amount of oxygen which it is capable of saturating, but which, on the other hand, alters its chemical equivalent, its specific gravity, and other physical characters.

Moreover, if we find that by subtracting from the chemical equivalent of silver, half the difference between the equivalents of silver and gold, we obtain the equivalent of a third metal, copper ($e_{Cu}=63·4$), which also, under equal volumes, combines with a quantity of oxygen expressed by a very simple relation

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with that capable of saturating gold and silver, we may at least speculate that the three may form a series consisting of two substances combined in different proportions. It is true that we must be extremely cautious about venturing upon hypotheses involving a compound constitution of bodies which all our efforts have hitherto proved ineffectual to decompose, but on the other hand it must be admitted that when we find so-called elements arranging themselves into a series of terms having a common difference, and when we find the terms of these series united by equality or simple relation of atomic volume, we cannot grant that their elementary nature has been absolutely established.

The following substances combine relations of chemical equivalents already pointed out, with analogies of atomic volume:

Differences of Equivalents.		Atomic† volume.	Relation of At. vol.
45*	Nitrogen, - - -	14·42	- 2
44	Phosphorus (vapor),	7·01	- 1
	Arsenic (vapor), - -	7·07	- 1
44·5*	Lead, - - -	9·09	- 1
45*	Tin, - - -	8·09	- 1
45	Nitrogen,		
44	Phosphorus, - -	17·7	- 2
45·3	Arsenic, - - -	12·58	- $\frac{3}{2}$
87·7	Antimony, - -	18·—	- 2
or twice 43·85	Bismuth, - - -	21·18	- $\frac{7}{3}$

(Where phosphorus, arsenic, &c., are compared in the solid state, the unit of relation is of course different). It has been already remarked that in point of chemical relations generally, lead and tin are less closely united with the series than the other members composing it, but the relation between the atomic volumes of lead and antimony, the latter almost the last term at the other end of the series, is almost absolutely exact. Nitrogen is of course omitted in the second table, as we do not know what would be its atomic volume in the solid state.

* In the cases of nitrogen, tin and lead, the equivalents are taken with a negative sign, as before explained.

† The numbers here given for the atomic volumes are calculated from the specific gravities adopted in Gmelin's Handbook, and the latest and most reliable determinations of chemical equivalents.

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Differences.		Atomic Volume.	Relation of At. Vol.
89 or twice 44·5	Gold, - - -	10·26 -	- 1
	Silver, - - -	10·35 -	- 1
44·6	Copper (Cu=63·4), -	7·27 -	- $\frac{2}{3}$

It will be observed that the difference between the equivalents of copper and silver approaches very near to absolute exactness with half the difference between the equivalents of silver and gold, and as the equivalent of copper is by no means positively settled, the relation may be rigorously exact. If we take the mean between the number adopted by L. Gmelin and that adopted by the Jahresbericht (always considering cuprous oxyd as CuO), we shall have for the difference between the equivalents of copper and silver the number 44·5, half the difference between the equivalents of silver and gold, with mathematical exactness.

Difference.		Atomic volume.	Relation of At. vol.
44	Tungsten, - -	5·28 -	- 1
	Molybdenum, -	5·58 -	- 1

In the series Hg, Cd, Mg, Zn, similar analogies are not well marked: the atomic volumes of the three first metals are not very far apart,

	Atomic volume.
Hg, - - - -	7·37
Mg,* - - -	6·85
Cd, - - - -	6·48

but the atomic volume of zinc differs considerably; it is 4·72.

In each of these different series, each term differs in its equivalent by the number 45 or a number approaching very near to 45, and yet the addition of this large amount of matter is in most cases accompanied by no change in volume, or when a change takes place, it is expressed by some simple relation to the original volume. Some of these relations of atomic volume are well known and are only here presented in view of the confirmation which they afford of the series here established, but it is believed that the connection of the atomic volume of copper with those of gold and silver, and of those of tin and lead with those of the elements of the antimony group are pointed out for the first time.

Ammermüller has noticed a fact not wholly dissimilar from this in the case of protoxyds of copper, mercury, tin and lead, which combine with a second equivalent of oxygen without change of atomic volume, the density alone increasing. But according to L. Gmelin, the specific gravities on which he based

* Taking the sp. gr. of magnesium at 1·75 as determined by Deville.

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his calculations are too unreliable to render the fact well established.

The numbers adopted for the equivalents in the foregoing calculations are those obtained by the latest and most reliable determinations; they are taken from the table contained in the Jahresbericht der Chemie of Kopp & Will for 1857, published in August, 1858, and the last which the author has been able to obtain at the time of concluding this paper, and have been in no case altered or modified in the slightest degree with a view to preserve or increase numerical relations, which by slight changes of this kind can be often rendered much more symmetrical. Dumas, in one of his highly interesting papers on this subject* (Comptes Rendus, XLV, 709, extracted in Kopp and Will's Jahr. 1857) in his series $a+xd+yd'$, adopts the equivalents N=14, P=31, As=75, Sb=119, Bi 207 (see Jahresbericht, p. 35, where the equivalent of Bi is erroneously printed 108, by substitution of the values given for a, x, y, d, and d', 207 is obtained): whereas the equivalent of Sb as lately found by R. Schneider, confirmed by H. Rose, and adopted by Kopp & Will is 120·3. In another place (Comptes Rendus, XLVII, 1027) Dumas has taken the equivalent of the same metal at 122, thus adopting alternately the numbers 119 and 122, neither the true one, according to the exigencies of the two series. The equivalent number of bismuth in the series just mentioned is taken at 207, whereas it should be 208. In the series $a+xd$ we find Mg=12, Ca=20, Sr=44, Ba 68, Pb 104—the last three should be Sr 43·77, Ba 68·6, Pb 103·5. So with Li, Na, K, V, Zr, &c.

In the foregoing tables the calculated and received equivalents are placed by side of each other for comparison. The differences rarely exceed the possible errors in the determination of chemical equivalents, respecting some of which there is still much doubt. Dumas, in the paper above referred to, gives the results of many new determinations by himself, and arrives at the number 26 for both chrome and manganese, instead of the ordinarily received Cr=26·7, Mn=27·5. For copper his results disagreed too much to lead him to any positive conclusion.

The analogies here presented, all depending upon the same or approximately the same number, extend therefore—

To the series Pb, Sn, N, P, As, Sb, Bi.
To the series Hg, Cd, Mg, Zn.
To the series Au, Ag, Cu.
To the magnesia group, including Mn, Fe, Co, Ni, U, Co, and some of the metals also classed in the three preceding series.

* The interesting paper of Prof. Cooke (Memoirs Amer. Ac., 2d ser., vol. v) to which the author's attention has been called since concluding this paper, will be more particularly referred to in the Second Part.

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To the metals belonging to the group Ti, Ta, W, V, Mo, Te and No; Sn belongs also to this series as well as to the first.
To the platinum group, Rh, Ru, Pd, Pt, Ir, Os.
To C, B, Si.
To G, Al, Zr.
The differences between Cl and Br, Br and I, approximate to the same number, as likewise do the relations between Li, Na and K, and between Ca, Sr and Ba.

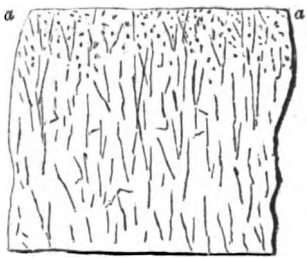
This relation, therefore, extends to no less than forty-eight of the elementary bodies: to all except those as yet imperfectly understood, most of which may yet range themselves under the same law, and except the oxygen group, oxygen, sulphur, selenium and tellurium, substances which stand alone and unmistakably apart from the other elements.

Philadelphia, Nov. 10, 1859.

Art. XIV.—*Remarks on the Dissolution of Field Ice;* by Chas. Whittlesey, of Cleveland, Ohio.

The interesting paper of Col. Totten, U. S. A., in the November number of this Journal for 1859, upon the rapid disappearance of ice in the northern lakes, recalls some observations that I had an opportunity to make on Lake Superior a few years since.

On the 8th of March, 1855, the inhabitants of Eagle River, a village in Haughton County, situated upon the most northerly part of Point Kewenaw, were engaged in procuring ice for their summer use. The severity of winter in that latitude (47° 22′ north) had so far relaxed, that the surface of the field was slightly porous from the direct action of the sun. There had been no rain; the atmosphere was clear and cool, but on the sunny side of houses and other objects the snow melted rapidly in the day time.



Block of ice 30 inches thick.
a a, upper surface.

Below the soft and moist surface, at a few inches in depth, the ice was solid and pure to the bottom, its thickness being thirty inches. The blocks which the people were cutting out, were taken about 1000 feet from the shore. One of them nearly in the form of a cube, of thirty inches on each face, was suffered to lie upon the unbroken ice, its natural surface uppermost, as represented in the figure here inserted.

I was thus enabled to take a direct view of the progress of its decay, as no doubt others have done many times, upon these lakes. As the force of the sun increased, the porous part on the

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surface increased rapidly in depth, lines or planes of separation extending downward from it into the hitherto transparent and homogeneous mass. There were not at any time horizontal planes visible, indicating layers or lamination, in the original structure. A thin film of matter followed each newly formed crevice downwards, and bubbles of air rose continually through the same to the surface. These planes of division converged below, giving the block the appearance above represented, of inverted spikes or rudely formed pyramids, with their bases upward.

By ten o'clock A. M. the upper half of the block was divided in this manner. The figures were somewhat regular and were principally triangular and rectangular, reminding me of the imperfectly columnar red trap of the north shore of Lake Superior. By noon the block was so far disintegrated that it fell to pieces under a single blow, and remained a pile of roughly formed spikes, pyramids and prisms of various lengths. After this as so much new surface was exposed to the sun it melted very fast. The newly cut ice was still solid and clear except a few inches at the surface.

There seemed to be in the block that had so suddenly lost its form and solidity, a process of contraction, arising from an increase of temperature. I presume that this appearance can be thus accounted for. No doubt the planes of division existed in the solid ice, as results of the crystallization in freezing. The general law of structure in all masses slowly crystallizing from a state of fusion is the production of a prismatic structure perpendicular to the cooling surfaces. Basalt assumes its polygonal figures in obedience to the same law, and the structure of ice is quite in accordance with it. Its effects are not wanting even in some pastes, like starch and domestic cake.

This structure exists often where it is concealed. An ingot of block tin shows no crystalline structure, but by slow fusion the amorphous parts melt and run out leaving a skeleton of crystalline prisms. Ice is in the same predicament, and since in freezing water expands one-seventh of its volume, the first result of the fusion of a part of it is to dissect out the prismatic masses, leaving them standing isolated by reason of their being on a larger scale than the fluid volume from which they were formed. In this process the air bubbles no doubt materially assist by opening channels of escape for the ice-water.

What I have stated may assist in explaining why immense fields of fresh water ice disappear in a single gale of a few hours duration. When the temperature rises above 32° the ice soon loses its cohesion, and the first agitation breaks it up. In popular phrase it sinks, and is thus lost sight of suddenly; but in truth it is dissolved by the warmer water acting upon the fragments in the shape of little columns and pyramids such as Col. Totten saw strewed along the shore of Lake Champlain.

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SCIENTIFIC INTELLIGENCE.

I. CHEMISTRY AND PHYSICS.

1. *On Platinum and the metals which accompany it.*—H. St. Claire Deville and Debray have published a very interesting and valuable memoir on the platinum metals, considering the subject rather from a metallurgical than from a purely chemical point of view. For the details of the processes employed we must refer to the original memoir, which rarely admits of abridgment, and which is in the highest degree instructive. The authors employ exclusively the *dry* method of investigation and operate at temperatures much higher than any which have hitherto been obtained upon a working scale. By a new arrangement of the oxy-hydrogen blowpipe most of the members of the platinum group may be fused—platinum even in larger quantities than was accomplished by Dr. Hare. By the same apparatus properly employed, the authors purify the metals and their alloys from more volatile elements with which they may be mixed. Osmium has a density of 21·3 to 21·4, and when dissolved in tin exhibits traces of a crystalline structure. It is bluish white, has no odor, and gives off vapors of osmic acid only above the temperature of melting zinc. At the temperature of melting ruthenium, osmium is sensibly volatilized but it does not fuse, and hence resembles arsenic in having its boiling point lower than its point of fusion. Two determinations of the density of the vapor of osmic acid gave 8·88 and 8·89, corresponding to 2 vols. Next to osmium ruthenium is most difficult of fusion, but may yet be obtained in small fused masses when its density is from 11 to 11·4. The authors give analyses of the protoxyd of ruthenium and of the crystallized deutoxyd isomorphous with stannic acid. They also describe a beautiful alloy of ruthenium and tin having the formula $RuSn_2$ and crystallizing in cubes. Palladium fuses even more readily than platinum, and volatilizes at the temperature at which iridium melts. It also absorbs oxygen when fused like silver without becoming oxydized. Its density at 22·5° is 11·4. With tin it forms an alloy crystallizing in small brilliant plates having the formula Sn_2Pd_3. Rhodium fuses less easily than platinum; it has about the color of aluminum and when pure is malleable and ductile; its density is 12·1. It forms crystalline alloys with zinc and tin.

Platinum may (as first shown by Dr. Hare) be fused in large quantities before the oxy-hydrogen blowpipe. Deville and Debray give a detailed description of the apparatus by which this metal may be fused in quantities of not less than 12 to 15 kilograms at an expense of from 0·24 fr. to 0·40 fr. per kilogram. (The late Dr. Hare fused 28 ounces at one operation.) The fusion of platinum is best accomplished in crucibles of lime, which serve to refine the metal by absorbing the impurities. When fused and refined, platinum is as soft as copper; it is whiter than ordinary platinum and free from porosity; its density is 21·15. With tin platinum forms a beautiful crystallized alloy having the formula $PtSn_4$. Iridium requires an extremely high temperature for its fusion, but when fused has a pure white color, and is brittle under the hammer like a crystalline metal; its density is the same as that of platinum, namely 21·15.

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(Dr. Hare, who long ago succeeded in fusing iridium, found its density 21·83.) Iridium forms with tin a beautiful alloy crystallizing in cubes having the formula $IrSn_2$.

The authors remark that the alloys of platinum with iridium and rhodium are much more valuable in the arts than pure platinum, many of them resisting the action of aqua-regia, and possessing a considerable degree of hardness and rigidity. The memoir contains in addition numerous elaborate analyses of different specimens of platinum ore and of osmiridium, as well as new processes for the treatment of platinum ores upon the larger scale, the preparation of pure platinum for industrial purposes, and of alloys of platinum with its associate metals possessing useful properties. For these we must refer to the original.—*Ann. de Chimie et de Physique*, lvi, 385, Aug. 1857.

[*Note.*—A memoir read before the Am. Association for the Advancement of Science at its meeting in August 1859, and shortly to appear in the 12th volume of the Transactions of the Smithsonian Institution, contains entirely new processes for the separation of all the platinum metals in a state of absolute purity. These processes are in the wet way; they are very simple and easy of execution, and they not only apply to the separation but to the qualitative analysis of mixtures of the different metals of this group in almost any proportions. The methods in question involve the preparation and properties of a new and remarkable series of salts, and will I hope be found to remove completely the difficulties which have hitherto surrounded the subject.—W. G.]

2. *Blowpipe experiments.*—Bunsen has contributed some very interesting additions to our knowledge of the use of the blowpipe in quantitative as well as qualitative analysis. The author employs the peculiar form of gas burner, first introduced by him, and now used in all laboratories, instead of the blowpipe. The lower part of the flame is surrounded by a conical sheet iron chimney, 30^{mm}. in diameter above, and 55^{mm}. below, so that the burner tube is in the axis of the chimney and 45^{mm}. below the upper base of the cone. The cock is to be regulated so that the apex of the inner non-luminous cone of gas within the flame exactly reaches the level of the upper base of the chimney. In this manner we obtain a flame of perfectly constant dimensions which is immovable, sharply defined in all its parts, and which may always be obtained of uniform character. The outer cone of flame has a very faint sky-blue color, which is invisible even by feeble daylight. The inner cone of flame is less intensely blue than the outer. The object to be submitted to the action of the flame must never be larger than the half or the third of a grain of millet seed. It is to be introduced into the flame by means of a little loop on the end of a platinum wire which is attached to a holder by which it may be moved gently and steadily, so that the object may be introduced into any part of the flame. The loop is to be moistened with water, when a grain or a little of the powdered substance will readily adhere to it.

The author remarks that the temperature which the flame is capable of producing depends principally upon the constitution of the gas consumed. The temperatures corresponding to gas analyzed in the Heidelberg laboratory on four different occasions were 2369 C., 2352 C., 2391

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C., 2386 C., or as a mean, 2350 C., so that the temperature of the flame where the quantity of air is exactly sufficient for the combustion of the gas, may be assumed in round numbers as 2300 C. It is easy to see, however, that the temperature will vary in different parts of the flame. The author gives a simple and elegant method of determining the point of maximum temperature by introducing a platinum wire into the flame, and determining at what point the light emitted by this is most intense. In this manner it is found that the zone of maximum temperature lies in the external cone of flame, a few millimetres above and below the apex of the internal non-luminous cone, which is on a level with the upper base of the chimney. The author employs this zone to investigate the action of a temperature of 2300 C. upon different substances, and terms it the *melting space*. The outer border of this *melting space* acts as an oxydizing flame, the inner as a reducing flame, the reduction being most powerful immediately above the apex of the innermost cone. The great constancy which the flame exhibits in all its parts allows us to observe and estimate the volatility of substances at the very high temperature of 2300 C. For this purpose a mass of matter having a measured diameter of 1 millimeter is introduced into the flame, and the time required for complete evaporation determined by means of a seconds pendulum or a metronome. The size of the mass introduced upon the platinum wire is easily regulated by adding new substance or by evaporation in the flame, and may be measured under the microscope. The author takes the volatility of carbonate of soda as unity, and gives a table of the comparative volatility of different substances in terms of this unit: thus the volatility of chlorid of potassium is 15·33; of chlorid of sodium 6·57; of phosphoric acid 23·00. Other substances are more or less completely decomposed at the temperature of 2300 C.

Besides its use in experiments on volatility the flame may also be employed for a series of other very valuable blowpipe reactions, among which the author cites the quantitative determination of soda in the presence of potash and lithia.

For the simple recognition of soda in its volatile salts, it is sufficient to introduce a small bead of the substance into the melting space and then to illuminate a crystal of bicromate of potash with the light produced. The salt appears perfectly colorless, transparent, and with a diamond lustre so long as the rays of the soda flame fall upon it, and this even by ordinary lamplight or daylight. A still more delicate reaction is obtained by using paper, covered with iodid of mercury, a square centimeter of which may be attached to the chimney in front of the flame by a movable arm. If we introduce the smallest quantity of a soda compound into the melting space, the red paper appears white, with a faint tinge of tawny yellow. When the soda salt is in solution the loop of the fine platinum wire may be flattened under the hammer to a little ring. This ring introduced into the liquid will take up a drop which must be gently evaporated to dryness and then tested as before. In this manner $\frac{1}{10,000}$ of a milligram of common salt may be easily detected.

Volatile potash compounds, as is well known, communicate a bluish violet tint which is completely concealed by small quantities of soda. In this case the potash may easily be detected by means of Cartmell's

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reaction, that is, by looking at the flame through a deep blue cobalt glass, when a violet or ponceau-red color appears. This potash test is even more delicate than that for soda, $\frac{2}{10,000}$ parts of chlorid of potassium may be detected with perfect distinctness.

The detection of lithia in the presence of potash and soda is best effected by looking at the flame through a hollow prism filled with a solution of sulphate of indigo. The carmine-red color of the lithia flame disappears when a certain thickness of solution is reached, and if a mark be put upon the prism, all the layers of liquid above the mark will allow only the red potash-rays to pass through. Lime and soda have no influence on this reaction. When potash and lithia are both present the flame from the salt to be tested should be compared with that of pure potash. The flame containing lithia and potash appears through thin layers of liquid redder than the potash flame alone; through thicker layers the potash flame appears scarcely weakened. In this manner some thousandths of lithia may the discovered in potash salts.

To render these processes available for silicates Bunsen mixes the mineral with pure gypsum and heats in the melting space, when silicate of lime and volatile sulphate of potash are formed. By comparing the intensity of the color produced by a mineral to be tested with that of a series of silicates whose percentages of alkali are known, it is easy even with very small fragments to determine the relative quantities of potash, soda and lithia with tolerable approximation.

The author in the first place determines whether the mineral to be examined contains lithia or not by the method already explained. The minerals of the first group are those containing no lithia, and the author arranges a scale of minerals for comparison according to the content of soda in each. These are—

	NaO	KO
1. Lazulite,	9·09	—
2. Nepheline,	15·44	4·94
3. Albite,	10·06	—
4. Orthoclase,	7·08	7·03
5. Sanidin,	4·0	8·0
6. Labradorite,	2·55	1·06
7. Anorthite,	1·13	0·62
8. Leucite,	—	2·2

These are to be ignited, pulverized and preserved with their numbers as blowpipe reagents. One of these and the mineral to be tested, with or without gypsum, are to be introduced into the flame together so that small equal ends of wire are ignited; the iodid of mercury paper then appears more or less bleached. Remove the test from the flame, if now the paper shows a reddish tint, the test contains more soda than the mineral used for comparison. If the paper however becomes paler the contrary is the case. In this manner it is easy to determine between what two minerals the test lies, so that the percentage of soda may be estimated within a few per cent. The substances to be compared must be as nearly as possible in equal quantities, the ignited lengths of wire be the same and the soda-flame of the same size and form. The eye must be accustomed to distinguish the different degrees of brightness of the same tint

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from actual differences of tint. When the iodid paper is intensely bleached it may also be illuminated by a candle flame so that the soda-flame produces with this foreign light a white tint nearer to red.

The potash color-test is not as accurate quantitatively as that for soda. It is sufficient for all purposes to distinguish *a slight*, *a strong*, and a *very strong* potash reaction, using for comparison in succession the flame of oligoclase, orthoclase and leucite heated with gypsum in the same flame with the test. The indigo prism is to be used and the dimensions, color and duration of the red flames observed.

Lazulite gives a stronger soda reaction than nepheline because it contains sulphuric acid, and it is always necessary to determine beforehand whether the test contains sulphuric acid, chlorine or fluorine. This is best accomplished by the common blowpipe.

For further details we must refer to the original memoir, which must create an entirely new department in blowpipe analysis.—*Ann. der Chemie und Pharm.*, cxi, 257.

[*Note.*—It is easy to see that Bunsen's memoir contains the solution of many chemical and physical problems of great interest. Thus it is easy to produce at will a flame which shall have any required temperature, at least between certain limits. Since according to Bunsen's calculations (Gasometrische Analyse, p. 254) the flame of hydrogen burning freely in the air has a temperature of 3259° C., and that of olefiant gas has a temperature of 5413° C., we may as readily experiment at these temperatures as at 2300° C. Very much higher temperatures are of course produced when these gases burn with pure oxygen—in the case of hydrogen 8061° C.; in that of olefiant gas 9187° C. Hence by burning mixtures of these gases with oxygen and varying the proportion of nitrogen arbitrarily, we can make a scale of flames the temperatures of which shall range from less than 2000° C. up to at least 9000° C. and the melting point as well as degree of volatility of almost all metals and mineral substances may be thus assigned within quite narrow limits. It is easy to calculate the percentage of nitrogen or carbonic acid to be added in each case to the combustible gas in order to produce the required temperature. It appears probable that at high temperatures the radiating power of a body for heat is proportional to its radiating power for light. By directly comparing the intensity of the light radiated from platinum heated in a furnace with the intensity of the light radiated when the platinum is heated to a known temperature in a gas-flame, the temperature of the furnace might be approximately determined. Interesting results could also be obtained as to the exact temperature at which bodies become luminous and as to the relative quantities of light which different substances emit when heated to the same temperature, or the same substance at different temperatures. With respect to Bunsen's scale or series of minerals containing different but known proportions of soda I will suggest that perhaps a series of *glasses* could be made containing perfectly definite quantities of either soda or potash from 2 up to 30 per cent, so that each number would contain 2 per cent more alkali that the next lowest number. These would then serve as universal standards of comparison and give much precision to blowpipe observations.—W. G.] W. G.

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II. GEOLOGY.

1. *Review.—On some points in the Geology of the Alps.**—For nearly a century the great problem of the structure of the Alps has engaged the attention of geologists, and although much still remains to be done, we are gradually attaining to a clearer notion of the age and the mode of formation of this great mountain region. Among the most remarkable conclusions deducible from modern investigations is that of its very recent upheaval, and the age, geologically modern, of a large portion of its rocks. The great mass of nummulitic strata, with the overlying series of slates, limestones, marls, and fucoidal sandstones (once regarded as of transition age), which constitute the *flysch*, and belong to the eocene division of the tertiary rocks, has been upheaved vertically more than two miles, and penetrated by intrusive granites. Then took place the great movements of folding, inversion, and lateral and vertical displacement, the slow sinking of the northern side of the chain permitting the accumulation of the conglomerate of the *molasse* or the *nagelflue* (a formation partly freshwater and partly marine), which belongs to the miocene, and in part perhaps to the pliocene, and attains in some parts a thickness of from 6000 to 8000 feet. After the subsidence which allowed this vast accumulation, we find it raised by a vertical displacement of many thousand feet, and dipping towards the older rocks, so that the upper beds of the *nagelflue* seem to pass beneath the nummulitic limestones. (Murchison, *Quar. Jour. Geol. Soc.*, v, 241.) These great foldings and dislocations have often given rise to lateral movements by which the older strata have actually been forced over and made to rest unconformably upon younger rocks.

The Messrs. Rogers have shown that these great movements of the Alps present a close resemblance with those observed by them in the Alleghanies, and we are persuaded that a farther study of our older mountain chains, rendered more easy of investigation from the smaller number of formations involved, and from the regions being less elevated and more accessible, will throw still farther light upon the structure of the Alps and of mountain chains in general.

The vast thickness of the sedimentary deposits of secondary and tertiary age over the area now occupied by the Alps, when compared with their volume in other parts of Europe, serves to exemplify the relation which Prof. James Hall has so well pointed out, of the apparent connection of mountain elevations with original deposition (*Geology of Iowa*, i, 41). At the same time the weight of evidence now tends to show that the crystalline nucleus of the Alps, so far from being an extruded mass of so-called primitive rock, is really an altered sedimentary deposit more recent than many of the fossiliferous strata upon their flanks, so that the Alps as a whole have a general synclinal structure.

In the memoir before us the learned professor of geology in the Academy of Geneva, already well known by his researches in Alpine geology, has devoted himself to the study of a number of sections which exhibit

* *Memoire sur les terrains liassique et keuperien de la Savoie, par* ALPHONSE FAVRE. (Extrait du tome XV *des Memoires de la Société de physique et d'histoire naturelle de Genève*, 1859. Quarto, pp. 92, with three plates.)

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the anthracitic and jurassic formations of Savoy. In these sections which he has given with lists of their organic remains, he includes (1) under the general name of the jurassic system both the oolitic or jurassic proper and the lias. Beneath these he distinguishes (2) a succession of limestones, chiefly dolomitic (*cargneules*), with interstratified beds of gypsum, underlaid by red and green marls and a silicious sandstone or arkose, the whole series, which attains a thickness of 2500 feet, being destitute of fossils, and reposing upon (3) shales and sandstones which enclose beds of anthracite and plants of the carboniferous epoch. Succeeding these comes the conglomerate of Valorsine (4), which, with the preceding group, he refers to the carboniferous system, while beneath all these are (5) reddish crystalline schists and gneiss, passing into a rose-colored protogine or talcose granite.

The third division is regarded by Favre as representing the *keuper* or triassic system, which, although known in the Tyrol and in Lombardy, has not hitherto been recognized in Savoy. Its identification is regarded by our author as highly important, inasmuch as it establishes a well defined geological horizon throughout this region. To the triassic system he refers the gypsums which are found encircling Mt. Blanc, and the salt deposits of Bex, of the Tarentaise, and elsewhere. Favre however recognizes the existence of more recent gypsums, with *cargneules*, in a fucoidal sandstone overlying the nummulitic limestone. The *cargneule*, which is here a constant associate of the gypsum, is according to the recent analyses of Marignac (p. 11) a cellular limestone or tufa containing about nine per cent of carbonate of magnesia, while the cells are filled with a nearly pure pulverulent dolomite. Massive, compact and crystalline dolomites are however frequently associated with these cargneules, which Haidinger conceives to have been formed by the alteration of dolomites under the influence of a solution of gypsum. We are however inclined to regard the cargneules as analogous to the tufas which we have elsewhere described as often associated with true dolomites. (This Journal, xxviii, 372.) To this subject we propose soon to return, but we take occasion to correct an error into which Mr. Favre has fallen in attributing to Sir William Logan the view that magnesian limestones owe their origin to the action of solutions of carbonate of soda upon sea-water (p. 42). This has arisen from an oversight in quoting from the *Literary Gazette*, where some observations made by us at the meeting of the American Association at Montreal in August, 1857, are reported in immediate connection with a paper of Sir W. E. Logan. We have endeavored to show that carbonate of magnesia has been formed in nature from the decomposition of soluble magnesian salts either by solutions of bicarbonate of soda or bicarbonate of lime, in the latter case sulphate of magnesia being concerned and sulphate of lime being also a product. (*Comptes Rendus de l'Acad.*, xlviii, 1003, and this Journal, xxviii, 170.)

Mr. Favre has not endeavored to identify the Permian system in this region, although according to Pidancet it occurs with its characteristic flora, beneath the variegated sandstones of the trias, in the adjacent department of the Jura. (*Bul. Soc. Geol. de Fr.*, [2], xii, 149.) The trias, according to our author, rests directly upon the shales and sandstones which are regarded by him as belonging to the carboniferous system, and

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at other times upon crystalline schists, which however he suggests may be no other than the carboniferous rocks so highly altered as not to be distinguished from the older crystalline schists.

In the view which he takes of the relations of the jurassic and anthracitic rocks of the Alps, Mr. Favre is so completely at variance with most of those who have preceded him that it will be necessary to enter into some details with regard to this so-called anthracitic formation, which has been hitherto regarded as intercalated with and forming a portion of the jurassic system of the Alps. The conglomerate of Valorsine, which Favre classes with the palæozoic series, is according to Sismonda, no other than the *verrucano* of Savi, and the lowest member of the liassic series, beneath which at Jano in Tuscany is an anthracitic formation associated with a strictly palæozoic fauna. (Ibid, p. 635.) In various localities in Savoy, on the contrary, and particularly at Petit-Cœur in the Tarentaise and near Briançon, the anthracitic beds, consisting of sandstones and shales with impressions of ferns and layers of anthracite, appear to be intercalated in the jurassic system. This fact, announced by Elie de Beaumont in 1828, has been confirmed by a great number of subsequent observers, among which may be mentioned Sismonda, Murchison, Gras and Rozet. The fossil plants of this formation were by Brongniart and subsequently by Heer identified with those of the carboniferous system; according to Mr. Bunbury, who has since examined the fossils of Petit-Cœur, which are very much distorted and replaced by a film of talcose matter, their identification is difficult, and although some are undoubtedly carboniferous species, others are doubtful. (*Geol. Journ.*, v, xxix, 130.) Elie de Beaumont, with Murchison and Rozet, conceives that the evidence of the age of a formation furnished by fossil plants, to be far less reliable than that afforded by its fauna, and the latter imagines that during the jurassic period the climate may have permitted the development, upon islets in the midst of the sea, of a flora like that of the coal period. The observations of Elie de Beaumont in 1828 led him to refer the ferns and anthracites of Petit-Cœur as well as those of the neighborhood of Briançon to the summit of the lias. Both Gras and Sismonda distinctly recognize two horizons of anthracitic beds with fossil ferns, the one at the summit and the other at the base of the great anthracitic system; but while Sismonda (grounding himself upon the inferiority of the true coal formation to the conglomerate of Valorsine,) agrees with Elie de Beaumont and Murchison in regarding this system as really of jurassic age, although including a flora like that of the coal, Gras is inclined to look upon it rather as a formation of carboniferous age, with a liassic fauna, which has not however afforded a single palæozoic species.

Notwithstanding the concurrent evidence of so many observers to the fact of the intercalation of the anthracitic beds over a large area, with liassic fossils, there have not been wanting those who have endeavored to explain this association by supposed foldings, which may have involved the liassic beds with others belonging to an underlying carboniferous formation. This explanation, long since suggested by Voltz, was afterwards brought forward by Favre in 1841, since which time it has been discussed by various geologists, although generally with little favor. In

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the present memoir our author examines the section of Petit-Cœur, and finds on one side, intercalated between the beds with belemnites and those containing ferns, a mass of cargneule which he considers the representative of the triassic system, separating the carboniferous and liassic series; the cargneule has not however been detected on the other side of the supposed fold, where the belemnite beds are in direct contact with the anthracite series. It might farther be objected that the mass of cargneule, the usual associate of gypsum, is not alone sufficient proof of the existence of the triassic system, since, according to Gras, gypsums accompanied by yellow altered limestones (in one case declared to be dolomite,) occur at two distinct horizons in the anthracitic series.

The observations of Mortillet on the section of Petit-Cœur are important; he not only concludes from his examinations that it is impossible to admit the existence of a fold in the strata at this locality, but asserts that the belemnites which underlie the fossil plants are different from those above. In the lower beds there is found but a single species, the *Belemnites acutus* (Miller), which as well as an associated ammonite, *A. bisulcatus*, belongs to the lower lias. In the beds above the coal fossils, on the contrary, there are several species of belemnites, not readily identified, but differing in form from that just mentioned, and apparently belonging to the middle or upper lias. He farther cites the existence in the same specimen of a belemnite and a fossil fern. (*Bull. Soc. Geol. de France*, [2], x, 18.)*

The testimony of Sir Roderick Murchison, after a minute examination of the section at Petit Cœur, is also most decisive as to the impossibility of admitting any folding or inversion of the strata, and he confirms the opinion of Elie de Beaumont and Sismonda as to the liassic age of the coal flora in question. As regards the vertical range of certain fossil plants he refers to a species of *Calamites* which extends from the carboniferous series through the permian and trias, and an *Equisetum* which is common in the trias of Germany and yet abounds in the oolite of England. (*Geol. Journal*, v, 178.)

In the Alps of Lombardy, Omboni has described a succession of rocks similar to that given by Favre; beneath the lias, according to him, are red and green marls with limestones containing the fossils of the muschelkalk, underlaid by red and green sandstones, the whole being referred to the triassic system. Lower still a series of limestones, dolomites and sandstones underlaid by black shales, the whole without fossils, and resting upon crystalline schists and protogines, is referred to the permian and carboniferous formations. These sandstones he considers as identical with the verrucano, which according to him contains in Tuscany the fossils of the coal period. (*Bull. Soc. Geol. de France*, [2], xii, 517.) We have already seen that Sismonda regards the verrucano as belonging to the base of the lias, and although associated with the anomalous liassic flora, as overlying the true palæozoic coal measures.

* For fuller details see Gaudry's sketch of the various papers on this subject, *Bull. Soc. Geol. de France*, [2], xii, 580 and 637. Sismonda, Ibid., p. 631. Rozet, *Memoire sur les Alpes Françaises*, ibid., 204; Gras, *Sur le terrain anthracifère des Alpes, Ann. des Mines*, [5], v, 475, and d'Archiac *Histoire de la Géologie*, vii, 107–157. Also Lyell, *Geol. Journal*, 1849, p. xxxviii, and Portlock, ibid., 1856, p. lxvii.

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Connected with the view which supposes the continuation of a palæozoic flora up to the epoch of the lias is a question raised by our author (p. 29) regarding the association (established by comparison of considerable lists of species from various localities), of the fossils of the different stages of the lias in a single bed. Analogous cases are frequently met with at different geological horizons, showing that the causes which have limited the vertical range of certain animals are so far local, that under somewhat modified conditions the duration of a species may be prolonged after its disappearance from the adjacent seas. As our author has well said, it is in the continuation of similar chemical and physical conditions, generally dependant upon local accidents of the surface, that we must look for the cause of these apparent anomalies in the distribution of fossils. The investigations of Prof. Safford in Tennessee and Sir William Logan in Canada have made known a remarkable example of this in the Lower Silurian series of North America. In the State of New York the fauna of the Black River and Trenton limestones are almost totally distinct, so that the division between the two formations constitutes a well-marked palæontological horizon, while in Canada and in Tennessee the fossils of the two formations are so completely intermingled that it is impossible to distinguish between the Black River and Trenton limestones. (Billings, *Report Geol. Survey of Canada*, 1857, p. 152.) The genus *Catenipora* in North America also presents a remarkable instance of anomalous distribution; this coral throughout New York and Western Canada is Upper Silurian and is unknown below the Clinton Group, while at Lake St. John on the Saguenay it is abundant in the Black River limestone, a position which corresponds with its geological horizon in Europe.

The anthracitic system of the Alps has been described by Gras with great minuteness in the memoir cited above. He estimates the total thickness of the system at between 25,000 and 28,000 feet, and divides it into an upper and lower series, which are unconformable. The former with a thickness of about one-tenth of the whole, is referred to four subdivisions, and consists chiefly of argillo-calcareous shales terminated by a considerable mass of limestone, immediately beneath which occurs a bed of spilite, associated with gypsum and a yellow altered dolomite. The inferior division also consists in great part of similar shales, interstratified with gneissoid and talcose rocks to be noticed farther on; gypsum and a yellow altered limestone (dolomite?) also occur in this portion. In both the upper and lower divisions sandstones and shales are found with coal plants and layers of anthracite, the latter sometimes changed into graphite, while the associated calcareous shales contain in various localities, and at different horizons, belemnites and ammonites of liassic age.

Sismonda confirms the accuracy of the observations of Gras, and admits an upper anthracitic series, resting unconformably upon the lower, and of oolitic age, the lower only being referred by him to the lias proper, while Studer concludes the discussion of the subject as follows: "The intercalation of the lower anthracitic slates sometimes with gneissoid and talcose schists, and sometimes with the belemnitic shales, the layer of jurassic limestone which separates the lower from the upper anthracites, the extraordinary thickness of this calcareous layer and the anthracitic

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zone which covers it, the presence of the verrucano (or talco-quartzose conglomerate) between these two formations and the quantity of talc disseminated in all these rocks, present us problems which science is not able to solve completely."—(Studer, cited by Laugel, *Bull. Soc. Geol. de France*, [2], xii, 576.)

In the metamorphic rocks of the anthracitic system Gras distinguishes what he calls a protogine or talcose formation, consisting of granite, gneiss and mica slates, generally more or less talcose, containing also schistose diorites, eurites and leptynites. This protogine formation is intimately associated with the argillo-calcareous shales of the lower anthracitic series, which are found interstratified with and even passing into gneiss and other feldspathic schists. Gras concludes that this somewhat irregular interpenetration of the two classes of rocks is due to an irregular metamorphism, portions of the argillo-calcareous sediments having, as he supposes, been profoundly altered by emanations from below, and he hence regards the protogine formation as a portion of the inferior anthracitic series. Similar crystalline schists also occur in some parts of the upper anthracitic series, and in both the upper and lower there are found serpentines, euphotides, porphyries and spilites, all of which the author regards as of sedimentary origin, and as having undergone *in situ* a profound metamorphism which has often effaced the marks of stratification; this view he declares is the only one which appears to agree with the observed facts. This protest against the theory of the igneous origin of serpentines is in accordance with the results obtained by the Geological Survey of Canada; in his report for 1848 Sir William Logan insisted upon the sedimentary origin of the serpentines which occur in the altered palæozoic strata of the Green Mountains.

Rozet, in his investigations of the Alps, has referred to the liassic and jurassic periods the great system of gneiss, with micaceous and talcose schists, which makes up Mts. Cenis and Pelvoux, and a great part of the mountains of the frontier of Piedmont, while, according to Fournet, the jurassic rocks of the Valais have been altered in like manner. (Coquand, *des Roches*, pp. 300–301.)

Our author however supposes that all these altered strata are of carboniferous age, and remarks that the fossils in the limestone of the lower lias show but slight marks of metamorphism, from whence he concludes that "in this region of the Alps there are no highly metamorphosed jurassic rocks," and that the metamorphic action "which took place beneath the sea before the elevation of these mountains" ceased to be powerful after the deposition of the Valorsine conglomerate, (which he regards as of carboniferous age,) the paste of which is often converted into a crystalline talcose schist. At the same, although the liassic and so-called triassic rocks are comparatively unaltered, the jurassic shales are described as more or less talcose and greasy to the touch, but the alterations of these argillaceous rocks our author regards "as rather mechanical than chemical." (p. 76.)

The question before us is then, whether in that series of rocks which embraces liassic and jurassic beds with gypsum, dolomite, anthracite, and a carboniferous flora, and which geologists have generally referred to one great system not older than the lias, we have really in these mountains,

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as Favre supposes, the whole succession of formations from the oolite to the carboniferous inclusive, so involved by foldings and inversions that it has hitherto been impossible to determine their real structure.

But even if we admit with Favre the palæozoic age of the protogine formation described by Gras and Rozet, we cannot agree with him in limiting to the rocks of that period the action of the metamorphic process. The development of a talcose character in the jurassic shales we cannot regard as the result of a mechanical process, and we have besides evidences in the Alps of the metamorphism of still higher rocks.

As early as the year 1834 Keferstein had asserted that the granites of Mont Blanc are nothing more than altered strata of *flysch*, (*Naturgeschichte des Erdkörpers*, i, 286–292; *Bul. Geol. Soc. de France*, [1], vii, 198,) and in 1850 Sir Charles Lyell in his address before the Geological Society of London suggested that the protogines of the Alps might be of tertiary age. This is so far true that both Studer and Murchison have shown that portions of the eocene flysch have been converted into crystalline gneiss, mica slate and even granitic beds; Murchison, like Gras, whom we have already cited, remarks that the metamorphism seems irregular, some bands of the rock being apparently much more altered than others. (*Geol. Journal*, v, 164, 210.) The intercalation of wedge-shaped masses of fossiliferous limestone of liassic age among the gneissic strata of the Alps has been well described by Studer (*Bul. Soc. Geol. de France*, [2], iv, 208), and similar phenomena observed in various other metamorphic regions present a problem the right understanding of which is most important in its relations to the theory of metamorphism, and one which we propose to consider at an early day.

In conclusion we hasten to say, that although dissenting from some of the views of Mr. Favre, we are not less grateful for his very suggestive memoir, which with its carefully prepared lists of fossils and its beautiful sections is a valuable contribution to alpine geology.

T. Sterry Hunt.

2. *The Geological Structure of the "Jornada del Muerto," New Mexico, being an abstract from the Geological Report of the Expedition under Capt. John Pope, U. S. Top. Eng., for boring Artesian Wells along the line of the 32d parallel;* by G. G. Shumard, M.D., Geologist of the Expedition, (Trans. Acad. Sci. St. Louis, vol. i, No. 3, p. 341. 1859).—This paper gives a description of the geological structure of a district of country lying immediately east of the Rio Grande, and between the 32d and 34th parallels. The "Jornada del Muerto" is described as a gently sloping plane, of an elliptic form, from twelve to forty miles in breadth, and extending from near the southern extremity of the Doña Ana Mountains eighty or ninety miles in a N.N.W. direction. It is bounded on the east and west by ranges of mountains, varying in their elevation from two or three hundred to one thousand feet above the plane, and seem to be mainly composed of dark colored limestones of Upper Carboniferous age, dipping towards the interior of the intermediate plane; these rocks however, in the western range, were observed at some places, surmounted by shales and sandstones referred to the Cretaceous epoch. Igneous rocks form a belt of low hills along the eastern side of the eastern range, and also occur between the other and the Rio Grande. From the structure of

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the surrounding country, the "Jornada" is supposed to be a great synclinal depression, in which water could probably be obtained by artesian borings, through heavy detrital deposits.

The fossils found in the Carboniferous rocks here, and in the Organ Mountains, are all Upper Carboniferous types, many of them being identical with species almost everywhere common in the western Coal Measures. Those mentioned in the Cretaceous beds are *Inoceramus* and *Cardium*. The paper also contains interesting local details in regard to the igneous and metamorphic rocks of the several mountain ranges explored. M.

3. *Notice of Fossils from the Permian strata of Texas and New Mexico, obtained by the United States Expedition under Capt. John Pope, for boring Artesian Wells along the 32d parallel, with descriptions of new Species from these strata and the Coal Measures of that region;* by B. F. SHUMARD, M.D., (Trans. Acad. Sci. St. Louis, vol. i, Part 3d. 1859).—This is an important paper, containing descriptions of many new species, with an enumeration of others identified with forms known in the Kansas rocks, and of a few which are, by the author, supposed to be identical with foreign species; it is also illustrated by an excellent plate of twenty-seven figures by Leopold Gast & Brother of St. Louis.

Most of these fossils are from an extensive deposit of white limestone, and inferior beds of sandstone and darker colored limestone, in the Guadalupe Mountains, referred by Dr. S. to the Permian System. The new species described from these rocks are—

Campophyllum? Texanum, Chonetes Permiana, Spirifer Guadalupensis, Terebratula perinflata, Rhynchonella indentata, R. Texana, Camerophoria Swalloviana, Crania Permiana, Axinus securis, Turbo Guadalupensis, Pleurotomaria Halliana, and *Chemnitzia Swalloviana.* Those from the beds regarded as Carboniferous, are *Turbo Texanus, Straparollus cornudanus, Pleurotomaria Proutiana, P. obtusipira, P. perornata* and *Machrocheilus Texanus.*

All of which appear to be described with the author's well known care and accuracy.

Dr. S. had previously described from the beds he places in the Permian,

Phillipsia perannulata, Fusulina elongata, Productus Mexicanus, P. pileolus, P. Popei, Strophalosia (Aulosteges) Guadalupensis, Spirifer Mexicanus, S. sulciferus, Spiriferina Billingsii, Terebratula perinflata, Rhynchonella Guadalupæ, Camerophoria bisulcata, Retzia papilata, R. Meekiana, and *Myalina recta,*—

several of which are well illustrated in the plate accompanying the paper now under consideration. He also gives the following list of forms from these rocks, regarded by him, with more or less confidence, as identical with species occurring in the Permian and Upper Carboniferous beds of Kansas, viz:

Acanthocladia Americana, Productus Calhounianus, P. Norwoodii, Spirifer cameratus, Streptorhynchus (Orthisina) Shumardianus, Edmondia suborbiculata, and *Pleuraphorus occidentalis;* while he thinks he recognizes the following foreign species in the same association:—*Chætetes Mackrothii, Productus semireticulatus,* var. *antiquatus, P. Leplayi?, Terebratula elongata, Camerophoria Schlotheimi?, Myalina squamosa, Monotis Speluncaria,* and *Turbo helicinus?.*

As Dr. S. finds a *Phillipsia* and a *Fusulina,* in these rocks, neither of which genera are known to range up into Permian beds in the old world, and *Spirifer cameratus* is a characteristic Coal-measure species, from Pennsylvania to the Rocky Mountains; while *Productus semireticulatus,*

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is regarded by most authors as peculiar to the Carboniferous system—and a large proportion of the other species mentioned as common to the New Mexican and Kansas rocks, are known to occur in the latter territory in beds containing even a majority of well marked Coal-measure species, we may infer that in New Mexico, as in Kansas, there is a considerable blending of Carboniferous and Permian types; so that it becomes a matter of doubt and difficulty to determine at what particular horizon the line of demarkation should be drawn between these two Systems, if indeed there is any such natural break in our upper Palæozoic series of this country. It is to be hoped Dr. S. will continue his investigations of the fossils occurring in these formations, which he will doubtless have an opportunity to do, in connection with the geological survey of Texas, under his charge. M.

4. *Observations on the Geology of the County of Ste. Geneviève, being an extract from the Report made to the Missouri Geological Survey, in* 1859; by B. F. SHUMARD, M.D., (Trans. St. Louis Acad. Sci., vol. i, part 3, page 404, 1859.)—In addition to information respecting the iron and lead mines, building materials, &c., of the county, this extract contains some facts having an important bearing on mooted points in the classification of the Lower Carboniferous Series of the West. Dr. S. found this series to be composed of the following members, in the descending order:

1st. THE UPPER ARCHIMEDES LIMESTONE, characterized by *Pentremites pyriformis, P. sulcatus, Agassizocrinus dactyliformis, Spiriferina spinosa, Spirifer trigonalis*, and species of *Archimedipora*.

2d. THE FERRUGINOUS SANDSTONE, in which no fossils were found,—estimated thickness, 80 to 100 feet.

3d. THE STE. GENEVIEVE LIMESTONE, a second Archimedes bed, in which the following fossils were found:—*Rhynchonella trimela, R. Wortheni, Spirifera hirsuta, Retzia Marcyi, Spiriferina spinosa, Spirifera Leidyi, Productus elegans, P. bisulcatus, Murchisonia vermicula, Pentremites florealis*, and one or more species of *Archimedipora*.

4th. THE SAINT LOUIS LIMESTONE—containing *Lithostrotion mammillaria, Archæocidaris* and *Pentremites conoides*,—thickness 100 feet or more.

5th. THE THIRD ARCHIMEDES LIMESTONE, containing *Pentremites laterniformis, P. conoides, Archimedipora, Dichocrinus simplex, Spirigera hirsuta, Productus Indianensis, Rhynchonella subcuneata*, and *Holopea Prouti*,—thickness from 100 to 150 feet.

6th. THE ENCRINITAL LIMESTONE (= BURLINGTON LIMESTONE), with its usual fossils—being the lowest member of the great Carboniferous series.

Of the Devonian rocks he recognizes the CHEMUNG GROUP, HAMILTON GROUP, and the ORISKANY SANDSTONE. Of the Silurian—1st. LOWER HELDERBERG SERIES, 2d. The NIAGARA GROUP, 3d. The HUDSON RIVER GROUP, 4th. The RECEPTACULITE LIMESTONE, 5th. The TRENTON LIMESTONE, 6th. The BLACKRIVER and BIRDSEYE LIMESTONES. Then comes the five members of the GREAT MAGNESIAN LIMESTONE SERIES, which represents the Calciferous, and possibly portions of the Potsdam and Chazy Limestones of New York; and last of all, eruptive rocks.

Of the Coal measures, only thin outliers cap the hills half a mile above St. Mary's on the Mississippi. The beds are alternations of shale and sandstone, surmounted by a thin bed of hard siliceous limestone. M.

5. *Third Series of Descriptions of Bryozoa, from the Palæozoic Rocks of the Western States and Territories;* by H. A. PROUT, (Trans. Acad. Sci. St. Louis, vol. i, part 3, p. 443, 1859.)—Every geologist who has

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worked amongst western rocks, must have regretted that Palæontologists have generally given so little attention to the remains of *Polyzoa* characterizing these formations. These delicate forms of life existed in great profusion during portions of the Palæozoic era, especially during the deposition of some of the lower members of the great Carboniferous series, and are often met with in a good state of preservation, where no other organic remains are to be seen. Consequently when accurately classified, and the species and genera are fully described and illustrated,—due care being taken to determine the exact geological position of each,—they will at once become an important guide in the identification of strata.

The task of classifying, describing, and illustrating these remains occurring in the western rocks, has been undertaken by Dr. Prout of St. Louis, who has produced several valuable papers on this subject, previous to the publication of that now under review. His last paper, mentioned at the head of this notice, contains full descriptions of nine new species, and two new genera, with four beautiful plates illustrating these and some of the species described in his former papers. These plates are engraved on stone by Leopold Gast and Brother, of St. Louis, from drawings by Dr. Prout and Mr. Gast, and bear evidences of skill and accuracy. The new genera described in this paper are *Semicoscinium* and *Septopora*, and the new species, *Semicoscinium rhomboideum*, *Fenestella hemitrypa*, *F. banyana*, *Limaria falcata*, *Flustra spatula*, *F. tuberculata*, *Septopora Cestriensis*, and *Polypora tuberculata*.

Dr. S. also thinks he has identified a Permian species, *Polypora biamica* of Keyserling, in the Upper Archimedes Limestone, a member of the Lower Carboniferous Series,—at any rate no essential differences were observed in the specimens compared. It is probable however, that when better specimens are obtained, showing all the characters of this Lower Carboniferous form, it will prove distinct from the Permian species. M.

III. BOTANY AND ZOOLOGY.

1. *Collections of Cuban Plants.*—Mr. Charles Wright revisited the eastern part of Cuba in the autumn of the year 1858, where he still remains, engaged in botanical explorations in that little-known region. His collections of dried plants, up to last autumn, have already been received; and the Ferns, which form a large and very attractive part of them, have been distributed into sets. A number of these sets, not yet appropriated, are offered for sale. The fullest of these sets contain about 180, the smallest 120 species, which may be increased by further collections to a moderate extent. Sets can be obtained from Professor Gray, Cambridge, at $10 per hundred specimens. Of phænogamous plants, soon to be distributed, a very few sets are still open to subscribers, at the same price. It is expected that the species will be named very soon. The names of the ferns are about to be published by Mr. Eaton of New Haven, our principal Pteridologist. The rich collections in the lower *Cryptogamia*, made by Mr. Wright in his former visit to Cuba, along with those of the present exploration, are now in course of study, the *Musci* and *Hepaticæ* by Mr. Sullivant, the *Lichenes* by Prof. Tuckerman, the *Fungi* by the Rev. Dr. Curtis, preparatory to their distribution in

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named sets. Those who desire to secure full sets should make early application to Prof. Gray, who will have them in charge during Mr. Wright's absence. A. G.

2. *Systematic Arrangement of the Species of the Genus Cuscuta, with critical remarks on old species, and descriptions of new ones;* by GEORGE ENGELMANN, M.D. (Extr. from Trans. Acad. Science of St. Louis, vol. i, pp. 453–523; separate issue pp. 73,) 8vo, 1859.—It is well known to botanists that Dr. Engelmann has for many years been making a special study of *Cuscuta*, and that, besides his own and other American collections, those of the principal European herbaria have been rendered to him for examination. His recent visit to Europe enabled him to extend and to revise his study of this genus. The study of the *Cactaceæ* of North America, which so long interrupted the former investigation, having been brought to a conclusion, Dr. Engelmann has at length been able to publish his revision of the whole genus *Cuscuta*. The thirty-eight species more or less known to Choisy when he elaborated this genus for the Prodromus are doubled in the present enumeration, although an equal number, including several of Choisy's, and of his own formerly proposed, are reduced to synonyms, or arranged as varieties. Many species prove to be remarkably polymorphous, and require an array of varieties and subvarieties to express their manifold diversities in systematic form. The work has involved immense labor, and bears the marks throughout of the most patient and conscientious treatment. Sound views prevail in the generalities as well as in the details; the Linnæan genus is preserved intact, but disposed in three primary groups or subgenera,—*Cuscuta* proper, *Grammica*, and *Monogyna*, and these in nine sections. The species of the first group are all indigenous to the Old World; those of the second are mostly American and Eastern Asian; of the third principally Asian, but two species extend into Europe, one is Texan, and one South African. A. G.

3. *On the Distribution of the Forests and Trees of North America, with Notes on its Physical Geography;* by J. G. COOPER, M.D. pp. 40, 8vo. Extr. from the Report of the Smithsonian Institution for 1858.—The tables illustrating the geographical distribution of our trees and larger shrubs bring together and systematize a great amount of valuable information. As a first essay, it appears to be all that could be expected; and the author himself, having taken up the subject with great zeal and good opportunities, will doubtless perfect it in the future reports for which he is collecting materials. Since the catalogue purports to be one of the *trees* of the United States, we should not have included shrubs, except those of the largest class, and a few which belong to genera characteristically arboreous, such as *Quercus*, *Acer*, &c. Such depressed shrubs as *Prunus maritima* and *Cerasus pumila* are surely out of place. The incongruity is all the more serious in the catalogue of the trees of the regions of the Rocky Mountains and westward, which regions being "comparatively poor in trees," the standard is there still farther reduced, "since shrubs become more valuable where trees diminish in number." This confusion of economical with scientific considerations has the effect of representing the western part of the continent to be far richer in trees than it really is. Of the 108 species in this list only a moiety would

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seem to make good their claim to be called forest trees, or trees at all. And in this connection we venture to suggest that the *minimum* as well as the *maximum* height which the trees attain should be given. The *average* height is the more important to be known; the maximum is rather a matter of curiosity. "The reason for giving the maximum heights [only] is, that it is thought the cultivation of trees will become some day a matter of national interest, and I wish to show what they are under the best natural circumstances, supposing that, with cultivation, they will at least equal this standard." We do not suppose so. Under the *same circumstances*, the Lambert-pines of future ages might indeed aspire to 300 feet in height, and the giant *Sequoia* to 450 feet; but planted trees, with room to spread as they should, are never drawn up as in primæval forests.

The delineation of *natural provinces* and *regions* in North America, according to the distribution of our arborescent vegetation, opens wide questions upon which we must not here enter. We should incline to broader views and fewer subdivisions, as preferable for exhibiting the general facts of the case, and more likely to be stable.

The Floridian region is said to have "about thirty-two characteristic and thirteen peculiar trees," and accordingly would appear to be far more strongly marked than any other of the nine. The erroneous impression which this may give would be removed by an expansion of the statement that "Florida appears rather to belong to the West Indian province." Nearly all these peculiar or characteristic trees belong to the Keys, and are Bahaman or West Indian species. Probably no other part of North America is really so destitute of peculiar species of plants as southern and eastern Florida.

In conclusion, while we heartily thank Dr. Cooper for this interesting and useful essay, and expect still better results from his continued investigations, we simply enter a protest against the anachronism of appending an early author's name to a species under a genus which he never heard of, and against the bad taste of writing the names of persons without a capital initial,—both innovations from which botanical writings have thus far been nearly free. A. G.

Zoological Notices.—

1. Prof. J. Victor Carus, of Leipsic, writes to the Smithsonian Institution, July 27, 1859, as follows: "During the last two years, I have been collecting materials towards a general catalogue of zoological literature, from 1750 up to the present day, including not only all the separately published works, books and pamphlets, (most of which are to be found in Engelmann's Bibliotheca Historico-Naturalis,) but also, and especially, all the papers, articles and notices contained in periodicals, the number of which is increasing every year. All the titles and references will be arranged systematically, not according to the alphabet of the authors, but within the classes and groups according to the alphabet of the genera; so that at a glance one will find the whole literature of any particular genus." Prof. Carus calls for aid in obtaining access to American scientific papers, many of which are found in periodicals which are difficult to obtain, as he wishes to make his work as nearly complete as possible. Any who wish to further his laudable object would confer a favor by forward-

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ing to him synopses of the contents of the more obscure publications, copies of minor papers, etc.

2. *Die Klassen und Ordnungen des Thier-Reichs, wissenschaftlich dargestellt in Wort und Bild.* Von Dr. H. G. Bronn, Prof. an der Univ. Heidelberg. Leipsic and Heidelberg; C. F. Winter, 1859. 8vo. with plates.—Of this excellent work now in the course of publication, five parts have appeared, containing the Amorphozoa and part of the Actinozoa. The subject is treated in a masterly manner, and the work will undoubtedly be far more complete in its character than any general treatise on Zoology yet published. It commences with an introduction, giving a short general account of the nature of the Animal Kingdom, a history of its investigation by zoologists, and a tabular view of the characteristics of the five subkingdoms, (which are named Amorphozoa, Actinozoa, Malacozoa, and Entomozoa and Spondylozoa,) in regard to their radical form, radical number, embryonic development with reference to the "Primitive-Theil" and position of the yolk, organic system, the head, nervous system, structure of skeleton, teeth, circulatory system, and blood.

In the body of the work each class is treated of under the following heads: I. Introduction, an account of the name and literary history of the class; II. Organic structure; III. Chemical composition; IV. Vitality, and Embryology or Development; V. Classification; VI. Geographical and Topographical Distribution; VII. Geological evolution; and VIII. Signification in the Economy of Nature. In the *Introduction* a full bibliography is given for each class. Under the head of *Classification*, a systematic review or synopsis is given of the sub-classes, orders, suborders, families, sub-families, relationships, (sippschaft) and genera, (sippen); more than the usual number of subdivisions being introduced. In this synopsis, such characters only are given as are necessary to discriminate between known genera, but the work is carefully brought up to the present state of the science in a more thorough manner than has been attempted before in any general work. Some useful abbreviations are introduced which will tend to a convenient shortening of descriptions, although it would have been perhaps better to have formed them from Latin rather than German words. (See p. 44, Vol. II.) The plates are well done, and illustrate not only the structure and development, but a considerable number of the generic forms of each class. The wood-cuts are less remarkable for artistic excellence. W. S.

3. *Description of Oceania (Turritopsis) nutricula, n. s. and the embryological history of a singular Medusan Larva found in the cavity of its bell;* by Prof. John McCrady, (Proc. Elliott Soc. Nat. Hist. of Charleston, S. C., vol. i, p. 55–90); and *The Gymnophthalmata of Charleston Harbor;* by the same author. (Ibid., i, p. 103–221. Plates 4–12.)—These articles, particularly the latter, form the most valuable contribution to the history of our American *Hydroidea* that has appeared since Agassiz' papers in the Memoirs of the American Academy. Like the latter, they are written in that easy, interesting style which is so much more agreeable to the reader than the dry description of details which constitutes the bulk of biological writings. The author has put forth some new views, and enlarged upon the suggestions of his predecessors, in a manner well worthy of the attention of scientific zoologists.

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Prof. McCrady states that there is no essential difference between the so-called alternation of generations and a regular metamorphosis, which he calls a *homogony*, after Gegenbaur. The hydroid larvæ therefore of the Gymnophthalmata should no longer be separately named and described, but their history should be included in that of the species to which they belong, as is done in the case of Insects. For the group the name *Hydroidea* is retained, following the rule of priority, *Cryptocarpæ* of Eschscholtz being rejected on account of its erroneous significance. In subdividing the group the author seizes upon an excellent character, founded upon the mode of growth in the medusa-buds, which has been overlooked by previous investigators. In *Tubularia*, *Coryne*, etc., the outer covering of the bud becomes the disc of the medusa, and the digestive tube is enclosed from the first. In *Campanularia* and its allies, on the contrary, the digestive tube projects at first freely from the bud, and becomes afterward enclosed by the over-arching disc, which grows outward from its base. Upon this ground the Hydroidea are separated into two sub-orders—*Endostomata* and *Exostomata*. This subdivision is confirmed by characters derived from the full-grown Medusæ:—in the first group these are deeply bell-shaped, sometimes even sub-cylindrical, with no sinuses in the radiating tubes, and a long digestive cylinder pendant from the disc; in the second they are generally broad and shallow, or saucer-shaped, with many sinuses in the radiating tubes, and a short digestive cavity imbedded in the disc. In the former group are included the Corynidæ, Velellidæ, Tubularidæ and Siphonophoræ. Recent investigations have shown that the Velellidæ and Siphonophoræ are true Hydroidea and the free medusa (*Chrysomitra*) of Velella has been observed and described by Huxley, Vogt, Kolliker and others. The development of the medusa-buds in these two families is after the manner of the Endostomata. In the latter group, Exostomata, the author includes the Campanularidæ, Sertularidæ and Æginidæ, with several genera the family connections of which are uncertain.

In the description of the Hydroidea of Charleston Harbor, Prof. McCrady has more than doubled the number of species known to exist on our shores. He describes 32 species belonging to 30 genera, 8 of which are new, as follows;—among the Endostomata, *Turritopsis*, *Corynitis*, and *Dipurena*; among Exostomata, *Encheilota*, *Entima*, *Epenthesis*, *Phortis*, and *Persa*. In the genus *Nemopis* the author has failed to discover the ocelli described by Agassiz as existing in the tips of the upturned tentacles, and says that the darker hue of these tips is occasioned by their greater thickness. The *Globiceps tiarella* of Ayres (*Eucoryne elegans*, Leidy) is found in Charleston Harbor, and is placed in the genus *Pennaria* by Prof. McCrady, who describes its medusa. The parasitic medusa found in the bell of *Turritopsis nutricula*, and described in the first paper as the young of that species, is afterward considered to be the young of a *Cunina*. The paper closes with a discussion of the geographical distribution of the American Hydroidea. W. S.

4. *On the zoological affinities of Graptolites*; by Prof. JOHN MCCRADY. (Proc. Elliott Soc. Nat. Hist., i, p. 229.)—These paradoxical fossils are regarded by Prof. McCrady as similar to the toothed rods of the larvæ of Echinoderms, described by Joh. Muller. The great discrepancy in size

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he explains by suggesting that these embryo-like forms were fully developed animals characteristic of the early geological period at which they existed. W. S.

5. *Letters from Alabama, chiefly relating to Natural History;* by PHILIP HENRY GOSSE, F.R.S. London, 1859, pp. 306: small 8vo.—Another popular scientific work of this prolific writer. It consists of a series of letters written many years ago, when the author was teaching school in the interior of Alabama. In his usual interesting style, Mr. Gosse describes the habits and points out the beauties of many of our Southern plants, insects, reptiles and birds, with numerous illustrations on wood. W. S.

6. *Sketch of a revision of the genera of Mithracidæ;* by WM. STIMPSON.—The old genus *Mithrax* was divided into three groups by Milne-Edwards, (Hist. Nat. des Crust., i. 318), *Mithrax triangulaires*, *M. transversaux*, and *M. déprimés*. These groups are now considered of generic value. For the first group De Haan has proposed the name *Dione*, which cannot however be adopted, as it was previously applied to a genus of Lepidoptera, by Hübner. It is also used for a bivalve shell. To the third group White gave the name *Mithraculus*, which was adopted by Dana.

In the Proceedings of the Zoological Society of London, 1847, p. 222, Mr. Adam White gives a wood-cut of a maioid crustacean, to which he applies the name *Schizophrys*, with the following description: "Carapax oval, depressed, somewhat attenuated behind; beak deeply cloven; upper orbit deeply cloven, with a strong tooth in the middle of the cleft; under orbit an elongated appendage on the inside, with two teeth at the end. Tale of male with 7 joints, the sides nearly parallel. Fore-legs shortest. Fingers without teeth." This genus, in the catalogue of the British Museum, is placed between *Othonia* and *Pericera*. But if we consider that by the "elongated appendage on the inside" of the orbit, Mr. White probably means the basal joint of the external antennæ, and observe how closely the figure corresponds to young crabs belonging to De Haan's genus *Dione*, the conclusion is unavoidable that *Schizophrys* is really synonymous with the *Mithrax triangulaires* of Milne-Edwards, and as such we here adopt it. In the following synopsis new characters are introduced; some new genera are described, and a list given of the species known up to the present date.

A. Maxillipedis externi merus margine antico integer.

MITHRACULUS, White. (*Mithrax*, pro parte, De Haan, Fauna Jap. Cr. 82.) Carapax depressus, rostro brevissimo vel nullo.—*M. sculptus*, (M.-Edw.,) *M. nodosus*, (Bell,) *M. denticulatus*, (Bell,) *M. coronatus*, (Herbst.) *M. cinctimanus*, Stm.

B. Maxillipedis externi merus angulo antero-interno excisus, ad palpum incipiendum.

a. Frons angusta. Orbitæ parvæ, profundæ, oculos pæne operientes, MITHRAX, Leach. Carapax plus minusve transversus. Orbita multifissa, margine dentibus vel tuberculis armata.

1. Antennæ externæ articulus basalis spinis tribus armatus.—*M. verrucosus*, M.-Edw., *M. aculeatus*, (Herbst.)

2. Antennæ externæ articulus basalis spinis duabus armatus.—*M. spinossissimus*, (Lamk.), *M. hispidus*, (Herbst,) *M. ursus*, Bell, *M. rostratus*,

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Bell, *M. pygmæus*, Bell, *M. cornutus*, De S., *M. armatus*, De S., *M. tuberculatus*, Stm.

Teleophrys, nov. gen. Carapax antice triangularis, postice et lateraliter rotundatus, sulco cervicali sat profundo. Orbita margine supero externoque integra, nec dentata.—*T. cristulipes*, Stm.

b. Frons lata. Oculi majores. Orbitæ grandes, non profundæ, late fissæ, oculos non operientes.

Schizophrys, White. (*Dione*, De Haan.) Carapax ovato-triangularis. Rostrum longum, bifidum, cornibus bidentatis.—*S. dichotoma*, (Latr.) *S. affinis*, (De Haan,) *S. aspera*, (M.-Edw.), *S. serrata*, White, *S. spinigera*, White, *S. dama*, (Herbst.)

Cyclomaia, nov. gen. Carapax orbiculatus, antrorsum quam retrorsum vix angustior. Rostrum brevissimum, bifidum, cornibus acutis, non dentatis. Oculi grandes, sat breves. Antennæ externæ articulus basalis trispinosus, spinis superne conspicuis. Maxillipidis externi merus apice interno profunde sinuatus. Pedes mediocres.—*C. suborbicularis*, Stm.

Cyclax, Dana. *Cyclomaiæ* carapacem affinis. Oculi longi; pedes longi, tenues.—*C. Perryi*, Dana.

Mithraculus, *Mithrax*, and *Teleophrys* are American; *Schizophrys*, *Cyclomaia* and *Cyclax*, old-world types.

7. *Archiv für Naturgeschichte*, vol. xxiv, for 1858; Berlin, 1859:—contains the following articles of special interest:—Anatomy and development of Copepoda, with 2 plates; *Claus.*—Deseriptions of new Chilian Vertebrates by *Dr. Philippi.*—Revision of the Gadidæ, Soleinæ, and Plagusinæ; *Kaup.*—(Mr. Kaup considers our *Morrhua pruinosa* and *M. americana* as one species and identical with *M. vulgaris!*)—Geographical and historical remarks on certain mammals; *Martens.*—On the species of Velutina; *Martens.*—On Annelides of the Brazilian Coast, 2 plates; *F. Müller.*—Enthelminthica, 2 plates; *Wagener.*—Anatomy and Histology of some Trematodes; *Walter.*—New Batrachians, *Günther.*—On the hard-cheeked Acanthopterygians; *Kaup.* (The number of genera very much reduced.)—Critical remarks on Castelnau's Siluroids; *Kner.*

W. S.

8. *The Natural History Review and Quarterly Journal of Science*, a periodical published at London and Dublin, containing reviews of works relating to Natural History, and also the proceedings of the Dublin Scientific Associations, as follows:—the Geological Society, the Natural History Society, the University Zoological and Botanical Association, the Royal Irish Academy, and the Royal Society. In giving a synopsis of the more important zoological papers read before these five Associations, we shall, for the sake of convenience, cite the "Quarterly Journal" instead of their own regular publications:—

Vol. v. 1858, contains—p, 134, On new genera and species of Polyzoa, 4 plates; *W. Thompson.*—p. 148, Cambrian fossils, Histioderma, n. g. (annelide); *Kinahan.*—p. 168, Steropis, a new genus of Carboniferous Crustacea, allied to Limulus; *Baily.*—p. 194, On some Oniscoidea, with a plate, *Kinahan.*—p. 202, New forms of Diastylidæ (wood-cuts); *Bate.*—p. 207, On Oldhamia, a Cambrian fossil; *Kinahan.*—p. 276, Ancient and modern races of Oxen in Ireland, (wood-cuts); *Wilde.*—Vol. vi., 1859, p. 108, On the urticating organs of Actinia; *M'Donnell.*—p. 113, Irish

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—p. 191, Crimean Fossils; *Bailey.*—p. 199, New Irish Orthocerata; Actiniadæ; *Wright.*—p. 125, Platyarthrus, Brandt, and allied genera: *Kinahan.*—p. 152, Anatomy of the brain in some small Quadrupeds, 2 plates; *Garner.*—p. 161, Subterranean Gammaridæ; *Bate* and *Hogan. Haughton.*—p. 237, Morphology of the Hydrozoa, with reference to the constitution of the sub-kingdom Cœlenterata; *Greene.* (Mr. Greene includes the Lucernaridæ with Hydrozoa, and considers the Medusidæ—"Animal consisting of a polype suspended from the under surface of a natatorial organ"—an order distinct from the Hydridæ and Tubularidæ !)

9. *An essay on Classification*, by Louis Agassiz.—This work, forming the introduction to Prof. Agassiz' Contributions to the Natural History of the United States, has been published in a separate form, a convenient octavo, by the Longmans and Trübner & Co., London.

10. *On the genus Synapta;* by Woodward and Barrett, (Proc. Zool. Soc. of London, xxvi, 360. Plate xiv).—A short historical and anatomical account of the interesting family of Holothurians in whose skin are found the miniature anchors and wheels, which form such elegant objects for the microscope. They give detailed descriptions of *Synapta digitata* and *S. inhærens*, and add one new species, *S. bidentata*, from China. They also give what purports to be a list of the known species of the family, to which, however, the following should be added:—*Synapta oceanica* Less., *S. doreyana* Quoy and Gaimard, *S. punctulata* Q. & G., *S. bachei* Pourt., *S. tennis* Ayres, *S. pellucida* Ayres, *S. dolabrifera* Stm., *Chirodota fusca* Q. & G., *C. rubeola* Q. & G., *C. tenuis*, Q. & G., *C. rufescens* Brandt, *C. rotifera* Pourt., *C. pallida* Ayres, *C. australiana* Stm., *C.* (*Myriotrochus*) *brevis* Huxley. W. S.

Proceedings of the Elliott Society of Natural History of Charleston, S. C.—Vol. I. 1856.—p. 50, On Cicadæ; *J. Lee.*—New and rare Phænogamous Plants found in the State of South Carolina; *H. W. Ravenel.*—p. 55, Description of Oceania (Turritopsis) nutricula, nov. sp., and the embryology of a Medusan Larva found in its bell; with four plates; *J. McCrady.*—1857, p. 91, On the past and present condition of Niagara Falls; *L. R. Gibbes.*—p. 101, Notice of an ore of Argentiferous Galena; *Frampton.*—p. 102, On the fruit of Yucca gloriosa; *L. R. Gibbes.*—p. 103, Gymnophthalmata of Charleston Harbor, with five plates, (noticed in this Volume, p. 130); *J. McCrady.*—p. 222, On Specific Form; *J. McCrady.*—p. 223, On a Bolina found in Charleston Harbor; *J. McCrady.*—p. 225, Description of Ranilia muricata, M. Edw., with a plate; *L. R. Gibbes.*—p. 229, On the zoological affinities of Graptolites (noticed in this volume, p. 131); *J. McCrady.*—p. 237, Gigantic Orthoceras from Minnesota; *L. A. Frampton.*—p. 238, On a Cactus from Eding's Bay, S. C.; *L. R. Gibbes.*—p. 239, Medusæ of Port Royal Harbor, S. C.; *J. McCrady.*—p. 241, Botany of Eding's Bay; *L. R. Gibbes.*—p. 251, Preparation of Metallic Cobalt; *W. Sharswood.*—p. 254, Development of two species of Ctenophora found in Charleston Harbor, with a plate; *J. McCrady.*—1858, p. 272, Cacti of S. Carolina; *L. R. Gibbes.*—p. 275, Instance of incomplete longitudinal fission in Actinia cavernosa; *J. McCrady.*—p. 278, A new locality for Rutile; *W. Sharswood.*—p. 280, Three new Univalves; *E. Ravenel.*—p. 282, New genus of fossil Echini, Ravenelia, allied to Pygorynchus; *J. McCrady.*—p. 287, Antidote for Arsenious Acid; *W. Sharswood.*—p. 288, Phenomena of the Earthquake of Dec. 19, 1857; *L. R. Gibbes.*—p. 291, On a convenient form of Aspirator; *L. R. Gibbes.*

Proceedings Boston Soc. Nat. Hist., 1859.—p. 49, On the priority of discovery of the fossil footmarks of the Connecticut Valley; *T. T. Bouvé.*—p. 54, Trilobite from Newfoundland; *C. T. Jackson.*—p. 58, Japanese plant-wax; *W. B. Rogers* and *C. T. Jackson.*—p. 60, On the Infusorial deposit in the Tertiary of Virginia and Maryland; *W. B. Rogers.*—p. 64, Mineral resources of the Rocky Mountain Chain;

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W. P. Blake.—p. 74, On the frozen well of Brandon, Vt.; *C. Stodder.*—p. 75, Paradoxides Harlani, *C. T. Jackson.*—Habits of marine animals observed at West Yarmouth, Mass.; *T. Lyman.*—Diatomaceæ from Milwaukee; *A. M. Edwards.*—p. 81, Report on the frozen well at Brandon, Vt.; *Jackson* and *Blake.*—p. 89, On collecting, preparing and mounting Diatomaceæ; *A. M. Edwards.*—p. 102, A list of Birds seen at the Bahamas, from Jan. 20th to May 14th, 1859, with descriptions of new or little known species; *H. Bryant.*

Proceedings Philadelphia Acad. Nat. Sci., 1859.—p. 162, On fossil teeth and bones collected by Prof. Emmons: Ontocetus Emmonsi, nov. sp., founded on the tooth of a Cetacean; *J. Leidy.*—Notice of Humboldt; *I. Lea.*—p. 164, On a specimen of Hydaticus zonatus; *C. A. Helmuth.*—p. 165, Description of new generic types of Cottoids from the Collection of the North Pacific Expedition; *T. Gill.*—p. 167, Description of a new species of Callianidea; *T. Gill.*—p. 168, Entomacrodus, a new genus of Salarianæ; *T. Gill.*—p. 169, Herpetological Notices; *C. Girard.*—p. 170, Twelve new Uniones from Georgia; *I. Lea.*—Catalogue of Birds collected on the Rivers Camma and Ogobai, W. Africa, by Mr. P. B. Duchaillu, with notes and descriptions of new species; *J. Cassin.*—p. 177, On fresh-water shells, *I. Lea.*—Fossils from the Post-Pliocene of S. C., with Dr. Leidy's paper on the fossil Horse, and Prof. Agassiz' letter; *Holmes.*—p. 187, Four new exotic Unionidæ; *I. Lea.*—p. 188, Notes on American land-shells, No. 5; *W. G. Binney.*—p. 189, Catalogue of birds collected in the vicinity of Fort Tejon, Cal., with description of a new Syrnium; *J. Xantus.*—p. 194, Freyia Americana, a new animalcule from Newport Harbor; *J. Leidy.*—p. 195, Evorthodus, a new genus of Gobioids; *T. Gill.*—p. 196, Pimeletropis, a new genus of Siluroids from South America; *T. Gill.*—p. 197, New genera and species of N. American Tipulidæ with short palpi, with an attempt at a new classification of the tribe, (pp. 59 and three plates); *R. von Osten Sacken.*—p. 255, A spider catches a fish; *E. A. Spring.*—p. 256, Contributions to American Lepidopterology; *B. Clemens.*—p. 262, On a deformed fragmentary Human Skull from Jerusalem; *J. A. Meigs.* Supplement—Catalogue of the Invertebrate Fossils of the Cretaceous Formation of the United States; *W. M. Gabb.*

Journal Philadelphia Acad. Nat. Sci., Vol. VI, Pt. II.—Contains the following papers:—Synopsis of the North American Sphingidæ, by B. Clemens, M.D.; and New Unionidæ of the United States, by I. Lea, LL.D.

Canadian Naturalist and Geologist, Oct. 1859.—A new Gasterosteus, G. gymnetes; *Dawson.*—Glacial Phenomena of Canada and the North-eastern United States during the Drift Period; *Ramsey.*—On Ozone; *Smallwood.*—Fossils of the Calciferous Sandrock, etc.; *Billings.*—New Trilobites; *Billings.*—On the Aurora of 28th of August; *Smallwood.*

Annals and Magazine of Natural History, London, Oct. 1859.—Cellulose in starch-grains; *H. von Mohl.*—New spiders from Madeira; *J. Blackwall.*—Nudibranchiate mollusks of Ceylon; *Kelaart.*—New genera and species of phytophagous insects; *Baly.*—Digestive power in the Actiniæ; *Holdsworth.*—New Entomostraca from Jerusalem; two plates; *Baird.*—N. American Fungi; *Berkeley* and *Curtis.*—A new antelope (Kobus), from Central Africa; *Gray.*—Nov. 1859.—Reproduction of Bark-lice, with a plate; *R. Leuckart.*—New Anthribidæ; *Pascoe.*—Nomenclature of the Foraminifera; *Parker* and *Jones.*—Coleoptera of Old Calabar; *Murray.*—On certain genera of plants; *Miers.*—On Hydroid Zoophytes; *Allman.*—Ceylon insects; *F. Walker.*—A new Catharus from Western Mexico; *Sclater.*

Proceedings of the Zoological Society of London.—The volume for 1858 contains the following papers of more or less interest to American zoologists:—p. 38, Genera of Olivæ; *J. E. Gray.*—p. 90, On Stavelia, a new genus of Mytilidæ, and on some Distorted Bivalves, with a plate; *J. E. Gray.*—p. 92, Nerita and its operculum; *J. E. Gray.*—p. 145, Rearrangement of the genera of British Actiniadæ; *W. Thompson.*—On Sponges, by Dr. *Gray*, pages 113, 114, 229, and 531, with plates.—p. 136, Separation of the Salamandridæ into two families, by the form of the skull, *J. E. Gray.*—p. 225, Description of new Pinnæ; *S. Hanley.* (The South Carolinian Pinnæ called by American Conchologists P. seminuda and P. muricata are P. carolinensis, Hanl. and P. squamosissima, Phil.)—p. 339, Systematic Arrangement of the Tail-less Batrachians; *Gunther.*—p. 360, On the genus Synapta; *Woodward & Barrett.*—p. 373, Geographical Distribution of Reptiles; *Gunther.*—p. 413, Monograph of the Cæcidæ; *P. P. Carpenter.*—Also many papers relating to Central American Ornithology, by *P. L. Sclater.* W. S.

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IV. ASTRONOMY AND METEOROLOGY.

1. *Discovery of the 57th planetoid (Mnemosyne).*—Another planet, appearing like a star of the 10th magnitude, supposed to be one of the group between Mars and Jupiter, was discovered by *M. Robert Luther*, Sept. 22, 1859, at the Observatory of Bilk. It is the 57th of the group, and has been named *Mnemosyne.*—(*Comptes Rendus*, Oct. 5, 1859.)

2. *Total Solar Eclipse of July* 18, 1860.—M. Faye has called the attention of astronomers and of all lovers of astronomy to the rare opportunity for important observations presented by this eclipse, which will traverse the earth from California to the Red Sea. The total darkness will travel across North America about the 60th degree of North latitude, leaving it at Hudson's Straits, and leaping the Atlantic, pass across Spain, strike the Balearic Isles, pass through Algeria, and crossing the Nile north of Dongola, take leave in Ethiopia. He names seven stations as specially favorable for observation, viz., 1. In Oregon between the Pacific ocean and the Rocky Mountains. 2. In Labrador, in lat. 59° N. 3 and 4. In Spain on the Atlantic and on the Mediterranean coasts. 5. At Ivica in the Balearic isles. 6. At Kabylia in Algeria. 7. At Dongola on the Nile.

At the time of the eclipse, Venus, Mercury, Jupiter and Saturn, will be in the vicinity of the Sun, and form a sort of rhomboid about it. Such a spectacle will not be visible again for many ages.

The objects to be secured by these observations may be arranged under four heads. 1. The more exact determination of the errors of the lunar tables. 2. The determination of the longitudes of places too remote from each other to be connected by the electric telegraph. 3. The verification of the present data for the solar and lunar parallax and the flattening of the earth. 4. The solution of certain questions respecting the physical constitution of the sun, and of the space in its vicinity.

M. Faye proposes that at the two principal stations photographic methods should be substituted in place of direct observation. A telescope of large object glass and long focus should be used, and a large number of proofs should be taken between the first and last contact, taking care to keep horizontal the collodionized plate. During the total obscuration, the whole object glass should be uncovered, and the most sensitive plates employed in order to obtain proofs on a large scale of the aureola and solar flames, while observers provided with hand telescopes, with fresh eyes, should deliberately study all particulars which photography can not secure.

As to the meteorological phenomena, M. Faye proposes to add the sympiezometer as more quick to show the rapid fluctuations of the atmosphere; and instead of the common thermometer to use a self-registering Breguet's metallic thermometer carried into the air by a captive balloon. The variations of the magnet should also be observed, for if the earth's magnetism is affected by the spots which periodically obscure part of the sun's disk, may it not be affected by the more rapid obscuration of the same by the moon? Possibly the wires of the electric telegraph, arranged now with and now against the direction of the eclipse may show perturbations too fugitive to be detected by bar magnets.

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The station at Ivica seems to combine all the advantages offered by the peak of Teneriffe. Here especial attention should be given to the form and prolongations of the aureola, the nature and intensity of its light, and also to the zodiacal light, which is now made to play so important a part in the solar system. Careful search should also be made for the small planets near the sun, suspected by M. LeVerrier. Perhaps, moreover, it may be possible to notice clearly the motion of the cone of the lunar shadow, the lower base of which should traverse the surface of the sea at the rate of 900 metres per second, while the upper terminus, if visible, will show by its distance from the zenith the height of the upper strata of our atmosphere.

3. *Notice of the Meteor of Nov.* 15, 1859; by Prof. E. Loomis.—On the morning of Nov. 15th, about 9½ o'clock, a remarkable meteor was witnessed by a large number of persons in New York and its vicinity. The meteor was so brilliant that although the sun was unclouded and had an elevation of about twenty degrees above the horizon, the flash attracted the attention of well nigh every person who happened at that time to be looking nearly toward that part of the heavens. The apparent diameter of its head was somewhat less than that of the sun, and it had an appendage like the tail of a comet several degrees in length. Its apparent path was nearly vertical, with a slight inclination towards the west; and the length of its visible path was variously estimated from 15° to 25°. The entire period of its visibility did not exceed one or two seconds. No sound was heard at New York which could reasonably be ascribed to the meteor. By taking the mean of the estimates of several observers, I have determined that the point of the horizon where the meteor vanished was about 21° west of south.

From the newspaper reports we learn that the same meteor was seen at Salem, Boston, and New Bedford, Mass., at Providence, R. I., at New Haven, Middletown, and Waterbury, Conn., at Albany and many other places in New York, at numerous places in New Jersey, at Baltimore, Md., at Washington and Georgetown, D. C., as also at Alexandria and Fredericsburg, Va. At all of those places the meteor appears to have been seen at the same instant of absolute time; and at all the stations north of New York the appearance was almost identical, and the direction of the meteor was somewhat west of south.

From a newspaper notice coming from Prof. Henry of the Smithsonian Institution, we learn that at Washington the apparent path of the meteor was nearly perpendicular to the horizon, and its point of disappearance was estimated to be four degrees north of east. Those lines of direction as observed at New York and Washington intersect at a point a little north of Cape May; and inasmuch as at each of those stations the apparent path was nearly vertical, the actual path must also have been nearly vertical, and the meteor undoubtedly struck the earth at some point not very remote from Cape May.

This conclusion is confirmed by the reports of the meteor from New Jersey. The meteor was generally observed throughout the southern part of that State, and was everywhere succeeded by a very remarkable explosion. At Beeseley's Point, situated on the Atlantic Ocean near lat. 39° 20′, the course of the meteor is said to have been from northeast to

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southwest. It was attended by a sudden flash of light, and left behind a curling track of a smoky or light cloudy appearance, which soon vanished. About a minute after the flash, there was heard a series of terrific explosions, which were compared to the discharge of a thousand cannon. These explosions continued for one or two minutes; they were very sharp and distinct, and shook the windows and doors of the houses. Similar noises have been reported from numerous stations in the southeastern part of New Jersey. These noises occasioned considerable alarm, and by some were thought to have been produced by an earthquake.

From the preceding facts it seems almost certain that the meteor must have struck the earth at some point a little north of Cape May; and as it was unquestionably a body of considerable size and of great density, if it struck on dry ground the meteor ought to have been discovered. As we have received no account of such a discovery there is reason to apprehend that the meteor may have descended into water, and probably into Delaware Bay. Analogy would lead us to conclude that this belonged to the class of iron meteors of which we have numerous specimens in our cabinets.

The velocity of this meteor was very extraordinary. It probably struck the earth at a distance of 110 miles from Washington, and is said to have been first seen at an elevation of 45°. This would make the length of its visible path 110 miles, and it is said to have described this path in two seconds, giving a velocity of 55 miles per second. A small portion of this velocity (7 miles per second) may be ascribed to the earth's attraction, and another portion was due to the motion of the earth in its orbit, for the earth was moving obliquely towards the meteor; but there still remains an independent velocity nearly double the velocity of the earth in its orbit. The path of the meteor in space could not therefore have been a circle with the sun for its center, as the above velocity is too great for any ellipse or even parabola; but such conclusions must be received with caution on account of the imperfection of the observations, for if we suppose the time of describing this path was three seconds, the independent velocity of the meteor would not have been much greater than that of the earth in its orbit.

4. *Meteoric Explosion, in West Tennessee, Sept. 1st*, 1859; by Prof. B. W. McDonnold, of Bethel College.—The first of September was made memorable by the great Aurora. Here, that day of the calendar had another *creta nota*—a meteoric explosion. This explosion was heard at Bethel College about 10 o'clock, A. M., and was at first thought to be the firing of cannon in honor of a political election.

The first report was double, like the almost simultaneous explosion of two great rockets, The reverberations were protracted, deep, distant. After the lapse of perhaps a half minute another explosion was heard, louder, deeper than the former, and the reverberations more protracted. The bearing of the sound was N. E.

I find that the report was heard forty miles north of us, where it was supposed to bear South East; it was heard twelve miles south and west, bearing same as here; but farther south and west it was not heard at all.

As yet, I have heard of no fragments of the meteor being found. I feel satisfied myself, however, of the meteoric origin of the explosion.

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5. Catalogue of the Meteorites in the Imperial Austrian Collection at Vienna; by Prof. W. HAIDINGER.—Haidinger has communicated to the Austrian Academy of Sciences a complete list of the meteorites contained in the Imperial Collection at Vienna. It is an abstract from the complete catalogue made by the late Prof. Partsch and continued by Dr. Hoernes, the present director of the Imperial Cabinet.

In the list—which here follows—only the names of the localities and the time of falling (I) are given, or (II) in case of meteorites the time of falling of which is unknown they are classified according to the date when first described.

The letter *I* following the year indicates the specimens to be meteoric *iron.*

I. *Meteorites, with time of fall.*

	A. D.		
1.	1492,	Nov. 7,	*Ensisheim*, Alsace, Département du Haut-Rhin, France.
2.	1715,	April 11,	*Garz* (Schellin), near Stargard, Prussia.
3.	1751, *I.*	May 26,	*Agram* (Hraschina village), Croatia.
4.	1753,	July 3,	*Tabar* (Plan, Strkow), Bohemia.
5.	1753,	Sept. 7,	*Liponas*, near Pont de Verle and Bourg en Bresse, Dép. del'Ain, France.
6.	1768,	Sept. 13,	*Lucé* en Maine, Dép. de la Sarthe, France.
7.	1768,	Nov. 20,	*Mauerkirchen*, Inn, Lower Austria.
8.	1773,	Nov. 17,	*Sigena* (Sena village), Aragon, Spain.
9.	1785,	Feb. 19,	*Eichstaedt* (Wittens), Franconia, Bavaria.
10.	1787,	Oct. 13,	*Charkow* (Bobrik), Government Charkow, Russia.
11.	1790,	July 24,	*Barbotan* (Roquefort, Créon, Juillac, Mezin, Agen, &c.), Dép. des Landes, Dép. du Gers, Dép. du Lot et Garonne, formerly Gascony, France.
12.	1794,	June 16,	*Sienna*, Tuscany.
13.	1795,	Dec. 13,	*Wold Cottage*, Yorkshire, England.
14.	1798,	March 8–12,	*Salès*, near Villefranche, Dép. du Rhône, France.
15.	1798,	Dec. 13,	*Benares* (Krakhut village), Bengal, E. Indies.
16.	1803,	April 26,	*L'Aigle*, Normandy, Dép. de l'Orne, France.
17.	1803,	Oct. 8,	*Apt* (Saurette), Dép. de Vaucluse, France.
18.	1803.	Dec. 13,	*Maessing* (Dorf St. Nikolas), Eggenfeld, Bavaria.
19.	1804,	April 5,	*Glasgow* (High Possil), Scotland.
20.	1805,	March 25,	*Doroninsk*, Government Irkutsk, Siberia.
21.	1805,	June,	*Constantinople*, Turkey.
22.	1805,	Nov.,	*Asco*, Corsica.
23.	1806,	March 15,	*Alais*, St. Étienne de Solm and Valence. Dép. du Gard, France.
24.	1807,	March 13,	*Timochin*, (Timschino, according to Eichwald), Iuchnow, Gov. Smolensk, Russia.
25.	1807,	Dec. 14,	*Weston*, Connecticut.
26.	1808,	April 19,	*Parma* (Casignano, Borgo S. Donino).

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27. 1808, May 22, *Stannern*, Iglau, Moravia.
28. 1808, Sept. 3, *Lissa*, Bunzlau, Bohemia.
29. 1809, ? *Kikina*, Wiasemsk, Gov. Smolensk, Russia.
30. 1810, Aug. *Tipperary* (Mooresfort), Ireland.
31. 1810, Nov. 22, *Charsonville*, near Orleans, Dép. du Loiret, France.
32. 1811, March 12, *Kuleschowka*, Gov. Poltawa, Russia.
33. 1811, July 8, *Berlanguillas*, near Burgos, Spain.
34. 1812, April 10, *Toulouse*, Dép. de la Haute-Garonne, France.
35. 1812, April 15, *Erxleben*, between Magdeburg and Helmstaedt, Prussia.
36. 1812, Aug. 5, *Chantonnay*, between Nantes and La Rochelle, Dép. de la Vendée, France.
37. 1813, Sept. 10, *Limerick* (Adair, Scagh, Brasky, Faha), Limerick County, Ireland.
38. 1813, Dec. 13, *Lontalax* (Lontalaks), Gov. Wiborg, Finland.
39. 1814, Feb. 15, *Bachmut*, Gov. Iekaterinoslaw, Russia.
40. 1814, Sept. 5, *Agen*, Dép. du Lot et Garonne, France.
41. 1815, Oct. 3, *Chassigny*, near Langres, Dép. de Haute-Marne, France.
42. 1818, April 10, *Zaborzika* (Saboryzy, Saboritz on the Slutsch), Volhynia, Russia.
43. 1818, June, *Seres*, Macedonia, Turkey.
44. 1818, Aug. 10, *Slobodka*, Iuchnow, Gov. Smolensk, Russia.
45. 1819, June 13, *Jonzac* (Barbézieux), Dép. de la Charente, France.
46. 1819, Oct. 13, *Politz*, near Gera, Duchy of Reuss.
47. 1820, July 12, *Lixna* (Liksen), Lasdany, Gov. Witebsk, Russia.
48. 1821, June 15, *Juvenas*, near Libonnez, Dép. de l'Ardèche, France.
49. 1822, Dec. 13, *Épinal* (la Baffe), Dép. des Vosges, France.
50. 1823, Aug. 7, *Nobleborough*, Maine.
51. 1824, Jan. 15, *Renazzo*, in Ferrara, Papal States.
52. 1824, Oct. 14, *Zebrak* (Praskoles), near Horzowitz, Beraun, Bohemia.
53. 1825, ? Government *Iekaterinoslaw*, Russia.
54. 1825, Feb. 10, *Nanjemoy*, Maryland.
55. 1825, Sept. 14, *Honolulu*, Sandwich Islands.
56. 1827, May 9, *Nashville* (Drake Creek), Tennessee.
57. 1827, Oct. 5, *Bialystok* (Kuasta or Kuasti village), Russian Poland.
58. 1828, June 4, *Richmond*, Virginia.
59. 1829, May 8, *Forsyth*, Monroe County, Georgia.
60. 1829, Sept. 9, *Krasnoi-Ugol*, Gov. Riesan, Russia.
61. 1831, July 18, *Vouillé*, near Poitiers, Dép. de la Vienne, France.
62. 1831, Sept. 9, *Wessely* (Dorf Znorow), Moravia.
63. 1833, Nov. 25, *Blansko*, Bruenn, Moravia.
64. 1833, Dec. 27, *Okniny* (Okaninah), Kremenetz District, Gov. Volhynien, Russia.

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65. 1835, Nov. 13, *Simonod* (Samonot), Belmont, Dép. de l'Ain, France.
66. 1836, Nov. 11, *Macao*, Prov. Rio Grande de Norte, Brazil.
67. 1837, July 24, *Gross-Divina*, near Budetin, Hungary.
68. 1837, Aug. *Esnaude*, Dép. de la Charente, France.
69. 1838, June 6, *Chandakapoor*, Berar, E. Indies.
70. 1838, Oct. 13, *Capeland* (Bokkeveld, 15 miles from Tulpagh), South Africa.
71. 1839, Feb. 13, *Little Piney*, west of Potosi, Missouri, lat. 37° 55′ N., long. 92° 5′ W. from Greenwich.
72. 1840, July 17, *Cereseto*, near Offiglia, Casale, Piedmont.
73. 1841, March 22, *Grueneberg* (Heinrichsau), Prussian Silesia.
74. 1841, June 12, *Château-Renard*, S. E. of Montargis, Dép. du Loiret, France.
75. 1842, April 26, *Milena* (Milyan), Pusinsko Selo, 4½ miles S. of Milena, Croatia.
76. 1842, June 4, *Aumières*, Canton St. George, Dép. de la Lozère, France.
77. 1843, March, *Bishopville*, South Carolina.
78. 1843, June 2, *Utrecht*, Blaauw Kapel, Loewenhutye, Netherlands.
79. 1843, Sept. 16, *Klein-Wenden*, near Nordhausen, Prussia.
80. 1846, May 8, *Macerata*, Monte Milan village, Ancona, Papal States.
81. 1847, Feb. 25, *Iowa*, Linn County, Iowa.
82. 1847, *I.* July 14, *Braunau* (Hauptmannsdorf), Koeniggraetz, Bohemia.
83. 1849, Oct. 31, *Cabarras County*, North Carolina.
84. 1851, April 17, *Guetersloh*, Westphalia.
85. 1852, Sept. 4, *Mezö-Madaras* (and Fekete), Transylvania.
86. 1852, Oct. 13, *Borkut*, Marmaros, Hungary.
87. 1853, Feb. 10, *Girgenti*, Sicily.
88. 1855, May 13, *Bremervoerde*, Landdrostei Stade, Hanover.
89. 1857, Oct. 10, *Ohaba*, E. of Karlsburg, Transylvania.
90. 1857, Apr. 15, *Kaba*, S. W. of Debreczin, Nordbihar, Hungary.
91. 1858, May 19, *Kakova*, N. W. of Oravitza, Temesvar Banat.

II. *Meteorites, with time of discovery.*

92. 1751, *I.* *Steinbach*, between Eibenstock and Johann-Georgenstadt, Saxony (sometimes given as coming from Norway, Tabor, Senegal, &c.).
93. 1763, *I.* *Senegal*, Siratik in Bambuk, Africa.
94. 1776, *I.* *Krasnojarsk*, Gov. Ieniseisk, Siberia.
95. 1784, *I.* *Toluca*, Mexico.
96. 1788, *I.* *Tucuman* (Otumpa), Argentine Republic, S. America.
97. 1792, *I.* *Zacatecas*, Mexico.
98. 1801, *I.* *Cape of Good Hope*, Africa.
99. 1811, *I.* *Elbogen*, Bohemia.

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100. 1811, *I.* *Durango*, Mexico.
101. 1814, *I.* *Bitburg*, Lower Rhine, Prussia.
102. 1814, *I.* *Texas* (Red River).
103. 1815, *I.* *Lénarto*, Scharosch, Hungary.
104. 1816, *I.* *Bahia* (Bemdego), Brazil.
105. 1819, *I.* *Baffins Bay*, Greenland.
106. 1822, *I.* *Brahin*, Gov. Minsk, Russia.
107. 1823, *I.* *Rasgata*, New Granada, S. America.
108. 1827, *I.* *Atacama*, Bolivia.
109. 1828, *I.* *Caille* (Grasse), Dép. du Var, France.
110. 1829, *I.* *Bohumilitz*, Prachin, Bohemia.
111. 1830, *I.* *Guilford*, North Carolina.
112. 1838, *I.* *Claiborne*, Alabama.
113. 1839, *I.* *Asherville*, Buncombe Co., North Carolina.
114. 1840, *I.* *Smith County*, Coney Fork, Tennessee.
115. 1840, *I.* *Cocke County*, Cosby-Creek, (also called Sevier iron), Tennessee.
116. 1841, *I.* *Petropaulowsk*, Gov. Tomsk, Siberia.
117. 1843, *I.* *Oaxaca*, Mexico.
118. 1844, *I.* *Burlington*, Otsego Co., New York.
119. 1844, *I.* *Arva* (Szlanicza), Hungary.
120. 1845, *I.* *Lockport*, New York.
121. 1845, *I.* *Green County* (Babbs-Mills), Greenville, Tennessee.
122. 1845, *I.* *Government Simbirsk*, Russia.
123. 1845, *I.* *Government Kursk*, Russia.
124. 1845, *I.* *Government Poltawa* (according to Eichwald in the district of Kamensk), Russia.
125. 1847, *I.* *Seeläsgen*, Neumark, Brandenburg, Prussia.
126. 1849, *I.* *Chesterville*, South Carolina.
127. 1850, *I.* *Schwetz*, Province of Prussia.
128. 1850, *I.* *Ruff's Mountain*, Newberry, South Carolina.
129. 1850, *I.* *Salt River*, Kentucky.
130. 1851, *I.* *Seneca Falls*, Cayuga Co., New York.
131. 1852, *Mayence*, Duchy of Hesse.
132. 1853, *I.* *Union County*, Georgia.
133. 1853, *I.* *Lion River*, Namaqua Land, South Africa.
134. 1854, *I.* *Tazewell*, Claiborne Co., Tennessee.
135. 1854, *I.* *Putnam County*, Georgia.
136. 1854, *I.* *Canada*, Madoc, Canada West.
137. 1856, *Hainholz*, S. W. of Paderborn, Minden, Westphalia.

V. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *Inquiries into the Phenomena of Respiration;* by EDWARD SMITH, M.D., (L. E. and D. Phil. Mag., xvii, 439).—The author gives in this communication the result of numerous inquiries into the quantity of carbonic acid expired, and of air inspired, with the rate of pulsation and respiration,—1st, in the whole of the twenty-four hours, with and without exertion and food; 2nd, the variations from day to day, and from season to season; and 3rd, the influence of some kinds of exertion.

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After a description of the apparatus employed by previous observers, he describes his own apparatus and method. This consists of a spirometer to measure the air inspired, capable of registering any number of cubic inches; and an analytical apparatus to abstract the carbonic acid and vapor from the expired air. The former is a small dry gas-meter, of improved manufacture, and the latter consists of—1st, a desiccator of sulphuric acid to absorb the vapor; 2nd, a gutta-percha box, with chambers and cells, containing caustic potash, and offering a superfices of 700 inches, over which the expired air is passed, and by which the carbonic acid is abstracted; and 3rd, a second desiccator to retain the vapor which the expired air had carried off from the potash box. A small mask is worn, so as to prevent any air entering the lungs without first passing through the spirometer, and the increase in the weight of this with the connecting tube and the first desiccator gives the amount of vapor exhaled, whilst the addition to the weight of the potash box and the second dessiccator gives the weight of the carbonic acid expired. The balances employed weigh to the $\frac{1}{100}$ of a grain, with 7 lbs. in the pan. By this apparatus the whole of the carbonic acid was abstracted during the act of expiration, and the experiment could be repeated every few minutes, or continued for any number of hours, and be made whilst sleeping and with certain kinds of exertion.

The amount of carbonic acid expired in the twenty-four hours was determined by several sets of experiments. Four of these, consisting of eight experiments, were made upon four gentlemen, on the author, Professor Frankland, F.R.S., Dr. Murie, and Mr. Moul, during the eighteen hours of the working day. In two of them, the whole of the carbonic acid was collected, and in two others the experiment was made during ten minutes at the commencement of each hour, and of each hour after the meals. The quantity of carbonic acid varied from an average of 24·274 oz. in the author to 16·43 oz. in Professor Frankland. The quantity evolved in light sleep was 4·88 and 4·99 grains per minute, and scarcely awake 5·7, 5·94, and 6·1 grains at different times of the night. The author estimates the amount in profound sleep at 4·5 grains per minute; and the whole evolved in the six hours of the night at 1950 grains. Hence the total quantity of carbon evolved in the twenty-four hours, at rest, was, in the author, 7·144 oz. The effect of walking at various speeds is then given, with an estimate of the amount of exertion made by different classes of the community, and of the carbon which would be evolved with that exertion.

The author then states the quantity of air inspired in the working day, which varied from 583 cub. in. per minute in himself to 365 cub. in. per minute in Professor Frankland; the rate of respiration, which varied in different seasons as well as in different persons; the depth of inspiration, from 30 cub. in. to 39·5 cub. in.; and the rate of pulsation. The respirations were to the pulsations as 1 to 4·63 in the youngest, and as 1 to 5·72 in the oldest. One-half of the product of the respirations into the pulsations gave nearly the number of cubic inches of air inspired in some of the persons, and the proportion of the carbonic acid to the air inspired varied from as 1 gr. to 54·7 cub. in. to as 1 gr. to 58 cub. in. The variations in the carbonic acid evolved in the working day gave an average

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maximum of 10·43, and minimum of 6·74 grains per minute. The quantity increased after a meal and decreased from each meal, so that the minima were nearly the same, and the maxima were the greatest after breakfast and tea.

The effect of a fast of forty hours, with only a breakfast meal, was to reduce the amount of carbonic acid to 75 per cent. of that which was found with food; to render the quantity nearly uniform throughout the day, with a little increase at the hours when food had usually been taken, and to cause the secretions to become alkaline.*

The variations from day to day were shown to be connected with the relation of waste and supply on the previous day and night, so that with good health, good night's rest, and sufficient food, the amount of respiration was considerable on the following morning, whilst the reverse occurred with the contrary conditions. Hence the quantities were usually large on the Monday. Temperature was an ever-acting cause of variation and caused a dimunition in the carbonic acid as the temperature rose.

The effect of season was to cause a diminution of all the respiratory phenomena as the hot season advanced. The maximum state was in spring, and the minimum at the end of summer, with periods of decrease in June and of increase in October. The diminution in the author was 30 per cent. in the quantity of air, 32 per cent. in the rate of respiration, and 17 per cent. in the carbonic acid. The influence of temperature was considered in relation to season, and it was shown that whilst sudden changes of temperature cause immediate variation in the quantity of carbonic acid, a medium degree of temperature, as of 60°, is accompanied by all the variations in the quantity of carbonic acid, and that there is no relation between any given temperature and quantity of carbonic acid at different seasons. Whatever was the degree of temperature, the quantity of carbonic acid, and all other phenomena of respiration, fell from the beginning of June to the beginning of September. The author then described the influence of atmospheric pressure, and stated that neither temperature nor atmospheric pressure accounts for the seasonal changes.

The kinds of exertion which had been investigated were walking and the treadwheel. Walking at two miles per hour induced an exhalation of 18·1 grs. of carbonic acid per minute, and at three miles per hour of 25·83 grs.; whilst the effect of the treadwheel at Coldbath Fields Prison was to increase the quantity to 48 grs. per minute. All these quantities vary with the season, and hence the author recommends the adoption of relative quantities, the comparison being with the state of the system at rest, and apart from the influence of food.

2. *Dr. Newberry's Explorations in New Mexico, Utah and Texas*, during the past season, are rewarded by many new and important discoveries, especially in structual geology and palæontology. His collection of fossils is very large, offering conclusive evidence of the geological structure of a very large area. Of the cretaceous deposits he was fortunate in obtaining a particularly satisfactory analysis. Contrary to all our previous notions, these beds turn out to be much more largely developed—that is, existing in much greater force, stratigraphically, West of

* The quantity of air was reduced 30 per cent, that of vapor in the expired air 50 per cent, the rate of respiration was reduced 7 per cent, and of pulsation 6 per cent.

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the Rocky Mountains, than East of them. In Southern Utah, (just where Marcou claims there are no cretaceous rocks) he found beautiful exposures of 4000 feet thickness of strata of that age, with abundant fossils, both animal and vegetable. The bones of a huge Saurian are among Dr. Newberry's novelties.

We hope in our next Number to be able to give a more exact statement of Dr. Newberry's important discoveries.

Rumors reach us of other and even more startling geological discoveries in the extreme West and North, which we are not at liberty to name at present, but which ere long will be announced, from the proper authority.

3. *Discovery of Devonian rocks and fossils in Wisconsin.* (Private communication to the editors.)—At a late meeting of the Milwaukee Geological Club or Association, Mr. I. A. Lapham announced the discovery of rocks near Milwaukee, equivalent to the Devonian (Old Red Sandstone,) containing remains, which he exhibited, of characteristic fishes. These remains consist of fragments of bone, teeth, a paddle with portions of the tuberculated skin or osseous covering. The bed containing these remains overlies the Niagara group, and is the uppermost of the geological series yet observed in Wisconsin.

4. *Cretaceous Strata at Gay Head, Mass.*—Wm. Stimpson, Esq., accompanied by Messrs. Slack and Ordway, during an excursion in August to Martha's Vineyard, obtained at Gay Head many new fossils in addition to those mentioned by Hitchcock, an examination of which appears to authorize the conclusion that these well known beds are Cretaceous rather than Eocene. Among the fossils obtained are cretaceous bones, vertebræ and teeth of shark, (fragments of some teeth indicating a length of seven inches!) some brachyurous crustacea in a good state of preservation, twelve species of bivalve mollusca, and one univalve; also leaves, fragments and seeds of dicotyledonous plants, &c.

5. *The New Museum of Comparative Zoology, at Cambridge*, is making rapid progress. One wing is nearly ready to receive collections. During his summer trip in Europe, Agassiz made large and important acquisitions for the Museum, in addition to the vast stores already awaiting an occasion for display. Besides a superb suite of fossil Crustacea, Agassiz was so fortunate as to purchase at Heidelburg the collection of fossils from which Bronn's *Lethaea geognostica* was composed. This collection contains the original specimens of the first and most important writers on Palæontology.

Another important addition to the new museum has been made by a sea Captain, who has just brought from Penang and Singapore some three thousand specimens of fish, crustacea, and a most beautiful and choice collection of zoophytes.

6. William P. Blake, Esq., the geologist, has assumed the editorship of the Mining Magazine, a monthly heretofore published in New York. Under his direction this Journal will undoubtedly become a reliable exponent of the important interests it represents.

7. Prof. Wm. S. Chauvenet, lately of the U. S. Naval Academy, at Annapolis, has accepted the Chair of Mathematics in the University of Missouri, at St. Louis.

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8. Professor DANA was, by our last dates (Nov. 26th) at Florence, on his way to Rome—designing to divide the winter and spring between Rome, Naples and Sicily, if the state of the country permits his visiting that Island. His health was improving.

NEW BOOKS.—

1. *Archaia; or Studies of the Cosmogony and Natural History of the Hebrew Scriptures;* by J. W. DAWSON, LL.D., F.G.S., author of "*Acadian Geology*," Principal of McGill College. Montreal: B. Dawson & Son. London: Sampson, Low, Son & Co. 1860. 12mo, pp. 400.—The author of this interesting volume brings to his task the union of a varied scientific, literary and biblical acquirement with a hearty Christian faith. Like all devout and earnest men of science he has an unwavering confidence in the divine unity of truth, and does not for a moment doubt that the genesis of the rocks will confirm the genesis of Moses. He seizes boldly and with candor the real difficulties which every writer has found standing in his way when treating this interesting problem. After an eloquent introduction and a discussion of the object, character and authority of the Hebrew Cosmogony, of the general views of nature contained in the Hebrew Scriptures, &c., he thus sums up his chapter on the Days of Creation—the events of the first day. "At the beginning of the period, the earth, covered with a universal ocean and misty atmospheric mantle, was involved in perfect darkness. A luminous ether was called into existence, which spread a diffused light throughout the whole solar system. This luminous matter being gradually concentrated toward the centre of the system at length produced, in connection with the earth's rotation, the alternation of day and night. These changes were the work of a long period—an æon or day of the Creator."

Undoubtedly the most difficult points in the whole Mosaic Cosmogony, to explain in a rational manner consistent with the views of science, are the creation of plants before the appearance of the '*luminaries*,' and the separation of the two organic kingdoms by the introduction of this middle term. These difficult points are treated with much acuteness and learning, and with a full recognition of the various opinions put forth to meet them by various authors. If we cannot fully agree with our Author in his conclusions, we can truly say that no one has higher claims to a respectful hearing, and if the conclusions at which he arrives leave yet something to be desired, the want rests more in the imperfection of our knowledge than in Prof. Dawson's enunciation of it.

Prof. Dawson does not shrink from a fearless review of the much vexed question of the unity of the human race in a long and interesting chapter on the "Unity and Antiquity of Man." It is hardly necessary to add that he adopts the Mosaic view. We can do this volume little justice, in these few lines to which our last pages restrict us, but we can earnestly commend its spirit, and hope for its wide perusal by all who follow the course of the deep questions it involves.

2. *On the Origin of Species by means of Natural Selection: or, the Preservation of Favored Races in the Struggle for Life;* by CHARLES DARWIN. (Murray.)—[Waiting the arrival of our copy of this new volume from Mr. Darwin's pen, which at this present writing (Dec. 16) has not reached the United States, we copy some passages from a rather

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timid and superficial notice of the work in the London Athenæum of Nov. 19th. It is no doubt destined to produce a great discussion on what may properly be called the most fundamental truth of natural history.—Eds.]

"Naturalists of the highest eminence are thoroughly satisfied that each species of animal—all that flies, and walks, and creeps, and wades—has been independently created; and the majority of naturalists have agreed with Linnæus in supposing that all the individuals propagated from one stock have certain distinguishing characters in common, which will never vary, and which have remained the same since the creation of each species. Mr. Darwin, on the contrary, believes that 'the innumerable species, genera, and families of organic beings with which this world is peopled, have all descended, each within its own class or group, from common parents, and have all been *modified in the course of descent*.' To his mind, 'it accords better with what we know of the laws impressed on matter by the Creator that the production and extinction of the past and present inhabitants of the world should have been due to secondary causes, like those determining the birth and death of the individual.' When he views 'all beings not as special creations, but as the lineal descendents of some few beings which lived long before the first bed of the Silurian system was deposited, they seem to him to become ennobled.' We confess some doubt and some uneasiness here. 'Judging from the past, we may safely infer that not one living species will transmit its unaltered likeness to a distant futurity. And of the species now living very few will transmit progeny of any kind to a far distant futurity; for the manner in which all organic beings are grouped shows that the greater number of species of each genus, and all the species of many genera, have left no descendants, but have become utterly extinct. We can so far take a prophetic glance into futurity as to foretell that it will be the common and widely-spread species, belonging to the larger and dominant groups, which will ultimately prevail and procreate new and dominant species.' We cannot say that this is easy doctrine.

"To support these bold views the volume is devoted. The world of animals is contemplated as engaged in one vast unceasing struggle for existence. All organic beings are exposed to severe competition. The face of Nature, it is true, is bright with gladness, and her garner-houses are stored with an abundance of food. Birds sing, insects hum, beasts prowl about in ease and take no thought for the morrow: but the morrow measured by seasons and years has not always a superabundance of food for them. The struggle for existence does not merely relate to self, but includes success in leaving healthy progeny. The high rate at which all organic beings tend to multiply approaches to the rapidity of geometrical increase. More individuals are produced than can by any possibility be supported. There must, then, in every case, be a severe struggle, either of one individual with another of the same species, or with individuals of distinct species, or with the physical conditions of life."

"Now, how does the struggle for existence operate with respect to Variation? Man can produce varieties in animals by the practice of selection. What he has already done by this means the menagerie, the poultry-yard, the field, and the garden display. Is there anything analo-

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gous to this in the course of Nature? The author contends that there is, and he names it *Natural Selection.* This principle, whatever others may think of it, and whether they admit its operations or not, in Mr. Darwin's book plays the prominent part. It may be plainly defined, and appears to be briefly this. Under domestication it may be truly said that the whole animal organization becomes in some degree plastic. As variations useful to man have undoubtedly occurred, is it not to be expected that other variations, useful in some way to each being in the great and complex battle of life, should sometimes occur in the course of thousands of generations? If such do occur, then, remembering the struggle for existence, individuals possessing any advantage over others would have the best chance of surviving and of procreating their kind, while injurious variations would be rigidly destroyed. Such a continual preservation of favorable, and rejection of injurious variations, is the principle of Natural Selection. It is illustrated, amplified and confirmed by abundant examples through many pages."

"Certainly there is something poetical in the conception of a succession of created beings, daily and hourly making the wisest election amidst all variations and divergencies; carefully rejecting what is bad, and preserving and accumulating all that is good; operating silently and insensibly, whenever and wherever opportunity offers, towards the improvement of every organized existence in relation to its organic and inorganic condition of life. There is, too, a certain simplicity in the theory of descent with modification through natural selection from a few vastly remote progenitors. 'I believe,' says Mr. Darwin, 'that animals have descended from at most only four or five progenitors, and plants from an equal or lesser number. Analogy would lead us one step further—namely, to the belief that all animals and plants have descended from some one prototype.' A cabbage may have been the parent plant, a fish the parent animal.

"A man of imaginative power might most attractively depict the grand yet simple and direct issues of such a theory. Here are a vast variety of forms of life, most wonderfully co-adapted, most closely connected, most richly adorned, yet they are all 'the lineal descendants of those which lived before the Silurian epoch; and one may feel certain that the ordinary succession by generation has never once been broken, and that no cataclysm has desolated the whole world. Hence, we may look with some confidence to a secure future of equally inappreciable length. And as Natural Selection works solely by and for the good of each being, all corporeal and mental endowments will tend to progress toward perfection.' Yes, an unbroken, sure, though slow, living progress towards animal perfectibility is a delightful vision; natural and gradual optimism is a welcome fancy. What need of distinct creation? If a monkey has become a man—what may not a man become?

"Let the past history of organic life speak. From the thirteen miles in thickness of British strata (exclusive of igneous rocks) comes there no testimony? Palæontology is summoned into court, and is closely interrogated by Mr. Darwin. This proves but a hesitating and reluctant witness; yet counsel for the new theory detects and exposes its imperfections where its testimony is not favorable. We might fairly expect to find in

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the fossiliferous rocks not a few proofs of the former existence of the numerous intermediate links between distinct specific forms if the proposed theory be true. We do not find them, many will allege, because they never existed. Not so, says our theorist,—but because they were never preserved. Palæontology, however, has not yet revealed any such finely graduated organic scale, and it is not logical to assume that it ever will. When a record is flatly against you, it is quite allowable for you to display its imperfection, but, that being proved, you have only established a negative, and have acquired no confirmation. Grant imperfection, enormous lapse of time, poverty of palæontological collections, and comparative restriction of research, and other such postulates, and then the theory stands just as it stood before, uncorroborated by geology.

"There is positively hostile testimony from the rocks to be confronted. Whole groups of species suddenly and abruptly appear in certain formations, and seem at once to contradict any theory of transmutation of species. Either that fact or the theory must be overturned. Of course, Mr. Darwin accepts the former alternative, and strives to show how liable we are to error in supposing that whole groups of species have been suddenly produced. But another and an allied objection may be started, derived from the manner in which numbers of species of the same group suddenly appear in the lowest known fossiliferous rocks. To meet this and uphold the new theory, it must be sustained by another, viz.,—that before the lowest Silurian stratum was deposited, immensely protracted periods elapsed, at least as long as any subsequent periods, and that during these vast extensions of time the world swarmed with living creatures. Several of the most eminent geologists, including Murchison, will refuse to admit this presumption. Mr. Darwin's geology is more singular than we had thought. 'For instance,' says he, 'I cannot doubt that all Silurian trilobites have descended from some one crustacean which must have lived long before the Silurian age, and which probably differed greatly from any known animal.' Extend and multiply such assumptions, and the theories may take any form you please."

"After all, this book is but an abstract. The larger work is nearly finished, but it will demand two or three more years for completion. Health, labor, and observations are wanting for awhile, but in due season we hope to see the work 'with references and authorities for the several statements.' We should offer remarks on some important topics but that our author says, 'A fair result can be obtained only by fully stating and balancing the facts and arguments on both sides of the question; and this cannot possibly be here done.'

"Meanwhile Mr. Darwin anticipates small favor from many of the older and more eminent naturalists; his hopes chiefly rest on the young, and, as he would say, the unshackled. 'A few naturalists,' he observes, 'endowed with much flexibility of mind, who have already begun to doubt on the immutability of species, may be influenced by this volume; but I look with confidence to the future, to young and rising naturalists who will be able to view both sides of the question with impartiality.' It is enough for us to add that neither book, author, nor

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subject is of merely ordinary character. The work deserves attention, and will, we have no doubt, meet with it. Scientific naturalists will take up the author upon his own peculiar ground; and there will we imagine be a severe struggle for at least theoretical existence."

3. *Elements of Somatology: A Treatise on the general properties of Matter;* by GEO. M. MACLEAN, M.D., Prof. Chemistry and Nat. Philosophy, in Alleghany City, Pa. New York: J. Wiley, 56 Walker St., 1859. 12mo, pp. 124.—This modest little volume, the author tells us in his preface, is the fruit of many hours of study during a period of ill-health—a sort of "Consolations of a Philosopher."

It is a simple exposition of accepted doctrines on the familiar subjects of extension, impenetrability, figure, divisibility, indestructibility, porosity, compressibility, dilatibility, mobility, inertia, contraction, repulsion, polarity, elasticity, and the constitution of matter. The subject of Attraction he considers under thirty-one subdivisions. His expositions of the phenomena of adhesion, capillarity and osmose are more full and satisfactory than it is usual to find in elementary works. On many points in his discussions, we might join issue with our Author, as when he states cohesive attraction to be only a modification of gravitation—(p. 57)—and when he adduces the phenomena of contraction in a soap-bubble, in illustration of the cause of the meniscus of capillarity (p. 69).

It would have added materially to the value of the Treatise, and its interest to the student and general reader, if the author had appended to passages marked as quotations a reference to the authority from which they are copied. Except a quotation accredited to Cavallo, we do not recall a single reference to any authority in the volume.

A brief statement of the accepted doctrines of physics on the subject of "*Molecular Forces*" would have relieved his chapters on attraction and repulsion of several obscure points.

The work bears marks of haste, or want of careful revision of the press. Among many examples of this we may name the sentence under capillary attraction, commencing "The tube having the form of a syphon," (foot of p. 69) which conveys so confused a notion of what the Author seeks to express, that after several readings we have been unable to comprehend it. Bodies are said to weigh *less* near the poles than at the equator, (p. 55), and numerous typographical blemishes evidence the disadvantage of printing a scientific book at a distance from the press. These minor faults are easily removed in a new edition—which will be very likely to be called for, as the book is one of convenient reference for all teachers. The author will, however, confer a great favor on all such readers in a new edition, by quoting his authorities.

4. *The Telegraphic Manual, a complete history and description of the Semaphoric, Electric and Magnetic Telegraphs of Europe, Asia, Africa, and America, ancient and modern, with 625 illustrations;* by T. P. SCHAFFNER, of Kentucky. N. York, 1859. 8vo, pp. 850.—The title of this volume is an index to its contents. That he may leave nothing behind him for future explorers, the author commences his labors with Adam and Eve, in Eden! Mr. Schaffner has, from his wide and long experience in telegraphic construction and management both in the United States and in Europe, remarkable qualifications for the work he has under-

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taken. The result of his labors is satisfactory. His work, in fullness of detail, leaves little to desire, and he appears always solicitous to avoid the charge of partizanship in awarding to rival parties what he judges to be their respective shares of merit, in cases of contested claims. As a literary production, it is to be regretted that the author did not submit his manuscripts to the revision of some judicious literary friend—thus avoiding certain faults of style of too frequent occurrence. But these are minor faults and can be easily pardoned where there is so much to praise.

5. *Bail's Drawing System:* THE HUMAN HEAD, by LOUIS BAIL, (graduate of the Royal Academy of Fine Arts, in Munich). New Haven: Author, 1859. 8vo, 64 plates in outline.—Prof. Bail has here done a great service to both teachers and pupils in the Arts in the United States. The success which has followed the Author's use of his own system in many of our higher seminaries, as well as in public classes, is the best guaranty of the adaptation of its parts to the great ends of instruction, and no doubt will secure its general adoption.

6. *Memoir of John Griscom, LL.D., late Professor of Chemistry and Nat. Philosophy, &c.;* by his son, JOHN H. GRISCOM, M.D. New York: Carter, 1859. 8vo, pp. 427.—Some among the older readers of this Journal will recall with pleasure the selections from foreign scientific literature, which for many years Prof. Griscom prepared for these pages. His active life was well filled with varied duty as an instructor and philanthropist. He was either largely or entirely instrumental in the establishment of the New York High School; the Society for the prevention of Pauperism; the House of Refuge; and other institutions of public charity, which amid all the complaints of profligacy in her public administration, have shed a peculiar honor on the active benevolence of the City of New York.

As early as 1818, he instituted and sustained for many years, independent courses of scientific lectures, in New York city, and in other places—illustrating his courses by numerous experiments, and a costly apparatus procured at his own expense. This was long before the era of *popular lectures*, and Dr. Griscom, with the senior Editor of this Journal, may claim the honor of inaugurating a system which has since become almost universal in the United States.

Dr. Griscom published two volumes of Travels in Europe, in 1818–19, remarkable for the spirit of candor and kindness which is seen on every page, and interesting to this day, for the characteristic personal sketches he gives of the distinguished men of science he met abroad.

Dr. Griscom was an eminently good man; a member of the Society of Friends; a devout Christain believer, and without bigotry. His mild and gentle nature delighted in the most catholic liberality, and many of his warmest friends were members of other Christian sects. We have recently been called upon to commemorate several eminent scientific friends and collaborators—Cleaveland, Hare, Redfield and Olmsted, now numbered with the dead. We now add the name of Griscom—a name cherished long and warmly by intimate social and scientific relations. Our early auxiliaries and friends in science are now few in number, and our duties are soon to pass into younger and we hope better hands—

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but the pioneers will be remembered as the pilgrims of science, altho its votaries are now a Legion. This Memoir is a fine example of class, and does credit both to the filial piety and literary ability of distinguished Author. B.

FLEURY: *Des races qui se partagent l'Europe.* 8vo, 132 pages. Hachette & C This author has brought to his study of the races great learning and a deep kn edge of the facts. He considers modern European civilization as springing the German race.

JAMIN: *Cour de Physique de l'Ecole polytechnique.* T. II. with 3 plates and figures in the text.—This volume contains *Heat* and *Acoustics*. The plates are graved and the mathematical formulæ are printed with the neatness and accu which distinguish at present the productions of the press of Bachelier abo others in France.

G. LAMÉ: *Leçons sur les coordennées, curvilignes et leurs diverses applicatio* 8vo. 1859. Mallet-Bachelier, Paris.—Under this title the distinguished Pro of the Polytechnic School introduces to us a new branch of mathematical sc It is geometry considered from a physico-mathematical point of view.

OBITUARY.—

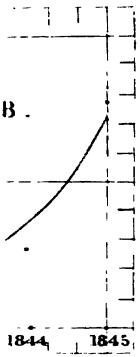
Professor WILLIAM W. TURNER, one of our most distinguished lologists, died at Washington, Nov. 29, 1859, in the 50th year of age. Although an excellent linguist, he devoted himself less to the of words than to that of the structure of languages, their origin connections, upon which subject his views were eminently philoso He was born in London, but was brought in his fifth year to this cou where he has ever since resided. He early developed a taste for study of oriental languages and was in 1842 appointed instructo the Hebrew and cognate tongues in the Union Theological Semina New York. The last seven years of his life were spent in Washin where his attainments and upright, amiable deportment secured to h host of friends. He did a vast amount of work in the way of transl and grammatic compilation, little of which has, however, appeared i name:—he is chiefly known for his contributions to the "Bibliothec cra" and to the journals of the American Oriental and Ethnological cieties. But his published works give no adequate idea of the exte his labors; his stores of knowledge were always open to his friends most freely imparted, thus contributing to the advancement of sci other than his own. He had, during the past ten years, given muc tention to the study of the Aborigines of North America and their guages, not only elaborating general principles from the vocabularie lected by travellers, but confirming these and adding new informatio communication with the delegates from various tribes that visited W ington upon business with the central government. In this investig he accumulated a large amount of materials which will, it may be h be some time given to the world. W.

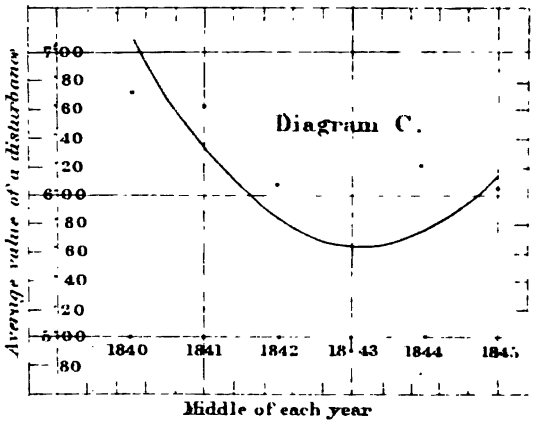
Dr. GEORGE WILSON, First Regius Professor of Technology in the versity of Edinburgh, and Director of the Industrial Museum of that died near the end of November last, at the early age of 41. He was biographer of Reid and Cavendish, and author of numerous researc among which are the discovery of fluorine in blood and sea-water. O of his published works are his "Researches on Color-blindness," an " mentary Treatise on Chemistry," and "The Five Gate-ways of Kno edge." He is a great loss to his native city and the world.

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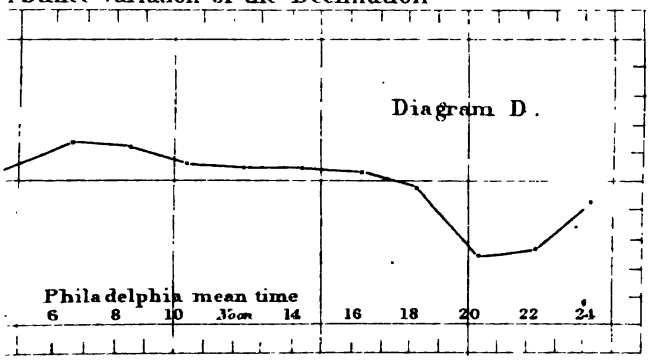
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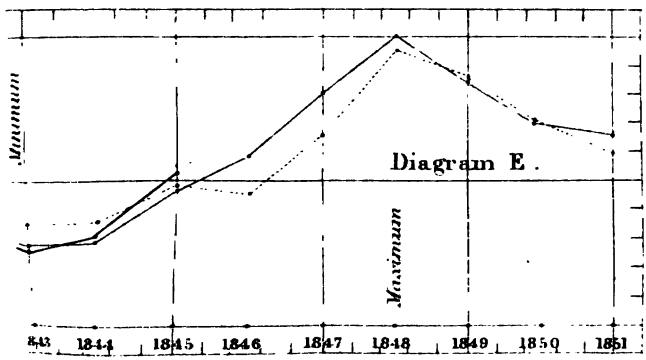
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THE

AMERICAN

JOURNAL OF SCIENCE AND ARTS.

[SECOND SERIES.]

ART. XV.—*Review of Darwin's Theory on the Origin of Species by means of Natural Selection.**

FULLY to understand the foregoing Essay of Dr. Hooker,† it should be read in the light of Mr. Darwin's book. The Essay is a trial of the Theory,—an attempt by one inclined in its favor to see how the theory will work, when applied to the flora of a large and most peculiar province of the world.

This book is already exciting much attention. Two American editions are announced, through which it will become familiar to many of our readers, before these pages are issued. An abstract of the argument,—for "the whole volume is one long argument," as the author states,—is unnecessary in such a case; and it would be difficult to give by detached extracts. For the volume itself is an abstract, a prodromus of a detailed work upon which the author has been laboring for twenty years, and which "will take two or three more years to complete." It is exceedingly compact; and although useful summaries are ap-

* *On the Origin of Species by means of Natural Selection, or the Preservation of Favored Races in the Struggle for Life;* by CHARLES DARWIN, M.A., Fellow of the Royal, Geological, Linnæan, etc. Societies, Author of "Journal of Researches during H. M. S. Beagle's Voyage round the World." London: John Murray. 1859. pp. 502, post 8vo.

† This article was intended to follow the remaining part of the essay of Dr. Hooker, commenced in our January number; the continuation of which we are obliged to defer, for want of room.—EDS.

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pended to the several chapters, and a general recapitulation contains the essence of the whole, yet much of the aroma escapes in the treble distillation, or is so concentrated that the flavor is lost to the general, or even to the scientific reader. The volume itself,—the proof spirit—is just condensed enough for its purpose. It will be far more widely read, and perhaps will make deeper impression than the elaborate work might have done, with its full details of the facts upon which the author's sweeping conclusions have been grounded. At least it is a more readable book: but all the facts that can be mustered in favor of the theory are still likely to be needed.

Who, upon a single perusal, shall pass judgment upon a work like this, to which twenty of the best years of the life of a most able naturalist have been devoted? And who among those naturalists who hold a position that entitles them to pronounce summarily upon the subject, can be expected to divest himself for the nonce of the influence or received and favorite systems? In fact, the controversy now opened is not likely to be settled in an off-hand way, nor it is desirable that it should be. A spirited conflict among opinions of every grade must ensue, which,—to borrow an illustration from the doctrine of the book before us—may be likened to the conflict in nature among races in the struggle for life, which Mr. Darwin describes; through which the views most favored by facts will be developed and tested by 'Natural Selection,' the weaker ones be destroyed in the process, and the strongest in the long run alone survive.

The duty of reviewing this volume in the American Journal of Science would naturally devolve upon the principal Editor, whose wide observation and profound knowledge of various departments of natural history, as well as of geology, particularly qualify him for the task. But he has been obliged to lay aside his pen, and to seek in distant lands the entire repose from scientific labor so essential to the restoration of his health,—a consummation devoutly to be wished, and confidently to be expected. Interested as Mr. Dana would be in this volume, he could not be expected to accept its doctrine. Views so idealistic as those upon which his "Thoughts upon Species"* are grounded, will not harmonize readily with a doctrine so thoroughly naturalistic as that of Mr. Darwin. Though it is just possible that one who regards the kinds of elementary matter, such as oxygen and hydrogen, and the definite compounds of these elementary matters, and their compounds again, in the mineral kingdom, as constituting species, in the same sense, fundamentally, as that of animal and vegetable species, might admit an evolution of one species from another in the latter as well as the former case.

* Article in this Journal, vol. xxiv, p. 305.

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Between the doctrines of this volume and those of the other great Naturalist whose name adorns the title-page of this Journal, the widest divergence appears. It is interesting to contrast the two, and, indeed, is necessary to our purpose; for this contrast brings out most prominently, and sets in strongest light and shade the main features of the theory of the origination of species by means of Natural Selection.

The ordinary and generally received view assumes the independent, specific creation of each kind of plant and animal in a primitive stock, which reproduces its like from generation to generation, and so continues the species.* Taking the idea of species from this perennial succession of essentially similar individuals, the chain is logically traceable back to a local origin in a single stock, a single pair, or a single individual, from which all the individuals composing the species have proceeded by natural generation. Although the similarity of progeny to parent is fundamental in the conception of species, yet the likeness is by no means absolute: all species vary more or less, and some vary remarkably—partly from the influence of altered circumstances, and partly (and more really) from unknown constitutional causes which altered conditions favor rather than originate. But these variations are supposed to be mere oscillations from a normal state, and in Nature to be limited if not transitory; so that the primordial differences between species and species at their beginning have not been effaced, nor largely obscured, by blending through variation. Consequently, whenever two reputed species are found to blend in nature through a series of intermediate forms, community of origin is inferred, and all the forms, however diverse, are held to belong to one species. Moreover, since bisexuality is the rule in nature (which is practically carried out, in the long run, far more generally than has been suspected), and the heritable qualities of two distinct individuals are mingled in the offspring, it is supposed that the general sterility of hybrid progeny, interposes an effectual barrier against the blending of the original species by crossing.

From this generally accepted view the well-known theory of Agassiz and the recent one of Darwin diverge in exactly opposite directions.

That of Agassiz differs fundamentally from the ordinary view only in this, that it discards the idea of a common descent as the real bond of union among the individuals of a species, and also the idea of a local origin,—supposing, instead, that each species originated simultaneously, generally speaking over the whole geographical area it now occupies or has occupied, and

* "Species tot sunt, quot diversas formas ab initio produxit Infinitum Ens; quæ formæ, secundum generationis inditas leges, produxere plures, at sibi semper similes." —*Linn. Phil. Bot.*, 99, 157.

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in perhaps as many individuals as it numbered at any subsequent period.

Mr. Darwin, on the other hand, holds the orthodox view of the descent of all the individuals of a species not only from a local birth-place, but from a single ancestor or pair; and that each species has extended and established itself, through natural agencies, wherever it could; so that the actual geographical distribution of any species is by no means a primordial arrangement, but a natural result. He goes farther, and this volume is a protracted argument intended to prove that the species we recognize have not been independently created, as such, but have descended, like varieties, from other species. Varieties, on this view, are incipient or possible species: species are varieties of a larger growth and a wider and earlier divergence from the parent stock: the difference is one of degree, not of kind.

The ordinary view—rendering unto Cæsar the things that are Cæsar's—looks to natural agencies for the actual distribution and perpetuation of species, to a supernatural for their origin.

The theory of Agassiz regards the origin of species and their present general distribution over the world as equally primordial, equally supernatural; that of Darwin, as equally derivative, equally natural.

The theory of Agassiz, referring as it does the phenomena both of origin and distribution directly to the Divine will,—thus removing the latter with the former out of the domain of inductive science (in which efficient cause is not the first, but the last word),—may be said to be theistic to excess. The contrasted theory is not open to this objection. Studying the facts and phenomena in reference to proximate causes, and endeavoring to trace back the series of cause and effect as far as possible, Darwin's aim and processes are strictly scientific, and his endeavor, whether successful or futile, must be regarded as a legitimate attempt to extend the domain of natural or physical science. For though it well may be that "organic forms have no physical or secondary cause," yet this can be proved only indirectly, by the failure of every attempt to refer the phenomena in question to causal laws. But, however originated, and whatever be thought of Mr. Darwin's arduous undertaking in this respect, it is certain that plants and animals are subject from their birth to physical influences, to which they have to accommodate themselves as they can. How literally they are "born to trouble," and how incessant and severe the struggle for life generally is, the present volume graphically describes. Few will deny that such influences must have gravely affected the range and the association of individuals and species on the earth's surface. Mr. Darwin thinks that, acting upon an inherent predisposition to vary, they have sufficed even to modify

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the species themselves and produce the present diversity. Mr. Agassiz believes that they have not even affected the geographical range and the actual association of species, still less their forms; but that every adaptation of species to climate and of species to species is as aboriginal, and therefore as inexplicable, as are the organic forms themselves.

Who shall decide between such extreme views so ably maintained on either hand, and say how much of truth there may be in each? The present reviewer has not the presumption to undertake such a task. Having no prepossession in favor of naturalistic theories, but struck with the eminent ability of Mr. Darwin's work, and charmed with its fairness, our humbler duty will be performed if, laying aside prejudice as much as we can, we shall succeed in giving a fair account of its method and argument, offering by the way a few suggestions, such as might occur to any naturalist of an inquiring mind. An editorial character for this article must in justice be disclaimed. The plural pronoun is employed not to give editorial weight, but to avoid even the appearance of egotism, and also the circumlocution which attends a rigorous adherence to the impersonal style.

We have contrasted these two extremely divergent theories, in their broad statements. It must not be inferred that they have no points nor ultimate results in common.

In the first place they practically agree in upsetting, each in its own way, the generally received definition of species, and in sweeping away the ground of their objective existence in Nature. The orthodox conception of species is that of lineal descent: all the descendants of a common parent, and no other, constitute a species; they have a certain identity because of their descent, by which they are supposed to be recognizable. So naturalists had a distinct idea of what they meant by the term species, and a practical rule, which was hardly the less useful because difficult to apply in many cases, and because its application was indirect,—that is, the community of origin had to be inferred from the likeness; that degree of similarity, and that only, being held to be conspecific which could be shown or reasonably inferred to be compatible with a common origin. And the usual concurrence of the whole body of naturalists (having the same data before them) as to what forms are species attests the value of the rule, and also indicates some real foundation for it in nature. But if species were created in numberless individuals over broad spaces of territory, these individuals are connected only in idea, and species differ from varieties on the one hand and from genera, tribes, &c. on the other only in degree; and no obvious natural reason remains for fixing upon this or that degree as specific, at least no natural standard, by which the opinions of different naturalists may be correlated.

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Species upon this view are enduring, but subjective and ideal. Any three or more of the human races, for example, are species or not species, according to the bent of the naturalist's mind. Darwin's theory brings us the other way to the same result. In his view, not only all the individuals of a species are descendants of a common parent but of all the related species also. Affinity, relationship, all the terms which naturalists use figuratively to express an underived, unexplained resemblance among species, have a literal meaning upon Darwin's system, which they little suspected, namely, that of inheritance. Varieties are the latest offshoots of the genealogical tree in "an unlineal" order; species, those of an earlier date, but of no definite distinction; genera, more ancient species, and so on. The human races, upon this view likewise may or may not be species according to the notions of each naturalist as to what differences are specific: but, if not species already, those races that last long enough are sure to become so. It is only a question of time.

How well the simile of a genealogical tree illustrates the main ideas of Darwin's theory the following extract from the summary of the fourth chapter shows.

"It is a truly wonderful fact,—the wonder of which we are apt to overlook from familiarity—that all animals and all plants throughout all time and space should be related to each other in group subordinate to group, in the manner which we everywhere behold—namely, varieties of the same species most closely related together, species of the same genus less closely and unequally related together, forming sections and sub-genera, species of distinct genera much less closely related, and genera related in different degrees, forming sub-families, families, orders, sub-classes, and classes. The several subordinate groups in any class cannot be ranked in a single file, but seem rather to be clustered round points, and these round other points, and so on in almost endless cycles. On the view that each species has been independently created, I can see no explanation of this great fact in the classification of all organic beings; but, to the best of my judgment, it is explained through inheritance and the complex action of natural selection, entailing extinction and divergence of character, as we have seen illustrated in the diagram.

"The affinities of all the beings of the same class have sometimes been represented by a great tree. I believe this simile largely speaks the truth. The green and budding twigs may represent existing species; and those produced during each former year may represent the long succession of extinct species. At each period of growth all the growing twigs have tried to branch out on all sides, and overtop and kill the surrounding twigs and branches, in the same manner as species and groups of species have tried to overmaster other species in the great battle for life. The limbs divided into great branches, and these into lesser and lesser branches, were themselves once, when the tree was small, budding twigs; and this connexion of the former and present buds by ramifying branches may well represent the classification of all extinct and living species in groups subordinate to groups. Of the many twigs which

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flourished when the tree was a mere bush, only two or three, now grown into great branches, yet survive and bear all the other branches; so with the species which lived during long-past geological periods, very few now have living and modified descendants. From the first growth of the tree, many a limb and branch has decayed and dropped off; and these lost branches of various sizes may represent those whole orders, families, and genera which have now no living representatives, and which are known to us only from having been found in a fossil state. As we here and there see a thin straggling branch springing from a fork low down in a tree, and which by some chance has been favored and is still alive on its summit, so we occasionally see an animal like the Ornithorhynchus or Lepidosiren, which in some small degree connects by its affinities two large branches of life, and which has apparently been saved from fatal competition by having inhabited a protected station. As buds give rise by growth to fresh buds, and these, if vigorous, branch out and overtop on all sides many a feebler branch, so by generation I believe it has been with the great Tree of Life, which fills with its dead and broken branches the crust of the earth, and covers the surface with its ever branching and beautiful ramifications."

It may also be noted that there is a significant correspondence between the rival theories as to the main facts employed. Apparently every capital fact in the one view is a capital fact in the other. The difference is in the interpretation. To run the parallel ready made to our hands:*

"The simultaneous existence of the most diversified types under identical circumstances, ... the repetition of similar types under the most diversified circumstances, ... the unity of plan in otherwise highly diversified types of animals, ... the correspondence, now generally known as special homologies, in the details of structure otherwise entirely disconnected, down to the most minute peculiarities, ... the various degrees and different kinds of relationship among animals which [apparently] can have no genealogical connection, ... the simultaneous existence in the earliest geological periods....of representatives of all the great types of the animal kingdom, ... the gradation based upon complications of structure which may be traced among animals built upon the same plan; the distribution of some types over the most extensive range of surface of the globe, while others are limited to particular geographical areas, ... the identity of structures of these types, notwithstanding their wide geographical distribution, ... the community of structure in certain respects of animals otherwise entirely different, but living within the same geographical area, ... the connection by series of special structures observed in animals widely scattered over the surface of the globe, ... the definite relations in which animals stand to the surrounding world, ... the relations in which individuals of the same species stand to one another, ... the limitation of the range of changes which animals undergo during their growth, ... the return to a definite norm of animals which multiply in various ways, ... the order of succession of the different types of animals and plants characteristic of the

* Agassiz, Essay on Classification; Contrib. to Nat. Hist., i, p. 132, et seq.

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different geological epochs, ... the localization of some types of animals upon the same points of the surface of the globe during several successive geological periods; .... the parallelism between the order of succession of animals and plants in geological times, and the gradation among their living representatives, ... the parallelism between the order of succession of animals in geological times and the changes their living representatives undergo during their embryological growth,* .... *the combination in many extinct types of characters which in later ages appear disconnected in different types*, ... the parallelism between the gradation among animals and the changes they undergo during their growth, ... the relations existing between these different series and the geographical distribution of animals, ... the connection of all the known features of nature into one system,—"

In a word, the whole relations of animals, &c. to surrounding nature and to each other, are regarded under the one view as ultimate facts, or in their ultimate aspect, and interpreted theologically;—under the other as complex facts, to be analyzed and interpreted scientifically. The one naturalist, perhaps too largely assuming the scientifically unexplained to be inexplicable, views the phenomena only in their supposed relation to the Divine mind. The other, naturally expecting many of these phenomena to be resolvable under investigation, views them in their relations to one another, and endeavors to explain them as far as he can (and perhaps farther) through natural causes.

But does the one really exclude the other? Does the investigation of physical causes stand opposed to the theological view and the study of the harmonies between mind and Nature? More than this, is it not most presumable that an intellectual conception realized in nature would be realized through natural agencies? Mr. Agassiz answers these questions affirmatively when he declares that "the task of science is to investigate what has been done, to enquire if possible *how it has been done*, rather than to ask what is possible for the Deity, since *we can know that only by what actually exists*;" and also when he extends the argument for the intervention in nature of a creative mind to its legitimate application in the inorganic world; which, he remarks, "considered in the same light, would not fail also to exhibit unexpected evidence of thought, in the character of the laws regulating the chemical combinations, the action of physi-

* As to this, Darwin remarks that he can only hope to see the law hereafter proved true (p. 449); and p. 338: "Agassiz insists that ancient animals resemble to a certain extent the embryos of recent animals of the same classes; or that the geological succession of extinct forms is in some degree parallel to the embryological development of recent forms. I must follow Pictet and Huxley in thinking that the truth of this doctrine is very far from proved. Yet I fully expect to see it hereafter confirmed, at least in regard to subordinate groups, which have branched off from each other within comparatively recent times. For this doctrine of Agassiz accords well with the theory of natural selection."

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cal forces, etc., etc."* Mr. Agassiz, however, pronounces that "the connection between the facts is *only intellectual*;"—an opinion which the analogy of the inorganic world, just referred to, does not confirm, for there a material connection between the facts is justly held to be consistent with an intellectual,—and which the most analogous cases we can think of in the organic world do not favor; for there is a material connection between the grub, the pupa, and the butterfly, between the tadpole and the frog, or, still better, between those distinct animals which succeed each other in alternate and very dissimilar generations. So that mere analogy might rather suggest a natural connection than the contrary; and the contrary cannot be demonstrated until the possibilities of nature under the Deity are fathomed.

But the intellectual connection being undoubted, Mr. Agassiz properly refers the whole to "the agency of Intellect as its first cause." In doing so, however, he is not supposed to be offering a scientific explanation of the phenomena. Evidently he is considering only the ultimate *why*, not the proximate why or *how*.

Now the latter is just what Mr. Darwin is considering. He conceives of a physical connection between allied species: but we suppose he does not deny their intellectual connection, as related to a Supreme Intelligence. Certainly we see no reason why he should, and many reasons why he should not. Indeed, as we contemplate the actual direction of investigation and speculation in the physical and natural sciences, we dimly apprehend a probable synthesis of these divergent theories, and in it the ground for a strong stand against mere naturalism. Even if the doctrine of the origin of species through natural selection should prevail in our day, we shall not despair; being confident that the genius of an Agassiz will be found equal to the work of constructing, upon the mental and material foundations combined, a theory of nature as theistic and as scientific, as that which he has so eloquently expounded.

To conceive the possibility of "the descent of species from species by insensibly fine gradations" during a long course of time, and to demonstrate its compatibility with a strictly theistic view of the universe, is one thing: to substantiate the theory itself or show its likelihood is quite another thing. This brings us to consider what Darwin's theory actually is, and how he supports it.

That the existing kinds of animals and plants, or many of them, may be derived from other and earlier kinds, in the lapse

* Op. cit., p. 131.—One or two Bridgewater Treatises, and most modern works upon Natural Theology should have rendered the evidences of thought in inorganic nature not "unexpected."

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of time, is by no means a novel proposition. Not to speak of ancient speculations of the sort, it is the well-known Lamarckian theory. The first difficulty which such theories meet with is that, in the present age, with all its own and its inherited prejudgments, the whole burden of proof is naturally, and indeed properly, laid upon the shoulders of the propounders; and thus far the burden has been more than they could bear. From the very nature of the case, substantive proof of specific creation is not attainable; but that of derivation or transmutation of species may be. He who affirms the latter view is bound to do one or both of two things. Either, 1, to assign real and adequate causes, the natural or necessary result of which must be to produce the present diversity of species and their actual relations; or, 2, to show the general conformity of the whole body of facts to such assumption, and also to adduce instances explicable by it and inexplicable by the received view,—so perhaps winning our assent to the doctrine, through its competency to harmonize all the facts, even though the cause of the assumed variation remain as occult as that of the transformation of tadpoles into frogs, or that of *Coryne* into *Sarzia*.

The first line of proof, successfully carried out, would establish derivation as a true physical theory; the second, as a sufficient hypothesis.

Lamarck mainly undertook the first line, in a theory which has been so assailed by ridicule that it rarely receives the credit for ability to which in its day it was entitled. But he assigned partly unreal, partly insufficient causes; and the attempt to account for a progressive change in species through the direct influence of physical agencies, and through the appetencies and habits of animals reacting upon their structure, thus causing the production and the successive modification of organs, is a conceded and total failure. The shadowy author of the Vestiges of the Natural History of Creation can hardly be said to have undertaken either line, in a scientific way. He would explain the whole progressive evolution of nature by virtue of an inherent tendency to development,—thus giving us an idea or a word in place of a natural cause, a restatement of the proposition instead of an explanation. Mr. Darwin attempts both lines of proof, and in a strictly scientific spirit; but the stress falls mainly upon the first; for, as he does assign real causes, he is bound to prove their adequacy.

It should be kept in mind that, while all direct proof of independent origination is unattainable from the nature of the case, the overthrow of particular schemes of derivation has not established the opposite proposition. The futility of each hypothesis thus far proposed to account for derivation may be made apparent, or unanswerable objections may be urged against

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it; and each victory of the kind may render derivation more improbable, and therefore specific creation more probable, without settling the question either way. New facts, or new arguments and a new mode of viewing the question may some day change the whole aspect of the case. It is with the latter that Mr. Darwin now reopens the discussion.

Having conceived the idea that varieties are incipient species, he is led to study variation in the field where it shows itself most strikingly and affords the greatest facilities to investigation. Thoughtful naturalists have had increasing grounds to suspect that a re-examination of the question of species in zoology and botany, commencing with those races which man knows most about, viz. the domesticated and cultivated races, would be likely somewhat to modify the received idea of the entire fixity of species. This field, rich with various but unsystematized stores of knowledge accumulated by cultivators and breeders, has been generally neglected by naturalists, because these races are not in a state of nature; whereas they deserve particular attention on this very account, as experiments, or the materials for experiments, ready to our hand. In domestication we vary some of the natural conditions of a species, and thus learn experimentally what changes are within the reach of varying conditions in nature. We separate and protect a favorite race against its foes or its competitors, and thus learn what it might become if nature ever afforded it equal opportunities. Even when, to subserve human uses, we modify a domesticated race to the detriment of its native vigor, or to the extent of practical monstrosity, although we secure forms which would not be originated and could not be perpetuated in free nature, yet we attain wider and juster views of the possible degree of variation. We perceive that some species are more variable than others, but that no species subjected to the experiment persistently refuses to vary; and that when it has once begun to vary, its varieties are not the less but the more subject to variation. "No case is on record of a variable being ceasing to be variable under cultivation." It is fair to conclude, from the observation of plants and animals in a wild as well as domesticated state, that the tendency to vary is general, and even universal. Mr. Darwin does "not believe that variability is an inherent and necessary contingency, under all circumstances, with all organic beings, as some authors have thought." No one supposes variation could occur under all circumstances; but the facts on the whole imply an universal tendency, ready to be manifested under favorable circumstances. In reply to the assumption that man has chosen for domestication animals and plants having an extraordinary inherent tendency to vary, and likewise to withstand diverse climates, it is asked:

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"How could a savage possibly know, when he first tamed an animal, whether it would vary in succeeding generations, and whether it would endure other climates? Has the little variability of the ass or guinea-fowl, or the small power of endurance of warmth by the rein-deer, or of cold by the common camel, prevented their domestication? I cannot doubt that if other animals and plants, equal in number to our domesticated productions, and belonging to equally diverse classes and countries, were taken from a state of nature, and could be made to breed for an equal number of generations under domestication, they would vary on an average as largely as the parent species of our existing domesticated productions have varied."

As to amount of variation, there is the common remark of naturalists that the varieties of domesticated plants or animals often differ more widely than do the individuals of distinct species in a wild state: and even in nature the individuals of some species are known to vary to a degree sensibly wider than that which separates related species. In his instructive section on the breeds of the domestic pigeon, our author remarks that:—"at least a score of pigeons might be chosen, which if shown to an ornithologist, and he were told that they were wild birds, would certainly be ranked by him as well defined species. Moreover, I do not believe that any ornithologist would place the English carrier, the short-faced tumbler, the runt, the barb, pouter, and fantail in the same genus; more especially as in each of these breeds several truly inherited sub-breeds, or species as he might have called them, could be shown him." That this is not a case like that of dogs, in which probably the blood of more than one species is mingled, Mr. Darwin proceeds to show, adducing cogent reasons for the common opinion that all have descended from the wild rock-pigeon. Then follow some suggestive remarks:—

"I have discussed the probable origin of domestic pigeons at some, yet quite insufficient, length; because when I first kept pigeons and watched the several kinds, knowing well how true they bred, I felt fully as much difficulty in believing that they could ever have descended from a common parent, as any naturalist could in coming to a similar conclusion in regard to many species of finches, or other large groups of birds, in nature. One circumstance has struck me much; namely, that all the breeders of the various domestic animals and the cultivators of plants, with whom I have ever conversed, or whose treatises I have read, are firmly convinced that the several breeds to which each has attended, are descended from so many aboriginally distinct species. Ask, as I have asked, a celebrated raiser of Hereford cattle, whether his cattle might not have descended from long horns, and he will laugh you to scorn. I have never met a pigeon, or poultry, or duck, or rabbit fancier, who was not fully convinced that each main breed was descended from a distinct species. Van Mons, in his treatise on pears and apples, shows how utterly he disbelieves that the several sorts, for instance a Ribston-pippin

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or Codlin-apple, could ever have proceeded from the seeds of the same tree. Innumerable other examples could be given. The explanation, I think, is simple: from long-continued study they are strongly impressed with the differences between the several races; and though they well know that each race varies slightly, for they win their prizes by selecting such slight differences, yet they ignore all general arguments, and refuse to sum up in their minds slight differences accumulated during many successive generations. May not those naturalists who, knowing far less of the laws of inheritance than does the breeder, and knowing no more than he does of the intermediate links in the long lines of descent, yet admit that many of our domestic races have descended from the same parents—may they not learn a lesson of caution, when they deride the idea of species in a state of nature being lineal descendants of other species?"

The actual causes of variation are unknown. Mr. Darwin favors the opinion of the late Mr. Knight, the great philosopher of horticulture, that variability under domestication is somehow connected with excess of food. He also regards the unknown cause as acting chiefly upon the reproductive system of the parents, which system, judging from the effect of confinement or cultivation upon its functions, he concludes to be more susceptible than any other to the action of changed conditions of life. The tendency to vary certainly appears to be much stronger under domestication than in free nature. But we are not sure that the greater variableness of cultivated races is not mainly owing to the far greater opportunities for manifestation and accumulation—a view seemingly all the more favorable to Mr. Darwin's theory. The actual amount of certain changes, such as size or abundance of fruit, size of udder, stands of course in obvious relation to supply of food.

Really, we no more know the reason why the progeny occasionally deviates from the parent than we do why it usually resembles it. Though the laws and conditions governing variation are known to a certain extent, while those governing inheritance are apparently inscrutable. "Perhaps," Darwin remarks, "the correct way of viewing the whole subject would be, to look at the inheritance of every character whatever as the rule, and non-inheritance as the anomaly." This, from general and obvious considerations, we have long been accustomed to do. Now, as exceptional instances are expected to be capable of explanation, while ultimate laws are not, it is quite possible that variation may be accounted for, while the great primary law of inheritance remains a mysterious fact.

The common proposition is, that *species reproduce their like;* this is a sort of general inference, only a degree closer to fact than the statement that genera reproduce their like. The true proposition, the fact incapable of further analysis is, that *individ-*

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uals reproduce their like,—that characteristics are inheritable. So varieties, or deviations once originated, are perpetuable, like species. Not so likely to be perpetuated, at the outset; for the new form tends to resemble a grand-parent and a long line of similar ancestors, as well as to resemble its immmediate progenitors. Two forces which coincide in the ordinary case, where the offspring resembles its parent, act in different directions when it does not, and it is uncertain which will prevail. If the remoter, but very potent ancestral influence predominates, the variation disappears with the life of the individual. If that of the immediate parent—feebler no doubt, but closer—the variety survives in the offspring; whose progeny now has a redoubled tendency to produce its own like; whose progeny again is almost sure to produce its like, since it is much the same whether it takes after its mother or its grandmother.

In this way races arise, which under favorable conditions may be as hereditary as species. In following these indications, watching opportunities, and breeding only from those individuals which vary most in a desirable direction, man leads the course of variation as he leads a streamlet,—apparently at will, but never against the force of gravitation,—to a long distance from its source, and makes it more subservient to his use or fancy. He unconsciously strengthens those variations which he prizes when he plants the seed of a favorite fruit, preserves a favorite domestic animal, drowns the uglier kittens of a litter, and allows only the handsomest or the best mousers to propagate. Still more, by methodical selection, in recent times almost marvellous results have been produced in new breeds of cattle, sheep, and poultry, and new varieties of fruit of greater and greater size or excellence.

It is said that all domestic varieties if left to run wild, would revert to their aboriginal stocks. Probably they would whereever various races of one species were left to commingle. At least the abnormal or exaggerated characteristics induced by high feeding, or high cultivation, and prolonged close breeding would promptly disappear, and the surviving stock would soon blend into a homogeneous result (in a way presently explained), which would naturally be taken for the original form; but we could seldom know if it were so. It is by no means certain that the result would be the same if the races ran wild each in a separate region. Dr. Hooker doubts if there is a true reversion in the case of plants. Mr. Darwin's observations rather favor it in the animal kingdom. With mingled races reversion seems well made out in the case of pigeons. The common opinion upon this subject therefore probably has some foundation. But even if we regard varieties as oscillations around a primitive centre or

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type, still it appears from the readiness with which such varieties originate, that a certain amount of disturbance would carry them beyond the influence of the primordial attraction, where they may become new centres of variation.

Some suppose that races cannot be perpetuated indefinitely even by keeping up the conditions under which they were fixed: but the high antiquity of several, and the actual fixity of many of them, negative this assumption. "To assert that we could not breed our cart and race horses, long and short-horned cattle, and poultry of various breeds, for almost an infinite number of generations would be opposed to all experience."

Why varieties develope so readily and deviate so widely under domestication, while they are apparently so rare or so transient in free nature, may easily be shown. In nature, even with hermaphrodite plants, there is a vast amount of cross fertilization among various individuals of the same species. The inevitable result of this (as was long ago explained in this Journal*) is to repress variation, to keep the mass of a species comparatively homogeneous over any area in which it abounds in individuals. Starting from a suggestion of the late Mr. Knight, now so familiar, that close interbreeding diminishes vigor and fertility†; and perceiving that bisexuality is ever aimed at in nature,—being attained physiologically in numerous cases where it is not structurally,—Mr. Darwin has worked out the subject in detail, and shown how general is the concurrence, either habitual or occasional, of two hermaphrodite individuals in the reproduction of their kind; and has drawn the philosophical inference that probably no organic being self-fertilizes indefinitely; but that a cross with another individual is occasionally—perhaps at very long intervals—indispensable. We refer the reader to the section on the intercrossing of individuals (p. 96–101), and also to an article in the Gardeners' Chronicle a year and a half ago, for the details of a very interesting contribution to science, irrespective of theory.

In domestication, this intercrossing may be prevented; and in this prevention lies the art of producing varieties. But "the art itself is Nature," since the whole art consists in allowing the most universal of all natural tendencies in organic things (inheritance) to operate uncontrolled by other and obviously incidental tendencies. No new power, no artificial force is brought into play either by separating the stock of a desirable variety so as to prevent mixture, or by selecting for breeders those indi-

* Vol. xvii, [2], 1854, p. 13.

† We suspect that this is not an ultimate fact, but a natural consequence of inheritance,—the inheritance of disease or of tendency to disease, which close interbreeding perpetuates and accumulates, but wide breeding may neutralize or eliminate.

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viduals which most largely partake of the peculiarities for which the breed is valued.*

We see everywhere around us the remarkable results which Nature may be said to have brought about under artificial selection and separation. Could she accomplish similar results when left to herself? Variations might begin, we know they do begin, in a wild state. But would any of them be preserved and carried to an equal degree of deviation? Is there anything in nature which in the long run may answer to artificial selection? Mr. Darwin thinks that there is; and *Natural Selection* is the key-note of his discourse.

As a preliminary, he has a short chapter to show that there is variation in nature, and therefore something for natural selection to act upon. He readily shows that such mere variations as may be directly referred to physical conditions (like the depauperation of plants in a sterile soil, or their dwarfing as they approach an alpine summit, the thicker fur of an animal from far northward, &c.), and also those individual differences which we everywhere recognize but do not pretend to account for, are not separable by any assignable line from more strongly marked varieties; likewise that there is no clear demarcation between the latter and subspecies, or varieties of the highest grade (distinguished from species not by any known inconstancy, but by the supposed lower importance of their characteristics); nor between these and recognized species. "These differences blend into each other in an insensible series, and the series impresses the mind with an idea of an actual passage."

This gradation from species downward is well made out. To carry it one step farther upwards, our author presents in a strong light the differences which prevail among naturalists as to what forms should be admitted to the rank of species. Some genera (and these in some countries) give rise to far more discrepancy than others; and it is concluded that the large or dominant genera are usually the most variable. In a flora so small as the British, 182 plants generally reckoned as varieties, have been ranked by some botanists as species. Selecting the British genera which include the most polymorphous forms, it appears that Babington's Flora gives them 251 species, Bentham's only 112, a difference of 139 doubtful forms. These are nearly the extreme views; but they are the views of two most capable and most experienced judges, in respect to one of the best known floras of the world. The fact is suggestive, that the best known countries furnish the greatest number of such doubtful cases.

* The rules and processes of breeders of animals, and their results, are so familiar that they need not be particularized. Less is popularly known about the production of vegetable races. We refer our readers back to this Journal, xxvii, pp. 440–442 (May, 1859) for an abstract of the papers of M. Vilmorin upon this subject.

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Illustrations of this kind may be multiplied to a great extent. They make it plain that, whether species in nature are aboriginal and definite or not, our practical conclusions about them, as embodied in systematic works, are not *facts* but *judgments*, and largely fallible judgments.

How much of the actual coincidence of authorities is owing to imperfect or restricted observation, and to one naturalist's adopting the conclusions of another without independent observation, this is not the place to consider. It is our impression that species of animals are more definitely marked than those of plants; this may arise from our somewhat extended acquaintance with the latter, and our ignorance of the former. But we are constrained by our experience to admit the strong likelihood, in botany, that varieties on the one hand and what are called closely related species on the other do not differ except in degree. Whenever the wider difference separating the latter can be spanned by intermediate forms, as it sometimes is, no botanist long resists the inevitable conclusion. Whenever, therefore, this wider difference can be shown to be compatible with community of origin, and explained through natural selection or in any other way, we are ready to adopt the *probable* conclusion; and we see beforehand how strikingly the actual geographical association of related species favors the broader view. Whether we should continue to regard the forms in question as distinct species, depends upon what meaning we shall finally attach to that term; and that depends upon how far the doctrine of derivation can be carried back and how well it can be supported.

In applying his principle of natural selection to the work in hand, Mr. Darwin assumes, as we have seen: 1, some variability of animals and plants in nature; 2, the absence of any definite distinction between slight variations, and varieties of the highest grade; 3, the fact that naturalists do not practically agree, and do not increasingly tend to agree, as to what forms are species and what are strong varieties, thus rendering it probable that there may be no essential and original difference, or no possibility of ascertaining it, at least in many cases; also, 4, that the most flourishing and dominant species of the larger genera on an average vary most (a proposition which can be substantiated only by extensive comparisons, the details of which are not given);—and, 5, that in large genera the species are apt to be closely but unequally allied together, forming little clusters round certain species,—just such clusters as would be formed if we suppose their members once to have been satellites or varieties of a central or parent species, but to have attained at length a wider divergence and a specific character. The fact of such association is undeniable; and the use which Mr. Darwin makes of it seems fair and natural.

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The gist of Mr. Darwin's work is to show that such varieties are gradually diverged into species and genera through *natural selection;* that natural selection is the inevitable result of the *struggle for existence* which all living things are engaged in; and that this struggle is an unavoidable consequence of several natural causes, but mainly of the high rate at which all organic beings tend to increase.

Curiously enough, Mr. Darwin's theory is grounded upon the doctrine of Malthus and the doctrine of Hobbes. The elder DeCandolle had conceived the idea of the struggle for existence, and in a passage which would have delighted the cynical philosopher of Malmesbury, had declared that all nature is at war, one organism with another or with external nature; and Lyell and Herbert had made considerable use of it. But Hobbes in his theory of society and Darwin in his theory of natural history, alone have built their systems upon it. However moralists and political economists may regard these doctrines in their original application to human society and the relation of population to subsistence, their thorough applicability to the great society of the organic world in general is now undeniable. And to Mr. Darwin belongs the credit of making this extended application, and of working out the immensely diversified results with rare sagacity and untiring patience. He has brought to view *real causes* which have been largely operative in the establishment of the actual association and geographical distribution of plants and animals. In this he must be allowed to have made a very important contribution to an interesting department of science, even if his theory fails in the endeavor to explain the origin or diversity of species.

"Nothing is easier," says our author, "than to admit in words the truth of the universal struggle for life, or more difficult—at least I have found it so—than constantly to bear this conclusion in mind. Yet unless it be thoroughly engrained in the mind, I am convinced that the whole economy of nature, with every fact on distribution, rarity, abundance, extinction, and variation, will be dimly seen or quite misunderstood. We behold the face of nature bright with gladness, we often see superabundance of food; we do not see, or we forget, that the birds which are idly singing round us mostly live on insects or seeds, and are thus constantly destroying life; or we forget how largely these songsters, or their eggs, or their nestlings, are destroyed by birds and beasts of prey; we do not always bear in mind, that though food may be now superabundant, it is not so at all seasons of each recurring year."—p. 62.

"There is no exception to the rule that every organic being naturally increases at so high a rate, that if not destroyed, the earth would soon be covered by the progeny of a single pair. Even slow-breeding man has doubled in twenty-five years, and at this rate, in a few thousand years, there would literally not be standing room for his progeny. Linnæus has calculated that if an annual plant produced only two seeds—

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and there is no plant so unproductive as this—and their seedlings next year produced two, and so on, then in twenty years there would be a million plants. The elephant is reckoned to be the slowest breeder of all known animals, and I have taken some pains to estimate its probable minimum rate of natural increase: it will be under the mark to assume that it breeds when thirty years old, and goes on breeding till ninety years old, bringing forth three pairs of young in this interval; if this be so, at the end of the fifth century there would be alive fifteen million elephants, descended from the first pair.

"But we have better evidence on this subject than mere theoretical calculations, namely, the numerous recorded cases of the astonishingly rapid increase of various animals in a state of nature, when circumstances have been favorable to them during two or three following seasons. Still more striking is the evidence from our domestic animals of many kinds which have run wild in several parts of the world; if the statements of the rate of increase of slow-breeding cattle and horses in South America, and latterly in Australia, had not been well authenticated, they would have been quite incredible. So it is with plants: cases could be given of introduced plants which have become common throughout whole islands in a period of less than ten years. Several of the plants now most numerous over the wide plains of La Plata, clothing square leagues of surface almost to the exclusion of all other plants, have been introduced from Europe; and there are plants which now range in India, as I hear from Dr. Falconer, from Cape Comorin to the Himalaya, which have been imported from America since its discovery. In such cases, and endless instances could be given, no one supposes that the fertility of these animals or plants has been suddenly and temporarily increased in any sensible degree. The obvious explanation is that the conditions of life have been very favorable, and that there has consequently been less destruction of the old and young, and that nearly all the young have been enabled to breed. In such cases the geometrical ratio of increase, the result of which never fails to be surprising, simply explains the extraordinarily rapid increase and wide diffusion of naturalized productions in their new homes."—pp. 64, 65.

"All plants and animals are tending to increase at a geometrical ratio; all would most rapidly stock any station in which they could anyhow exist; the increase must be checked by destruction at some period of life."—p. 65.

The difference between the most and the least prolific species is of no account.

"The condor lays a couple of eggs, and the ostrich a score; and yet in the same country the condor may be the more numerous of the two. The Fulmar petrel lays but one egg, yet it is believed to be the most numerous bird in the world."—p. 68.

"The amount of food gives the extreme limit to which each species can increase; but very frequently it is not the obtaining of food, but the serving as prey to other animals, which determines the average numbers of a species."—p. 68.

"Climate plays an important part in determining the average numbers of a species, and periodical seasons of extreme cold or drought, I believe

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to be the most effective of all checks. I estimated that the winter of 1854–55 destroyed four-fifths of the birds in my own grounds; and this is a tremendous destruction, when we remember that ten per cent is an extraordinarily severe mortality from epidemics with man. The action of climate seems at first sight to be quite independent of the struggle for existence; but in so far as climate chiefly acts in reducing food, it brings on the most severe struggle between the individuals, whether of the same or of distinct species, which subsist on the same kind of food. Even when climate, for instance extreme cold, acts directly, it will be the least vigorous, or those which have got least food through the advancing winter, which will suffer most. When we travel from south to north, or from a damp region to a dry, we invariably see some species gradually getting rarer and rarer, and finally disappearing; and the change of climate being conspicuous, we are tempted to attribute the whole effect to its direct action. But this is a very false view: we forget that each species, even where it most abounds, is constantly suffering enormous destruction at some period of its life, from enemies or from competitors for the same place and food; and if these enemies or competitors be in the least degree favored by any slight change of climate, they will increase in numbers, and, as each area is already stocked with inhabitants, the other species will decrease. When we travel southward and see a species decreasing in numbers, we may feel sure that the cause lies quite as much in other species being favored, as in this one being hurt. So it is when we travel northward, but in a somewhat lesser degree, for the number of species of all kinds, and therefore of competitors, decreases northwards; hence in going northward, or in ascending a mountain, we far oftener meet with stunted forms, due to the *directly* injurious action of climate, than we do in proceeding southwards or in descending a mountain. When we reach the Arctic regions, or snow-capped summits, or absolute deserts, the struggle for life is almost exclusively with the elements.

"That climate acts in main part indirectly by favoring other species, we may clearly see in the prodigious number of plants in our gardens which can perfectly well endure our climate, but which never become naturalized, for they cannot compete with our native plants, nor resist destruction by our native animals."—pp. 68, 69.

After an instructive instance in which "cattle absolutely determine the existence of the Scotch Fir," we are referred to cases in which insects determine the existence of cattle.

"Perhaps Paraguay offers the most curious instance of this; for here neither cattle nor horses nor dogs have ever run wild, though they swarm southward and northward in a feral state; and Azara and Rengger have shown that this is caused by the greater number in Paraguay of a certain fly, which lays its eggs in the navels of these animals when first born. The increase of these flies, numerous as they are, must be habitually checked by some means, probably by birds. Hence, if certain insectivorous birds (whose numbers are probably regulated by hawks or beasts of prey) were to increase in Paraguay, the flies would decrease—then cattle and horses would become feral, and this would certainly

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greatly alter (as indeed I have observed in parts of South America) the vegetation: this again would largely affect the insects; and this, as we just have seen in Staffordshire, the insectivorous birds, and so onwards in ever-increasing circles of complexity. We began this series by insectivorous birds, and we had ended with them. Not that in nature the relations can ever be as simple as this. Battle within battle must ever be recurring with varying success; and yet in the long run the forces are so nicely balanced, that the face of nature remains uniform for long periods of time, though assuredly the merest trifle would often give the victory to one organic being over another. Nevertheless so profound is our ignorance, and so high our presumption, that we marvel when we hear of the extinction of an organic being; and as we do not see the cause, we invoke cataclysms to desolate the world, or invent laws on the duration of the forms of life!"—pp. 72, 73.

"When we look at the plants and bushes clothing an entangled bank, we are tempted to attribute their proportional numbers and kinds to what we call chance. But how false a view is this! Every one has heard that when an American forest is cut down, a very different vegetation springs up; but it has been observed that the trees now growing on the ancient Indian mounds, in the Southern United States, display the same beautiful diversity and proportion of kinds as in the surrounding virgin forests. What a struggle between the several kinds of trees must here have gone on during long centuries, each annually scattering its seeds by the thousand; what war between insect and insect—between insects, snails, and other animals with birds and beasts of prey—all striving to increase, and all feeding on each other or on the trees or their seeds and seedlings, or on the other plants which first clothed the ground and thus checked the growth of the trees! Throw up a handful of feathers, and all must fall to the ground according to definite laws; but how simple is this problem compared to the action and reaction of the innumerable plants and animals which have determined, in the course of centuries, the proportional numbers and kinds of trees now growing on the old Indian ruins!"—pp. 74, 75.

For reasons obvious upon reflection the competition is often, if not generally, most severe between nearly related species when they are in contact, so that one drives the other before it, as the Hanoverian the old English rat, the small Asiatic cockroach in Russia, its greater congener, &c.: and this, when duly considered, explains many curious results;—such, for instance, as the considerable number of different genera of plants and animals which are generally found to inhabit any limited area.

"The truth of the principle, that the greatest amount of life can be supported by great diversification of structure, is seen under many natural circumstances. In an extremely small area, especially if freely open to immigration, and where the contest between individual and individual must be severe, we always find great diversity in its inhabitants. For instance, I found that a piece of turf, three feet by four in size, which had been exposed for many years to exactly the same conditions, supported twenty species of plants, and these belonged to eighteen genera and to

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eight orders, which showed how much these plants differed from each other. So it is with the plants and insects on small and uniform islets; and so in small ponds of fresh water. Farmers find that they can raise most food by a rotation of plants belonging to the most different orders; nature follows what may be called a simultaneous rotation. Most of the animals and plants which live close round any small piece of ground, could live on it (supposing it not to be in any way peculiar in its nature), and may be said to be striving to the utmost to live there; but, it is seen, that where they come into the closest competition with each other, the advantages of diversification of structure, with the accompanying differences of habit and constitution, determine that the inhabitants, which thus jostle each other most closely, shall as a general rule, belong to what we call different genera and orders."—p. 114.

The abundance of some forms, the rarity and final extinction of many others, and the consequent divergence of character or increase of difference among the surviving representatives are other consequences. As favored forms increase, the less favored must diminish in number, for there is not room for all; and the slightest advantage, at first probably inappreciable to human observation, must decide which shall prevail and which must perish, or be driven to another and for it more favorable locality.

We cannot do justice to the interesting chapter upon natural selection by separated extracts. The following must serve to show how the principle is supposed to work.

"If during the long course of ages and under varying conditions of life, organic beings vary at all in the several parts of their organization, and I think this cannot be disputed; if there be, owing to the high geometrical powers of increase of each species, at some age, season, or year, a severe struggle for life, and this certainly cannot be disputed; then, considering the infinite complexity of the relations of all organic beings to each other and to their conditions of existence, causing an infinite diversity in structure, constitution, and habits, to be advantageous to them, I think it would be a most extraordinary fact if no variation ever had occurred useful to each being's own welfare, in the same way as so many variations have occurred useful to man. But if variations useful to any organic being do occur, assuredly individuals thus characterized will have the best chance of being preserved in the struggle for life; and from the strong principle of inheritance they will tend to produce offspring similarly characterized. This principle of preservation, I have called, for the sake of brevity, Natural Selection."—pp. 126, 127.

"In order to make it clear how, as I believe, natural selection acts, I must beg permission to give one or two imaginary illustrations. Let us take the case of a wolf, which preys on various animals, securing some by craft, some by strength, and some by fleetness; and let us suppose that the fleetest prey, a deer for instance, had from any change in the country increased in numbers, or that other prey had decreased in numbers, during that season of the year when the wolf is hardest pressed for food. I can under such circumstances see no reason to doubt that the swiftest and slimmest wolves would have the best chance of surviving,

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and so be preserved or selected,—provided always that they retained strength to master their prey at this or at some other period of the year, when they might be compelled to prey on other animals. I can see no more reason to doubt this, than that man can improve the fleetness of his greyhounds by careful and methodical selection, or by that unconscious selection which results from each man trying to keep the best dogs without any thought of modifying the breed.

"Even without any change in the proportional numbers of the animals on which our wolf preyed, a cub might be born with an innate tendency to pursue certain kinds of prey. Nor can this be thought very improbable; for we often observe great differences in the natural tendencies of our domestic animals; one cat, for instance, taking to catch rats, another mice; one cat, according to Mr. St.John, bringing home winged game, another hares or rabbits, and another hunting on marshy ground and almost nightly catching woodcocks or snipes. The tendency to catch rats rather than mice is known to be inherited. Now, if any slight innate change of habit or of structure benefited an individual wolf, it would have the best chance of surviving and of leaving offspring. Some of its young would probably inherit the same habits or structure, and by the repetition of this process, a new variety might be formed which would either supplant or coexist with the parent-form of wolf. Or, again, the wolves inhabiting a mountainous district, and those frequenting the lowlands, would naturally be forced to hunt different prey; and from the continued preservation of the individuals best fitted for the two sites, two varieties might slowly be formed. These varieties would cross and blend where they met; but to this subject of intercrossing we shall soon have to return. I may add, that, according to Mr. Pierce, there are two varieties of the wolf inhabiting the Catskill Mountains in the United States, one with a light greyhound-like form, which pursues deer, and the other more bulky, with shorter legs, which more frequently attacks the shepherd's flocks."—pp. 90, 91.

We eke out the illustration here with a counterpart instance, viz., the remark of Dr. Bachman that "The deer that reside permanently in the swamps of Carolina are taller and longer-legged than those in the higher grounds."*

The limits allotted to this article are nearly reached, yet only four of the fourteen chapters of the volume have been touched. These, however, contain the fundamental principles of the theory and most of those applications of it which are capable of something like verification, relating as they do to phenomena now occurring. Some of our extracts also show how these principles are thought to have operated through the long lapse of the ages. The chapters from the sixth to the ninth inclusive are designed to obviate difficulties and objections, "some of them so grave that to this day," the author frankly says, he "can never reflect on them without being staggered." We do not wonder at it. After drawing what comfort he can from "the imperfection of

* Quadrupeds of America, ii, p. 239.

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the geological record" (chap. 9), which we suspect is scarcely exaggerated, the author considers the geological succession of organic beings (chap. 10), to see whether they better accord with the common view of the immutability of species, or with that of their slow and gradual modification. Geologists must settle that question. Then follow two most interesting and able chapters on the geographical distribution of plants and animals, the summary of which we should be glad to cite; then a fitting chapter upon classification, morphology, embryology, &c., as viewed in the light of this theory, closes the argument; the fourteenth chapter being a recapitulation.

The interest for the general reader heightens as the author advances on his perilous way and grapples manfully with the most formidable difficulties.

To account, upon these principles, for the gradual elimination and segregation of nearly allied forms,—such as varieties, subspecies, and closely related or representative species,—also in a general way for their geographical association and present range, is comparatively easy, is apparently within the bounds of possibility, and even of probability. Could we stop here we should be fairly contented. But, to complete the system, to carry out the principles to their ultimate conclusion, and to explain by them many facts in geographical distribution which would still remain anomalous, Mr. Darwin is equally bound to account for the formation of genera, families, orders, and even classes, by natural selection. He does "not doubt that the theory of descent with modification embraces all the members of the same class," and he concedes that analogy would press the conclusion still farther; while he admits that "the more distinct the forms are, the more the arguments fall away in force." To command assent we naturally require decreasing probability to be overbalanced by an increased weight of evidence. An opponent might plausibly, and perhaps quite fairly, urge that the links in the chain of argument are weakest just where the greatest stress falls upon them.

To which Mr. Darwin's answer is, that the best parts of the testimony have been lost. He is confident that intermediate forms must have existed; that in the olden times when the genera, the families and the orders diverged from their parent stocks, gradations existed as fine as those which now connect closely related species with varieties. But they have passed and left no sign. The geological record, even if all displayed to view, is a book from which not only many pages, but even whole alternate chapters have been lost out, or rather which were never printed from the autographs of nature. The record was actually made in fossil lithography only at certain times and under certain conditions (i. e., at periods of slow subsidence and places of

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abundant sediment); and of these records all but the last volume is out of print; and of its pages only local glimpses have been obtained. Geologists, except Lyell, will object to this,—some of them moderately, others with vehemence. Mr. Darwin himself admits, with a candor rarely displayed on such occasions, that he should have expected more geological evidence of transition than he finds, and that all the most eminent palæontologists maintain the immutability of species.

The general fact, however, that the fossil fauna of each period as a whole is nearly intermediate in character between the preceding and the succeeding faunas, is much relied on. We are brought one step nearer to the desired inference by the similar "fact, insisted on by all palæontologists, that fossils from two consecutive formations are far more closely related to each other, than are the fossils of two remote formations. Pictet gives a well-known instance,—the general resemblance of the organic remains from the several stages of the chalk formation, though the species are distinct at each stage. This fact alone, from its generality seems to have shaken Professor Pictet in his firm belief in the immutability of species." (p. 335.) What Mr. Darwin now particularly wants to complete his inferential evidence is a proof that the same gradation may be traced in later periods, say in the tertiary, and between that period and the present; also that the later gradations are finer, so as to leave it doubtful whether the succession is one of species,—believed on the one theory to be independent, on the other, derivative,—or of varieties, which are confessedly derivative. The proof of the finer gradation appears to be forthcoming. Des Hayes and Lyell have concluded that many of the middle tertiary, and a large proportion of the later tertiary mollusca are specifically identical with living species; and this is still the almost universally prevalent view. But Mr. Agassiz states that, "in every instance where he had sufficient materials, he had found that the species of the two epochs supposed to be identical by Des Hayes and Lyell were in reality distinct, although closely allied species."* Moreover he is now satisfied, as we understand, that the same gradation is traceable not merely in each great division of the tertiary, but in particular deposits or successive beds, each answering to a great number of years; where what have passed unquestioned as members of one species, upon closer examination of numerous specimens exhibit differences which in his opinion entitle them to be distinguished into two, three, or more species. It is plain, therefore, that whatever conclusions can be fairly drawn from the present animal and vegetable kingdoms in favor of a gradation of varieties into species, or into what

* Proceedings of the American Academy of Arts and Sciences, iv, p. 178.

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may be regarded as such, the same may be extended to the tertiary period. In both cases, what some call species others call varieties; and in the later tertiary shells this difference in judgment affects almost half of the species!

We pass to a second difficulty in the way of Mr. Darwin's theory; to a case where we are perhaps entitled to demand of him evidence of gradation like that which connects the present with the tertiary mollusca. Wide, very wide is the gap, anatomically and physiologically (we do not speak of the intellectual) between the highest quadrumana and man; and comparatively recent, if ever, must the line have bifurcated. But where is there the slightest evidence of a common progenitor? Perhaps Mr. Darwin would reply by another question: where are the fossil remains of the men who made the flint knives and arrow-heads of the Somme valley?

We have a third objection, one, fortunately, which has nothing to do with geology. We can only state it here, in brief terms. The chapter on hybridism is most ingenious, able, and instructive. If sterility of crosses is a special, original arrangement to prevent the confusion of species by mingling, as is generally assumed, then, since varieties cross readily and their offspring is fertile *inter se*, there is a fundamental distinction between varieties and species. Mr. Darwin therefore labors to show that it is not a special endowment, but an incidental acquirement. He does show that the sterility of crosses is of all degrees;—upon which we have only to say, *Natura non facit saltum*, here any more than elsewhere. But, upon his theory he is bound to show how sterility might be acquired, through natural selection or through something else. And the difficulty is, that, whereas individuals of the very same blood tend to be sterile, and somewhat remoter unions diminish this tendency, and when they have diverged into two varieties the cross-breeds between the two are more fertile than either pure stock,—yet when they have diverged only one degree more the whole tendency is reversed, and the mongrel is sterile, either absolutely or relatively. He who explains the genesis of species through purely natural agencies should assign a natural cause for this remarkable result; and this Mr. Darwin has not done. Whether original or derived, however, this arrangement to keep apart those forms which have, or have acquired (as the case may be) a certain moderate amount of difference, looks to us as much designed for the purpose, as does a ratchet to prevent reverse motion in a wheel. If species have originated by divergence, this keeps them apart.

Here let us suggest a possibly attainable test of the theory of derivation, a kind of instance which Mr. Darwin may be fairly asked to produce,—viz., an instance of two varieties, or what may

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be assumed as such, which have diverged enough to reverse the movement, to bring out some sterility in the crosses. The best marked human races might offer the most likely case. If mulattoes are sterile or tend to sterility, as some naturalists confidently assert, they afford Mr. Darwin a case in point. If, as others think, no such tendency is made out, the required evidence is wanting.

A fourth and the most formidable difficulty is that of the production and specialization of organs.

It is well said that all organic beings have been formed on two great laws; Unity of type, and Adaptation to the conditions of existence.* The special teleologists, such as Paley, occupy themselves with the latter only; they refer particular facts to special design, but leave an overwhelming array of the widest facts inexplicable. The morphologists build on unity of type, or that fundamental agreement in the structure of each great class of beings, which is quite independent of their habits or conditions of life; which requires each individual "to go through a certain formality," and to accept, at least for a time, certain organs, whether they are of any use to him or not. Philosophical minds form various conceptions for harmonizing the two views theoretically. Mr. Darwin harmonizes and explains them naturally. Adaptation to the conditions of existence is the result of Natural Selection; Unity of type, of unity of descent. Accordingly, as he puts his theory, he is bound to account for the origination of new organs, and for their diversity in each great type, for their specialization, and every adaptation of organ to function and of structure to condition, through natural agencies. Whenever he attempts this he reminds us of Lamarck, and shows us how little light the science of a century devoted to structural investigation has thrown upon the mystery of organization. Here purely natural explanations fail. The organs being given, natural selection may account for some improvement; if given of a variety of sorts or grades, natural selection might determine which should survive and where it should prevail.

On all this ground the only line for the theory to take is to make the most of gradation and adherence to type as suggestive of derivation, and unaccountable upon any other scientific view, —deferring all attempts to explain *how* such a metamorphosis was effected, until naturalists have explained *how* the tadpole is metamorphosed into a frog, or one sort of polyp into another. As to *why* it is so, the philosophy of efficient cause, and even the whole argument from design, would stand, upon the admission of such a theory of derivation, precisely where they stand without it. At least there is, or need be, no ground of differ-

* Owen adds a third, viz:—Vegetative Repetition; but this, in the vegetable kingdom is simply Unity of Type.

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ence here between Darwin and Agassiz. The latter will admit, with Owen and every morphologist, that hopeless is the attempt to explain the similarity of pattern in members of the same class by utility or the doctrine of final causes. "On the ordinary view of the independent creation of each being, we can only say that so it is, that it has so pleased the Creator to construct each animal and plant." Mr. Darwin, in proposing a theory which suggests a *how* that harmonizes these facts into a system, we trust implies that all was done wisely, in the largest sense designedly, and by an Intelligent First Cause. The contemplation of the subject on the intellectual side, the amplest exposition of the Unity of Plan in Creation, considered irrespective of natural agencies, leads to no other conclusion.

We are thus, at last, brought to the question; what would happen if the derivation of species were to be substantiated, either as a true physical theory, or as a sufficient hypothesis? What would come of it? The enquiry is a pertinent one, just now. For, of those who agree with us in thinking that Darwin has not established his theory of derivation, many will admit with us that he has rendered a theory of derivation much less improbable than before; that such a theory chimes in with the established doctrines of physical science, and is not unlikely to be largely accepted long before it can be proved. Moreover, the various notions that prevail,—equally among the most and the least religious,—as to the relations between natural agencies or phenomena and Efficient Cause, are seemingly more crude, obscure, and discordant than they need be.

It is not surprising that the doctrine of the book should be denounced as atheistical. What does surprise and concern us is, that it should be so denounced by a scientific man, on the broad assumption that a material connection between the members of a series of organized beings is inconsistent with the idea of their being intellectually connected with one another through the Deity, i. e., as products of one mind, as indicating and realizing a preconceived plan. An assumption the rebound of which is somewhat fearful to contemplate, but fortunately one which every natural birth protests against.

It would be more correct to say, that the theory in itself is perfectly compatible with an atheistic view of the universe. That is true; but it is equally true of physical theories generally. Indeed, it is more true of the theory of gravitation, and of the nebular hypothesis, than of the hypothesis in question. The latter merely takes up *a particular, proximate cause*, or set of such causes, from which, it is argued, the present diversity of species has or may have *contingently* resulted. The author does not say *necessarily* resulted; that the actual results in mode and measure, and none other must have taken place. On the other

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hand the theory of gravitation, and its extension in the nebular hypothesis, assume a *universal and ultimate* physical cause, from which the effects in nature must *necessarily* have resulted. Now it is not thought, at least at the present day, that the establishment of the Newtonian theory was a step towards atheism or pantheism. Yet the great achievement of Newton consisted in proving that certain forces, (blind forces, so far as the theory is concerned,) acting upon matter in certain directions, must *necessarily* produce planetary orbits of the exact measure and form in which observation shows them to exist;—a view which is just as consistent with eternal necessity, either in the atheistic or the pantheistic form, as it is with theism.

Nor is the theory of derivation particularly exposed to the charge of the atheism of fortuity; since it undertakes to assign real causes for harmonious and systematic results. But of this, a word at the close.

The value of such objections to the theory of derivation may be tested by one or two analogous cases. The common scientific as well as popular belief is that of the original, independent creation of oxygen and hydrogen, iron, gold, and the like. Is the speculative opinion, now increasingly held, that some or all of the supposed elementary bodies are derivative or compound, developed from some preceding forms of matter, irreligious? Were the old alchemists atheists as well as dreamers in their attempts to transmute earth into gold? Or, to take an instance from force (power),—which stands one step nearer to efficient cause than form—was the attempt to prove that heat, light, electricity, magnetism, and even mechanical power are variations or transmutations of one force, atheistical in its tendency? The supposed establishment of this view is reckoned as one of the greatest scientific triumphs of this century.

Perhaps, however, the objection is brought, not so much against the speculation itself, as against the attempt to show how derivation might have been brought about. Then the same objection applies to a recent ingenious hypothesis made to account for the genesis of the chemical elements out of the etherial medium, and to explain their several atomic weights and some other characteristics by their successive complexity,—hydrogen consisting of so many atoms of etherial substance united in a particular order, and so on. The speculation interested the philosophers of the British Association, and was thought innocent, but unsupported by facts. Surely Mr. Darwin's theory is none the worse, morally, for having some foundation in fact.

In our opinion, then, it is far easier to vindicate a theistic character for the derivative theory, than to establish the theory itself upon adequate scientific evidence. Perhaps scarcely any philosophical objection can be urged against the former to which

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the nebular hypothesis is not equally exposed. Yet the nebular hypothesis finds general scientific acceptance, and is adopted as the basis of an extended and recondite illustration in Mr. Agassiz's great work.*

How the author of this book harmonizes his scientific theory with his philosophy and theology, he has not informed us. Paley, in his celebrated analogy with the watch, insists that if the time-piece were so constructed as to produce other similar watches, after the manner of generation in animals, the argument from design would be all the stronger. What is to hinder Mr. Darwin from giving Paley's argument a further *a-fortiori* extension to the supposed case of a watch which sometimes produces better watches, and contrivances adapted to successive conditions, and so at length turns out a chronometer, a town-clock, or a series of organisms of the same type? From certain incidental expressions at the close of the volume, taken in connection with the motto adopted from Whewell, we judge it probable that our author regards the whole system of nature as one which had received at its first formation the impress of the will of its Author, foreseeing the varied yet necessary laws of its action throughout the whole of its existence, ordaining when and how each particular of the stupendous plan should be realized in effect, and—with Him to whom to will is to do—in ordaining doing it. Whether profoundly philosophical or not, a view maintained by eminent philosophical physicists and theologians, such as Babbage on the one hand and Jowett on the other, will hardly be denounced as atheism. Perhaps Mr. Darwin would prefer to express his idea in a more general way, by adopting the thoughtful words of one of the most eminent naturalists of this or any age, substituting the word *action* for 'thought,' since it is the former (from which alone the latter can be inferred) that he has been considering. "Taking nature as exhibiting thought for my guide, it appears to me that while human thought is consecutive, Divine thought is simultaneous, embracing at the same time and forever, in the past, the present and the future, the most diversified relations among hundreds of thousands of organized beings, each of which may present complications again, which, to study and understand even imperfectly,—as for instance man himself—mankind has already spent thousands of years."† In thus conceiving of the Divine Power in act as cœtaneous with Divine Thought, and of both as far as may be apart from the human element of time, our author may regard the intervention of the Creator either as, humanly speaking, *done from all time*, or else as *doing through all time*. In the ultimate analysis we suppose that every philosophical theist must adopt one or the other conception.

* Contrib. Nat. Hist. Amer., i, p. 127-131. † Op. cit., p. 130.

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A perversion of the first view leads towards atheism, the notion of an eternal sequence of cause and effect, for which there is no first cause,—a view which few sane persons can long rest in. The danger which may threaten the second view is pantheism. We feel safe from either error, in our profound conviction that there is order in the universe; that order presupposes mind; design, will; and mind or will, personality. Thus guarded, we much prefer the second of the two conceptions of causation, as the more philosophical as well as Christian view,—a view which leaves us with the same difficulties and the same mysteries in Nature as in Providence, and no other. Natural law, upon this view, is the human conception of continued and orderly Divine action.

We do not suppose that less power, or other power, is required to sustain the universe and carry on its operations, than to bring it into being. So, while conceiving no improbability of "interventions of Creative mind in nature," if by such is meant the bringing to pass of new and fitting events at fitting times, we leave it for profounder minds to establish, if they can, a rational distinction in kind between His working in nature carrying on operations, and in initiating those operations.

We wished under the light of such views, to examine more critically the doctrine of this book, especially of some questionable parts;—for instance, its explanation of the natural development of organs, and its implication of a "necessary acquirement of mental power" in the ascending scale of gradation. But there is room only for the general declaration that we cannot think the Cosmos a series which began with chaos and ends with mind, or of which mind is a result: that if by the successive origination of species and organs through natural agencies, the author means a series of events which succeed each other irrespective of a continued directing intelligence,—events which mind does not order and shape to destined ends,—then he has not established that doctrine, nor advanced towards its establishment, but has accumulated improbabilities beyond all belief. Take the formation and the origination of the successive degrees of complexity of eyes as a specimen. The treatment of this subject (pp. 188, 189), upon one interpretation is open to all the objections referred to; but if, on the other hand, we may rightly compare the eye "to a telescope, perfected by the long continued efforts of the highest human intellects," we could carry out the analogy, and draw satisfactory illustrations and inferences from it. The essential, the directly intellectual thing is the making of the improvements in the telescope or the steam-engine. Whether the successive improvements, being small at each step, and consistent with the general type of the instrument, are applied to some of the individual machines, or entire new ma-

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chines are constructed for each, is a minor matter. Though if machines could engender, the adaptive method would be most economical; and economy is said to be a paramount law in nature. The origination of the improvements, and the successive adaptations to meet new conditions or subserve other ends, are what answer to the supernatural, and therefore remain inexplicable. As to bringing them into use, though wisdom foresees the result, the circumstances and the natural competition will take care of that, in the long run. The old ones will go out of use fast enough, except where an old and simple machine remains still best adapted to a particular purpose or condition,—as, for instance, the old Newcomen engine for pumping out coal-pits. If there's a Divinity that shapes these ends, the whole is intelligible and reasonable; otherwise, not.

We regret that the necessity of discussing philosophical questions has prevented a fuller examination of the theory itself, and of the interesting scientific points which are brought to bear in its favor. One of its neatest points, certainly a very strong one for the local origination of species, and their gradual diffusion under natural agencies, we must reserve for some other convenient opportunity.

The work is a scientific one, rigidly restricted to its direct object; and by its science it must stand or fall. Its aim is, probably not to deny creative intervention in nature,—for the admission of the independent origination of certain types does away with all antecedent improbability of as much intervention as may be required,—but to maintain that Natural Selection in explaining the facts, explains also many classes of facts which thousand-fold repeated independent acts of creation do not explain, but leave more mysterious than ever. How far the author has succeeded, the scientific world will in due time be able to pronounce.

As these sheets are passing through the press a copy of the second edition has reached us. We notice with pleasure the insertion of an additional motto on the reverse of the title-page, directly claiming the theistic view which we have vindicated for the doctrine. Indeed these pertinent words of the eminently wise Bishop Butler, comprise, in their simplest expression, the whole substance of our latter pages:—

"The only distinct meaning of the word 'natural' is *stated, fixed,* or *settled;* since what is natural as much requires and presupposes an intelligent mind to render it so, i. e., to effect it continually or at stated times, as what is supernatural or miraculous does to effect it for once." A. G.

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Art. XVI.—*Forces;* by Theodore Lyman.

The first article in this Journal for November last* brings to mind the singular part which "force" now plays in science. The theory set forth in that article may be stated as follows: the world, and everything on it, may be considered as *matter;* this matter is not the same throughout, but consists of a certain number of ultimate species called *elements;* these elements are not always isolated, but are found joined to form, 1st, simple compounds, known sometimes as *minerals;* 2d, compounds of a nature higher, more complicated, and differently characterized, known as *vegetables;* 3d, compounds still higher and more complicated, and again differently characterized, known as *animals.* As the elements do not remain isolated, so also their compounds continually change their mutual relations; and the result of these changes is that continual falling down and building up which may be seen in the material world. To move these elements and their compounds there is a fund of *force,* constant in quantity and in quality; if ever it seems to be less in quantity, some of it is latent; if ever it seems different in quality, it is but changed in appearance, from being connected with some peculiar compound. Here is Cosmos at a glance!—there is the force, the mover, a; and these are the elements, the things moved, b, c, d, e, &c.—a may be a' (mechanical force), or a'' (chemical force), or a''' (vegetable force), &c., but still it remains a. a may act on b, c, d, e, and there may result such compounds as be, ceb, dec, &c. When a joins b to c, a part of a becomes *latent,* and the result may be called $be+a$; but, when this compound is decomposed by a *different form* of a (e. g. a'''' or light) then a latent is *set free,* and immediately takes b and joins it to d, e, making the *higher compound bde,* while c is set free as an element. To give an instance, if b is carbon, c hydrogen, and d oxygen, and a''' is vegetable force, then b, c, and d, joined by the action of a''', would be the compound bcd, and might be a turnip. This theory looks simple, but its very roundness is suspicious.

The human mind, craving something more than mere fact, has tried to get at the *reason.* The fact is the law, the reason is the cause. It is in the search for the latter that scientific men have fallen on that unfortunate word, that shadow of a shadow, that last resort of ignorance—*Force!* It is safe to say that no

* The instructive and ingenious essay, by Prof. Joseph LeConte, is quoted in no invidious spirit, but simply as a fair sample of a philosophy now very common. Prof. LeConte may contend that he uses the word "force" only as a convenient supposition, on which to build a theory; if this be the case, it should be remembered that a form of expression which is sure to mislead ninety-nine readers in every hundred, should not be used in scientific writing.

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word in the English language has created so much ambiguity, so much meaningless discussion, and so much wrong-headed philosophy, as this ill begotten monosyllable. It is a gag for inquisitive people. What keeps the world from flying off in space? The force of attraction. What keeps it from rushing towards the sun? The centrifugal force. What makes oxyd of lead join acetic acid? The chemical force. What makes oxyd of lead *leave* acetic acid and go to sulphuric acid? The chemical force again. What makes a fish with a ventral fin? The vital force. What makes a fish *without* a ventral fin? The vital force again. Mark this! These forces are blind and they are the same always, yet they make different things, on the one hand, while on the other, they repeat the same thing over and over again. How is this? Can a round auger bore a square hole? Or, can it bore any hole at all without *intellect* to guide it? If several forces are not enough to account for the phenomena of the world, what can we expect of *one* force? Yet to this Prof. LeConte would reduce us. His "correlation of forces" is one force; and his "conservation of forces" is the amount of this force. The *amount of force*, acting on the *sum* of the *material elements*, produces all motion and all being. The force is spoken of as "latent," "set free," "liberated," and "developed," and as being "furnished" and "supplied," by decomposition.

Force is either something or nothing, if it be nothing, it is not worthy a place in science; if it be something it may be considered like any other fact of philosophy. What, then, is force and whence comes our idea of it? To answer these questions it is necessary to define, 1st, true causation; 2d, variable sequences; 3d, invariable sequences. The operations of my own will are the only instances of true causation of which I am conscious. The will, itself immaterial, works either on the material body, or on the immaterial mind. When the body is free and healthy the will has direct control over the voluntary muscles, but none over the involuntary. On the other hand, when the mind is free and healthy, the will has direct control over the whole of it. Whenever I will to do an act, mental or physical, I am *conscious* of using power. *Causation, then, is the act of x, using power to produce y.*

We may, in the outer world, see one thing happen immediately after another; yet, in a little while, we may see the first thing again, but this time *not* followed by the second. In such a case we say that the fact, that one thing followed another, was an accident. For instance, twelve mines of powder may explode, one after another, but probably this will not happen so again. *A variable sequence, then, is the fact that y has followed x, but will not, of necessity, do so again.*

We may, in the outer world, see a particular thing, which, as far as our experience goes, is always followed by another par-

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ticular thing; for example, the twelve mines above mentioned are fired by electricity, and, as far as we know, a certain amount of electric heat in contact with gunpowder is followed by an explosion. *An invariable sequence, then, is the fact, that y always follows x.* To sum up; a man, by conscious exercise of will, discharged the battery (*efficient causation*); the electric spark was followed by an explosion of gunpowder (*invariable sequence*); certain other mines happened to explode, after this one, in quick succession (*variable sequence*).

The admission of universal, invariable sequence (i. e., every x is invariably followed by its peculiar y) is the ground-principle of the theory of necessity, which leads directly to pantheistic atheism, with its idealistic and materialistic branches. The sole object of science thus becomes, to find out all the xs in the universe and all the ys, and to give to each x its proper y. All these, when tabulated so as to show their fixed relations to each other, make up the course of nature, and human knowledge has then done its utmost and has nothing left to work on. This course of nature, if admitted as entirely true, may still be looked at in two ways: 1st, it may be said that this course is such as it is by reason of an *essence* which exists in matter; or, 2d, it may be stated, that the course is such as it is, and that no reason therefor can be given, or ought to be given. Such views as these might be allowable if all our knowledge were got from observations on the *outer* world, and without reference to our own consciousness of the structure of our minds; for we could not prove the existence of *power*, in the phenomena of the outer world; and, if we suspected it, we might still set it down as an inherent property of matter. But the moment *consciousness of self* is brought into the question, a new element is introduced, namely, *intelligence*. Consciousness is fundamental, and cannot be denied; consciousness of thought, that is intelligence, cannot be denied; consciousness of *power* has been denied, and with what truth? I am fully conscious, that I turn my mind to a given thought, and I sharply distinguish this act of *causation* from any *sequence*; here then is the immaterial will exercising *power* over the immaterial mind. If there is no such thing as power, whence comes the word, and whence the general idea of it? Again, I will to move some part of my body, and do move it, and I compare this power with that used in mental action, and I find them the same. There is this difference between the cases: in the first instance I move what is strictly myself, the unit, the immaterial; in the second instance I move what is only a frame, over which I have limited control; the compound, the material. And observe this; there is a difference between merely *wishing* and the act of *exerting power consciously*. I may wish that my legs would, all at once, walk off with my body;

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but, if this should take place, I should be conscious that I was not exerting the power. Again, if, at my mere wish, a great rock should come to me, I should be conscious that something else moved it, and not myself. And, if my power of will was so extended, that I could thereby move rocks, I should be just as conscious of that power as I now am of the power to move my own body. A wish, followed by the thing wished for, is a sequence; a command, followed by the thing commanded, is power or causation. It is therefore wrong to say, that such sequences, if observed many times, would give me the idea that *I* exercised the power. When ignorant people attribute true causation to material bodies, (e. g., in the case of electricity,) they do so because they have the consciousness of power in themselves, and they attribute the same property to a body, which has in itself no power, but which shows signs of being acted upon by a power. If now it be said that matter has a certain essence, property, or what not, which makes it to do certain things at certain times, what is this essence or property but *power?* From our very idea of the word, anything that produces motion in another thing, either has or uses power. Still, though there may be power, is it *intelligent?* or do there exist a number of *un*intelligent powers, which so limit the scope of each other's action as to make a balance in the course of nature? (e. g., the power of centrifugal force balanced by the power of gravitation; the power of oxygen, produced by plants, balanced by the carbon, produced by animals; the power of insects, balanced by the birds that feed on them; the power of fruits, balanced by the parasitic fungi that grow on them.) To such a query, science is able to make this answer: "The phenomena observed in the outer world (*non-ego*) correspond to the phenomena of self (*ego*), which are produced by *intellect.*"* This is a sound induction; if it be not good, then no induction is good; and, if induction cannot stand, science must fall. To sum up, if it be admitted that there is an essence, inherent in matter, which necessitates its actions, then, 1st, this essence is *power;* and 2d, this power is *intelligent;* for, to deny that the essence was power would be to stultify our understanding, and, to deny this power to be intelligent, would be to ignore induction and to destroy science. It may be well also to take notice of the fact, that most theories, savoring of materialism, speak of some ultimate essence (force) which is at the bottom of all motion and action, as if such essence were fundamental and satisfactory; but this, after all, does not help the theory, for, if this essence is true matter, it moves, first *itself*, and then other matter, and, if it is a property, then this property moves first *itself* and then matter.

* For a full consideration of intellect, as shown in nature, see Essay on Classification, by Louis Agassiz.

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There still remains the second way of looking at the course of nature: it is such as it is, and no reason therefor can be given. What is the exact meaning of this statement? Simply that the upholder of the doctrine resolves, so far as concerns the gaining of truth, to put trust in the testimony of his eyes, ears, touch, and other physical senses, and in nothing else. He is a strict positivist; he says: "I see x, feel y, and hear z; to me, then, x, y, and z exist as things seen, felt and heard, respectively. To me there is no causation, for I do not directly perceive it. I believe in memory, it tells me what I have formerly seen; I believe in the axiom, *cogito ergo sum*, because the very denial of thought implies the action of thought. My duty is to clearly understand and to tabulate all the observations I have made; but there it stops; I may make many inductions and may use them for convenience, but am not sure of them. One induction tells me the sun will rise to-morrow: for my convenience I say it will; but of this I am not sure; perhaps it will *not:*—it is a thing of the future and the future I cannot see. I attempt to explain nothing:—*you* attempt to explain, and you come to no certain results:—I am more modest. By my individual observation I know certain things, and all other things are to me only as what may be. The earth may, or may not, turn again on its axis; causation may, or may not, exist; there may, or may not, be a God."

The holder of this theory, when compared with the believers in kindred opinions, is, in a certain sense, consistent; because he only makes statements, which are true as far as they go; and does not attempt explanations, which must in the end prove unsatisfactory. But his theory is incomplete and therefore one-sided and untrue. On the one hand, he acknowledges consciousness, and through consciousness believes in all the phenomena of the *outer* world (*non-ego*) as isolated facts; while, on the other, he ignores the properties of *self* (*ego*) which are also given by consciousness, and are as reliable as any of its data. Thus, he acknowledges the motions of his voluntary muscles, of which he is conscious, but refuses to acknowledge his own power to move them, or to leave them at rest, of which he is also conscious. We need go no further than the idea of *intelligent power*, to find the whole trouble in this, and in many other theories.*

What interest does a true conception of the ever-working Creative Intellect give to science! This correspondence of the human with the Divine mind! The astronomer works out, with pencil and paper, the possible answers to a certain problem of

* For able presentations of the doctrines of free-will and of necessity, consult the writings of Prof. Francis Bowen, and of J. S. Mill. The idea of causation has been carried to its last analysis by Sir William Hamilton.

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motion; he looks at the heavens, and there sees these answers, illustrated in the orbits of celestial bodies. The zoölogist, marking the changes of the embryo, thinks of these changes as so many different animals; deep in the rocks he finds all stages of this embryo, each represented by a species, perfect in its kind!

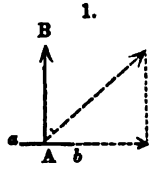
On the other hand, how dead the science, that puts "force" as its first cause! What is this force that makes the star-fish and the oyster, the medusa and the cuttle-fish, the crab and the whale, the tufted sea worm and the shark, each in its kind, and each telling its own story of manifold relations with animal creation, that is, that has been, and that is to come? Nature is no such simple thing that she should be dictated to by light, or heat, or electricity. These are her servants, not her masters!

Boston, Nov., 1859.

ART. XVII.—*On the causes of deviation in Elongated Projectiles;* by Maj. J. G. BARNARD, Corps of Engineers, U. S. A.

THE various and somewhat conflicting explanations given of the deviation of projectiles, both spherical and elongated, arising from their own rotary motions, leave room for a few additional words on this subject.

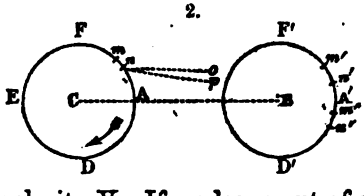
1.



If the plane surface $a\,b$ moves, in an elastic medium in the direction of its normal, with a velocity A B, that medium will oppose a force to which we apply the term "resistance" and which is measured by a function of the velocity A B.

If, at the same time, the surface has a velocity A C, *in its own plane*, the result will be an actual velocity of *each point of the surface*, represented by the diagonal A D; but the velocity of impact of the surface with the air, is the same in both cases, being due only to the normal velocity A B. The motion A C in its own plane, would displace, in no degree, the atmospheric particles, (except through the agency of that action known as friction—not now considered,) and would therefore generate no component of "resistance."

2.



If the sphere, whose great circle is A D E F, move through the air in the direction C B, with a velocity V, a resistance will be opposed to its motion which will, in magnitude, be a function of the diameter, and of the velocity V. If we leave out of consideration the force of *friction*, the character and intensity of the impact of the sphere with the

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air will be identically the same, whether it possesses or not rotary motion: for in either case, the surface, considered *as a whole*, advances in identically the same manner—the displacement of atmospheric particles is the same, and the resulting resistance, the same.

Let the rotation be supposed about a horizontal axis, perpendicular to the line of flight, and in the direction A D. The velocity of the *individual points*, *m*, *n*—or if you choose—*elementary surface*, *m n*, will be the resultant of the rotary and translative velocities, and the little surface *m n*, instead of moving (at the instant) in the direction *n o*, will move in an oblique direction *n p*. But the *rotary* component of velocity lies in the plane of this elementary surface, and has, (as in the case of the *lateral* velocity A C of the plane, Fig. 1) no agency whatever in displacing the air, or in affecting the intensity or character of its impact.

These considerations will, perhaps, be rendered more clear by reflecting that the resistance of a fluid, is due and *due only* to the *displacement of its particles*—that when the centre of the sphere has advanced from C to B, the anterior surface has advanced from F A D to F′ A′ D′, and displaced the air in identically the same manner, whether the sphere revolves or not.

These considerations are so obvious that it seems superfluous to insist on them; yet few of the writers on this subject have exhibited a clear understanding of them; or rather it may be said that they exhibit the reverse.

Thiroux, rejecting friction entirely, or rather considering its effects inappreciable, bases his reasoning on the higher velocity with which the points of the surface on the side A F *impinge on the air*, over that belonging to points on the side A D; an idea, as just shown, entirely fallacious. Capt. Neumann (Prussian artillery), in a theory as pretentious as it is unmeaning (Delobel's *Révue de Technologie Militaire*, vol. i.), carries this absurdity to the extreme of considering each elementary surface *m n*, *separately*, with its combined motion of translation and rotation, and, applying to each the ordinary expression for resistance of a plane surface impinging obliquely upon an elastic medium, *integrates* through each half of the anterior surface, to obtain the total action on each side.

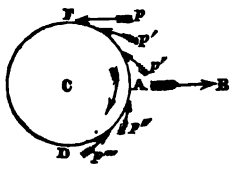
Not only are the conventional expressions for the resistance of isolated oblique plane surfaces found most inaccurate in practice, but they lose *all* applicability when they cease to be *isolated*, and form part of another larger surface (not plane); and this problem, of which the knot is so expertly cut by Capt. Neumann, who proceeds to apply his results to the criticism or *test* of Magnus' and other theories, is the very "pièce de résistance" which has defied the analysis of d'Alembert, Poisson and Poncelet—per-

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haps I might add of Newton and Laplace; one of those problems of mechanics to which the term *difficult* would be misapplied, for analysis has never yet been able to grasp it at all.

I have said that without the consideration of *friction*, the action upon the air of a rotating and non-rotating ball are identically the same. But *friction* materially alters the character of this action. Whatever may be the immediate cause of this force —whether simply a collision of the inequalities of the surface with the particles of the fluid—or whether it is due to *adhesion*, the effect is that the moving surface puts in motion *with it*, the adjacent fluid particles, and in so doing, developes forces tangential and opposed to its own motion.

3.

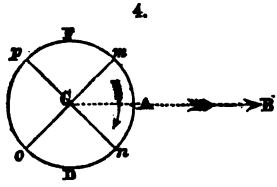


Thus the anterior surface of the sphere C, revolving from F to D, and advancing from A to B, creates, at each point, forces, p, p', p'' &c., tangential and opposed to its rotary motion, the resultant of which is a force acting from D towards F and tending to deflect the flight of the ball in that direction. This is the point of view, *and the sole one*, in which Poisson has considered the effects of friction.

But there is *another* effect which proves to be very powerful. Force cannot be applied to an elastic fluid, neither can motion be imparted or destroyed, without effecting, at the same time, its density and *pressure*. To retard a flowing current is to increase its pressure; to accelerate it is to diminish the same.

Applying this to the ball, the air, displaced and compressed in front, escapes along the surfaces A F and A D. Near its surface, the action of friction is to retard the escaping currents on the side A F, and to accelerate them on the side A D, and in consequence, an increase of pressure ensues on the side A F, and a diminution on the side A D; and therefore, a resulting pressure tending to deflect the ball from F towards D. If we divide the great circle A D F into four quadrants by the lines $m o$ and $n p$, drawn at angles of 45° with the direction of translation A B, we may better analyze the effects of friction, in the two forms in which I have presented them.

4.



The posterior quadrant $o p$ is in air so highly rarified that its action is insensible or nearly so.* On the side quadrant $m p$ the resultant of the forces of friction (the forces p, p', &c., of Fig. 3) are parallel (or nearly so) and *opposed* to the motion of translation. They have no effect (or but trifling) to *deflect* the ball from its course, but acting upon the air, in direct opposition to

* The *high velocities* of translation of military projectiles is supposed.

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the escaping currents, their whole force is expended in destroying velocity and generating *pressure*. On the anterior quadrant *m n* the resultant of the forces *p*, *p'*, &c., is from *n* towards *m*, and tends, almost entirely, to deflect the ball in that direction.

On the quadrant *n o* the resultant of the forces *p*, *p'*, is parallel to the motion of translation, and co-incident in direction with the escaping current whose motion it accelerates and whose *pressure* it diminishes. Thus, taking the four quadrants, in *one*, *o p*, the forces of friction are absent; in *two*, *m p* and *n o*, they are expended in producing an *inequality of pressure* on the two sides of the ball, tending to deflect the ball towards the side D (right); while in the anterior quadrant *m n*, they act to deflect the ball in the opposite direction F (left).

It would have been difficult to decide *a priori*, which of these forces would prevail, though, while the force of friction is nugatory in *one* quadrant, in two (*m p* and *o n*) it expends itself in developing forces tending to deflect to the right, and in only *one*, *m n*, does its direct action tend to deflect to the left; yet it must be remarked that in this quadrant the air is most dense, friction the greatest, and that it acts *directly* upon the projectile.

In the two lateral quadrants the air is less dense, and it is only through pressures *developed in the air* that it produces its effect; a loss of effect ensuing in the medium through which it acts.

Experience has shown, however, that the forces developed in the two lateral quadrants prevail, and the projectile is deflected to the right; and the experiments of Dr. Magnus give the same result when, instead of a projectile moving through the air, a *current* of air is directed upon a revolving cylinder.*

The deviation of elongated projectiles, having rotary motion about their axis of figure, though many authors, Thiroux, Panôt, Tamissier, &c., have attempted to refer it to the same causes which produce the deviation in spherical balls, is evidently governed by other causes.

Not only do such writers have to make, as to the direction which the axis maintains, assumptions which conflict with each

* Of course the division into quadrants which I have made is arbitrary, and only used as a simple means of illustrating how the conflicting effects are produced from one and the same *cause*. I comprehend under the term *friction*, all the forces by which a solid *surface* acts upon a fluid flowing along it, whether by adhesion or simple mechanical collision of particles; and this is the usual meaning of the word in this connection. Poisson has considered friction only in its direct action, i. e., as the resultant of the forces *p*, *p'*, &c., of Fig. 3, and, deducing therefrom a deviation to the left, he arrives at the conclusion that the magnitude of the force was not sufficient to account for the *amount* of observed deviation (irrespective, I presume, of *direction*). If this is true, it is difficult to conceive that the force (overlooked by him) arising from developed *pressures* (and which I show to have its origin in friction), is sufficient to annihilate the *direct* effects of friction itself, and to produce the deviations beside. Nevertheless it is all we can account for, and the experiments of Magnus seem to indicate its adequacy.

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other,* but the causes they assign are inadequate, and, moreover, as the deviation of an elongated ball depends more on the direction in which its axis happens to be deflected than upon the direction of the disturbing forces, no such uniformity in the deviations as is observed, would be produced, were friction on the inequalities of pressure the governing causes.†

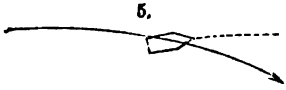
There is another cause which *necessarily* operates in this case; a cause which long before I knew of the experiments of Dr. Magnus, I conceived to be the predominating one.

The gyroscope shows, to the apprehension of every one, that a rotating solid of revolution supported by a point in its axis, about which it is free to move, solicited by a force tending to turn it in any direction, turns, *not* in the direction of the force which solicits it, but, with uniform and slow motion, normally to it; that the axis of figure will describe a *cone* about a line passing through the point of support, parallel to the direction of the soliciting force.

The elongated projectile is discharged from the piece with its axis coinciding with its trajectory, but, through the action of gravity, the trajectory deflects from its original direction and from that of the axis.‡ In consequence of this, the resistance of the air acts obliquely to the axis, and, with the ordinary forms of elongated projectiles, without grooves, its resultant passes in front and *above* the centre of inertia, tending to *raise* the point,

* Thiroux's theory requires that in the descending branch of the trajectory, the point of the ball shall be depressed *below* the trajectory; Panot's, that it shall continue parallel to its original direction, and hence elevated *above* the trajectory. Dr. Magnus supposes the axis to keep pretty nearly coincident with the trajectory, and says that experiments made with balls fired at low velocities confirm this assumption.

† To illustrate my meaning, take the friction theory of Panôt which supposes the axis elevated above the trajectory and that the friction on the under side (where the air is *densest*) carries it to the *right;* yet were this ball a hollow one (as nearly all are) the tail of the ball would be so much lighter than the point that the forces of friction (equal all along the cylindrical surface) would affect the tail more than the point and thus deflect the axis to the *left*, and produce a deviation in that direction.



‡ *Which preserves its original direction through the stability it derives from its axial rotation*, is the unmeaning and false expression (or something equivalent) commonly used in this connection. Properly *there is no such thing* as stability of the axis. Take away the possibility of this *conical* motion (in precession) just described, *confine the axis to one plane*, and it becomes as movable as if no rotation existed. This is clearly established in the Analysis of the Gyroscope (this Journal, [2], xxiv, 49), where the following language is used, "the popular idea that a rotating body offers any direct resistance to a change of plane, is unfounded. It requires as little exertion of force (in the direction of motion) to move it from one plane to another, as if no rotation existed." The "surprising phenomena" of "complete mobility" when one of the rings of the gyroscope is so held that the axis is confined to one plane of motion, which Dr. Magnus arrives at *experimentally* (and all the modifications of his experiments) flow directly from my analysis. *If, then*, stability of axial direction is attributed to axial rotation in projectiles, that which is the *inseparable accompaniment* of this *apparent* stability (i. e. the conical motion described in the text), and without which there is nothing even *resembling* stability—must be accepted with it, and the *deviation* of the projectile, which will *certainly result therefrom*.

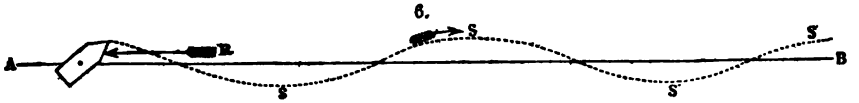
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and from this results the conical motion of the axis to the *right*, if the rotation is to the right—to the *left* in the contrary case.*

Before attempting to apply the foregoing to the real case, it will help us to consider a more simple one.

Suppose the rotating projectile with its axis inclined to its trajectory to be propelled through the air, and that the centre of inertia is *confined* to a rectilinear path (as if it slid along an extended wire, A B, for example), and that the force of gravity does not act.

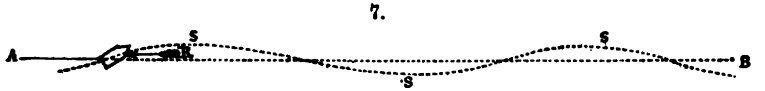
6.



The resistance of the air, R, acting *immediately* to increase the angle of inclination, produces *instead* the resulting conical motion to the right, which, combined with the motion of translation, would cause the *point* to describe a helix *s s s* about the rectilinear path of the centre of inertia.

Let now all the circumstances be as above *except* the confinement of the centre of inertia.

7.

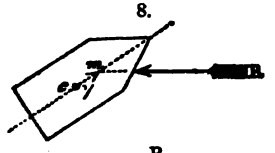


The resistance R, acting obliquely to the axis of figure, will (as in the familiar case of the sailing of a vessel *on* the wind) give the projectile a component of motion in the direction of its axis, and the centre of inertia cannot remain on its rectilineal trajectory A B. But, owing to the conical motion of that axis about a line parallel to the resistance R, (a direction, itself, always changing) the result will be that the projectile *itself* will describe a helix, *s s s*, about the line of original direction A B.†

* These results have been exhibited *experimentally* by Prof. Magnus; they flow directly from my analysis made *without* experiment of any kind.

† The *period* of these conical, or rather helical revolutions, could be computed if we knew the exact intensity of the force R; the distance C*m*, (or γ) from the centre of inertia, at which it cuts the axis of figure; the value k, of its radius of gyration about its axis of figure; and the velocity, n, of rotation.

8.



The period, T, (as deduced from my treatise on the Gyroscope) would be expressed in seconds thus—

$$T = 2\pi \frac{k^2 n}{\gamma r}$$

In which $r = \frac{R}{M}$ = *total* resistance of the air ÷ mass of the projectile. The value of r for the ordinary musket ball would be about 100. Assuming it at 80 for the elongated ball with more mass and *less* resistance—and assuming γ but $\frac{1}{10}$ inch, putting $n = 2\pi \times 150$, and $k^2 = \cdot 15$ inch, we should get $T = \frac{3}{4}''$ or three-fourths of a second.

If we suppose there to be an actual centre of *resistance*, *m*, (a supposition not true probably, but like that of the meta-centre, in hydraulics, admissible in narrow limits) this period would be *the same*, whatever be the inclination of the projectile. The period would increase as the resistance of the air diminished by loss of initial velocity.

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In the foregoing case there is no *permanent* departure, or *deviation* from the original direction of projection; nor would there be much *apparent* departure in the range of the modern rifled arms since the periods of these helical revolutions would probably be but a fraction of a second, while the time of flight is from 6 to 8 seconds.

The matter is much more complicated when, instead of the imaginary case above presented, the force of gravity is introduced and the actual flight of a projectile is considered. The ball is projected, having its axis coincident with the trajectory, but from the very outset of the flight a severance commences in the directions of the axis and trajectory, owing to the action of gravity. The resistance of the air becomes gradually more and more oblique to the axis, pressing (as in Fig. 7) the point upwards, and producing a *precession* (or conical motion) to the right.

The entire character of the motion becomes altered by the constantly shifting direction in which this resistance is acting, owing to the constant *fall* produced by gravity.

It is very difficult, *a priori*, to describe the exact character of the motion. It depends upon unknown facts, and doubtless varies much with the form of the projectile—its velocity of translation and rotation.

It would be going into reasoning probably too difficult to express intelligibly, to attempt to explain why, in all cases, the period of these helical revolutions would be very greatly prolonged, and why I think that in *most* cases (if not all) *no complete* revolution takes place at all, but that the whole motion is confined to the first quadrant. In the latter case the flight of the projectile would exhibit one continuous and constantly increasing deviation to the right. In the case of a considerable number of helical revolutions actually performed, there would still be, with each revolution, an increased deviation to the right, owing to the descending branch of the vibration being so much larger than the ascending* one.

It would, perhaps, be unprofitable—in the absence of any sufficient data—in the absence even of experiments made with any knowledge of the *real* causes of deviation, and therefore nearly useless as reference for data, to pursue this subject any further.

That the *precessory* motion is an inseparable attendant to that (so called) *stability* of axis which is the *very object* of giving rotation, I have shown; that it is an *adequate* cause of deviation I think will be admitted. Hence, while *other* causes *may* contribute to the effect, or may oppose it (according to the shape of the ball), this peculiar effect must be looked upon as the controlling cause of deviation of elongated balls discharged from rifled arms.

* In this case the helical path would probably be so *prononcé* as to be easily detected by firing through screens.

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I will but add a few very general conclusions I have arrived at from the foregoing.

First, the elongated ball possesses, from *its very shape*, a tendency to pursue the *direction* of *its axis*. This tendency should be made available, so far as practicable, to sustaining the flight, and flattening the trajectory.

Second. In the modern improvements in the art of throwing projectiles from rifled fire-arms, a decided step has been made *backwards*, in losing that most essential element to range and accuracy, *initial velocity*. It is desirable, and, I should think, not impracticable to restore it.

The initial velocity of the old rifle ball (weighing but about 180 grains) was 1750 feet per second. To the modern projectile, weighing about 500 grains, is given but from 900 to 1000 feet initial velocity.

It would doubtless be difficult to *give* the high velocity to so heavy a ball, and if given, the recoil would be inadmissible. But why throw so heavy a ball?—and why adhere to such calibres? The long range of such balls is not due to their weight, but to their *model* and to the *low ratio of their cross section to that weight*.

Both these advantages can be attained with a light ball as well as with a heavy one.

An increase of weight over that of the old musket ball is, *in itself*, objectionable, by increasing the soldier's load.

I can see no reason why, to a ball of the weight of the old musket ball (340 grains), may not be given all the properties of the heavier ones, with the additional great advantage of a *high* initial velocity, approximating to that of the old rifle balls.

To accomplish this, of course, the calibre must be greatly reduced—the ball elongated, and a *comparatively* large charge used (less, however, than that used in the old musket—viz. 110 grains).

This highly elongated ball, *balanced* (if I may use the term) by grooves around the after end, so arranged that the resultant of the air's resistance shall pass as nearly as practicable through the centre of inertia,* will fulfill likewise the *first* condition I

* According to my theory of the matter, these grooves, instead of acting like an arrow's feathers, to keep the axis coincident with the tangent to the trajectory, *have just the contrary object*, to keep it (if properly adjusted) parallel to that original direction, by so balancing the forces of resistance as to cause their resultant to pass through the centre of inertia. The theory of "arrows' feathers" supposes, on the contrary, that, by the grooves, this resultant is thrown *behind* that centre, and tends to *tilt* the axis; an operation which must inevitably, *likewise*, produce *deviation*. Prof. Magnus' observation on balls with low initial velocities (so as to be visible to the eye) confirm my position, though I attach little value to those observations.

Experience seems to show that this balancing of the forces of resistance may be produced *without* grooves (vide Wilcox, p. 160), for the Enfield ball and the Whitworth projectile seem to be without them, as likewise Lancaster's. I have seen no statements, however, as to the *deviation* of these projectiles.

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have laid down. For the highly elongated ball possesses in a proportional degree the tendency to pursue the direction in which its axis points: and the causes of deflection of the axis' direction being eliminated by a proper balancing about the centre of inertia of the forces of resistance, the tendency is to pursue the *original* line of direction, in opposition to the downward curvature due to gravity, and thus to *flatten* the trajectory and increase the range.

These conclusions are those of theory alone, but, if I mistake not, all the most recent advances in rifled arms have been in the direction which they indicate. The Swiss (Federal) rifle, one of the most perfect in Europe (see Lieut. Wilcox's "Rifles and Rifle Practice," p. 187) has a calibre of but 0·41 (inch), and its ball, 2½ calibres in length, weighs but 257 grains, thus combining with the small calibre, the highly elongated form, and even then weighing *less* than our old spherical musket ball. Though its initial velocity is not given, yet as the greatest *proportional* charge of powder is used with it, doubtless it also receives the *highest initial velocity*,* of any of its class of projectiles now known in any service.

The *hexagonal projectile* of Whitworth is another instance in confirmation of the principles I advance.

Of small calibre and highly elongated,† he throws this projectile with such accuracy as to hit with certainty, at 500 yards, a disk *not more than two inches in diameter;* and "asserts that he will not rest satisfied till he has fired a ball from one of his guns into the barrel of another, at a distance of 500 yards." (Edinburgh Review, Ap. 1859.)

The "Armstrong" projectile is another characteristic instance. So decidedly is the *elongation of the ball* characteristic of the most recent and successful efforts in obtaining range and accuracy, that the English writer just quoted applies the term "bolts" to the Whitworth and Armstrong projectiles.

In fact, the *two* springs from which have risen the modern improvements in projectile weapons are 1st, the application of the *rifled principle* to *all arms;* 2d, the *elongation* of the projectile. *Either one alone* may produce, *to a certain extent*, the results desired; it is only by the best possible combination of the two that the best results can be educed. The *increase of range* is due

* Initial velocity is the *very first* element in procuring range and accuracy. The greater it is, other things being equal, the *flatter* will be the trajectory (one of the principal elements of accuracy), and the flatter the trajectory the less the lateral deviation laterally. The extreme curvature of trajectory (though it may have its advantages in some peculiar circumstances), is the great difficulty as to accuracy *of fire*, in modern rifled weapons.

A *necessary evil* with the heavy balls used; the pretence that it is, *in itself*, an advantage, is too absurd for controversy.

† According to Wilcox the mean calibre is about 0·46, and its length 2½ calibres.

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almost entirely to the latter principle, and it is only by applying it to the utmost *practicable* extent (as in the case of the Whitworth and Armstrong projectiles), that the greatest range and most perfect accuracy can be obtained.

The *calibres* in use have been a positive *bar* to the successful use of this principle in small arms. Borrowed from the old smooth-bored weapons, the adherence to them has caused an unnecessary *increase* of weight,* and made a loss of *initial velocity* inevitable, with all the attendant evils of a highly curved trajectory, and large deviation.

"Notwithstanding" (says Wilcox) "the long time that has elapsed since the discovery of the rifle, its principle is not yet so well understood as to have led to the general adoption of any particular form of this arm as the best."

The above conclusion of the author of "Rifles and Rifle Practice," will justify me, I hope, in venturing to make the foregoing suggestions on the subject.

New York, Dec. 15, 1859.

ART. XVIII.—*Gulf Stream Explorations—Third Memoir. Distribution of Temperature in the Water of the Florida Channel and Straits;* by A. D. BACHE, Sup't. U. S. Coast Survey.—With Diagrams.

(Communicated by authority of the Treasury Department to the American Association for the Advancement of Science.)

The results of the explorations of the Gulf Stream in the survey of the Coast, have been communicated to the Association from time to time, as phenomena of peculiar interest have been developed.

The original plan of these explorations having been carefully studied, and having proved successful, has steadily been adhered to. The more recent observations have been directed to that part of the stream between Havana and Cape Florida, known as the channel and strait of Florida.

I have now to present four sections showing the depth and temperature in this most important region of the Gulf Stream. These results are from the observations of Commander B. F. Sands and Lieut. Commanding T. A. Craven, U. S. Navy, Assistants in the Coast Survey, whose names have already been mentioned before the Association in connection with explora-

* The old spherical musket ball of ·69 calibre weighed 340 grains. The new "rifle musket" ball of ·58 calibre weighs 500 grains, while still heavier balls are in use in Europe. Thus we see in use balls surpassing in weight the musket ball, and having *twice*, and even *three times* the weight of the Swiss ball described in the text, admitted to be, practically, the best in Europe.

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tions of the Gulf Stream, and furnish a sufficient guaranty that the results have all the reliability which care, experience and zealous labor could give them.

Section No. 1, from Cape Florida to Bemini was run by Lieut. Commanding Craven, in May, 1855; Section No. 4, by Commander Sands, in May, 1858; and Sections 2 and 3 by Lieut. Commanding Craven, in April and May of the present year, (1859). Sections 2, 3 and 4 are perpendicular to the direction of the Stream at distances of about fifty, one hundred, and two hundred miles from Cape Florida. The Florida strait is funnel-shaped, being about ninety miles wide at Havana and about forty-five miles wide at Cape Florida, the narrowest part.

Form of bottom.—The area of the water way and the form of the bottom are represented on diagrams 7, 8, 9 and 10. The Arabic numerals at the top represent distances from the Florida coast (the Keys) in miles, and the Roman numerals, the positions at which observations are made. The numbers at the left hand represent the depth in fathoms.

Commencing at the Cape Florida section, it will be seen that there is a rapid descent of the bottom to the Havana section, from three hundred and fifty fathoms to eight hundred fathoms, or twenty-seven hundred feet in a distance of two hundred miles. The most shallow as well as the narrowest part of the Stream is therefore at Cape Florida. The deepest water follows the coast of Cuba and the Grand Banks, the depth being eight hundred fathoms at a distance of only five miles from Havana, nearly four hundred fathoms within five miles of Salt Key Bank, and three hundred fathoms close to the island of Bemini. The descent from the Florida side is for the most part gradual, but from the opposite side abrupt. This effect seems to have been produced by the action of the sub-current in wearing a deeper channel upon the concave side of the Stream. At Havana there is an abrupt descent of nearly a mile within five miles of the shore, while on the side of the Tortugas and Key West the water is comparatively shallow, and the descent gradual. This fact goes to confirm the conclusion that the stronger current of the Gulf Stream makes the circuit of the Gulf of Mexico, since, if it impinged directly upon the island of Key west and the Tortugas we should find its effects in the wearing of a deeper channel on that side.

TEMPERATURES.

Change of temperature with depth.—In a former communication the law of change of temperature with depth was discussed, and types of the curves representing the law were given for different parts of the Stream. The curves were all merely modifications of a more general form. Thus, the cold water between the Gulf Stream and the Coast gave one form; the axis of the Stream

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another; and the water beyond the axis a third form, while in the strait of Florida a fourth was developed. It would be natural to expect in the course of many years' explorations by different individuals with different instruments not even of the same class, that general phenomena of this character should present some contradictions and some inexplicable results. Experience however has confirmed the first conclusions and the constancy of the phenomena. It is not difficult, having the curve representing the temperatures at any position from the surface to the depth of several hundred fathoms to determine from the temperatures alone, in what part of the Stream they were taken.

Temperature in a direction perpendicular to the Stream.—Diagrams 2, 3, 4 and 5, show the changes of temperature for the same depth in each of the sections, and diagrams 7, 8, 9, and 10, the depth for the same temperature.

Bands of warm and cool water.—In the section from Cape Florida to Bemini, the division of the Stream into bands is plainly exhibited, though more faintly than in the northern sections, and the form of the bottom in this section shows the elevations and depressions corresponding to the divisions. In the sections south of Cape Florida, all traces of the bands seem to disappear as well as the ridges of the bottom. The bands therefore seem to have their origin near Cape Florida, and the conclusion stated some years ago, as the probable one, is strengthened, that they are caused by the ridges and valleys of the bottom parallel to the general course of the Stream, and along which the Stream and counter stream have their course.

The Cold Wall.—The cold wall, as an exception to the remark made above in reference to the bands, is traced as far as the Tortugas, and is plainly shown in all the sections with more or less distinctness. In the Sombrero Key section (No. 3) it is strongly marked at depths ranging from seventy to a hundred fathoms, while in all the sections the warm water at the surface overflows the cold wall and reaches quite to the shore.

Diagram No. 6 represents the comparative curves of the cold wall in different sections of the Gulf Stream, including those in the Straits of Florida. The figures at the top show the distances of the cold wall from the shore in the different sections, and the numbers on the left the degrees of temperature. The curves are drawn for different depths in the several sections, as shown in the notes at the bottom of the diagram. The curves *g*, *h*, *i*, *k*, represent the cold wall in the four sections under consideration.

Longitudinal Sections.—It has been found very difficult to deduce any satisfactory law for the decrease of surface temperature along the axis of the Stream owing to the variability of the temperature of the water of the regions from whence the Gulf Stream is supplied. Two modes of investigating the subject

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have been pursued, one by following the stream from the Gulf of Mexico, and making hourly observations of the temperature of the water, and the other by comparing the mean temperatures of the various sections with each other, and with the temperature of the Gulf of Mexico. In the first method, the vessel must be allowed to drift with the current of the stream, a difficult condition except in the best weather, even for a day, and to float along thus, for hundreds of miles would rarely be practicable. Any motion communicated by sails or by steam must carry the vessel beyond the water in which she commenced her voyage, and the lateral overflow carries the water constantly from the axis toward the edges of the Stream.

In the comparison of mean temperatures of the different sections, the fact has been established, that the temperature of the water of the Stream at any point may be higher than at a point nearer the source, and hence vessels in running along the Stream may, and generally do, pass through water not of a constantly diminishing temperature, but from cool to warm, and the reverse. This is to be explained mainly though not entirely, by the variability of temperature at the source.

By taking the mean temperature of any one section, and going back to the date of the departure of the waters from the Gulf of Mexico, as determined by the velocity of the stream, and comparing the temperatures observed with the temperature of the Gulf waters, it was supposed that a solution of the question might be obtained. The temperatures were taken from the most authentic meteorological records of the Gulf for a series of years, and those periods sought which corresponded to the dates desired. The uncertainty of the temperatures of the waters of the Gulf of Mexico, as obtained from air temperatures taken here and there along its shores rendered the results unsatisfactory. Enough seems to have been determined, however, to show that the surface temperature of the Gulf Stream along its course is variable; that a vessel sailing along the axis at a more rapid rate than the motion of the stream, will pass through water of higher and lower temperature, depending generally upon two conditions, viz: the distance from the Gulf of Mexico, and the temperature of the Gulf at the time the water entered the straits of Florida; and further, that the latter cause is the predominating one in the parts of the Gulf Stream adjacent to the Atlantic coast where the current is rapid.

The influence of the form of the bottom in forcing the cold counter current of the bottom upward, has been adverted to, and the fact appears to be well established in the cross sections where the ridges and valleys parallel to the direction of the stream separate it into bands of warmer and cooler water, and this conclusion, as has just been stated, is strengthened by the fact that the

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bands and ridges simultaneously disappear south of Cape Florida. The phenomenon is moreover strikingly exhibited in the longitudinal section of the bottom, in connection with the lower temperatures.

The shallowness of the Stream in the strait of Florida, connected with the fact that the bottom falls off rapidly to the north and south afforded an excellent opportunity for testing the question. If the cold water of the under polar current follows the bottom, it should appear in the shallow part of the strait, and here the warm water of the surface, and the cold water of the bottom, would approach each other. Diagram No. 1 shows the curves of 40°, 45°, and 50° (bottom temperatures) along the deepest part of the stream, commencing at Sandy Hook, and running as far as the Tortugas. All these curves rise with the bottom and pass over the ridge which divides the bed of the Atlantic from that of the Gulf of Mexico, and again fall with the slope of the bottom towards the Gulf. In the narrowest part of the strait where the depth is three hundred and fifty fathoms, the temperature from the surface to the bottom, ranges between 80° and 40°.

On the effects of pressure on Saxton's deep sea thermometer.—In the exploration of the Gulf Stream, the temperatures below one hundred fathoms have mostly been determined by Saxton's metallic thermometer, and although the results have been consistent amongst themselves, and have agreed well with the indications of other thermometers, yet it was thought advisable to determine the effect of pressure by direct experiment.

Saxton's thermometer consists essentially of a compound ribbon of silver and platinum fused and pressed together by rollers. This ribbon is wound in a spiral form, one end of the spiral being firmly fastened to an interior solid axis and the other left free. Upon the free end is placed an index arm which moves over a circular graduated scale carrying with it a friction hand or indicator which is left at the extreme point of the arc reached by the true index. The instrument is enclosed in a case to which the water is freely admitted. A variation of temperature is immediately noticed, as the effect is to give a rotary motion to the index.

The experiments to determine the effect of pressure were made at my request by Mr. J. M. Batchelder with means devised by Mr. Thomas Davison at the Novelty Iron Works. The following description of the apparatus employed, is given by the last named gentleman.

"The gauge consists of a brass cylinder H, about eight inches long into which a steel plunger is fitted, the upper part of the plunger at A being ·70 of an inch in diameter, and the lower at B about ·786, so that the difference in area of the ends is equal

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to one tenth of a square inch. The cylinder is bored out a little larger than the plunger except for about a fourth of an inch near each end at C and D where both are accurately fitted. To the branch E a pipe connects, communicating with the hydraulic cylinder and leading the water into the centre of the gauge which it reaches after passing through the chamber F filled with sponge to prevent any impurities in the water from reaching the plunger. The upper end of the plunger connects by a wire W, to a spring as shown in the sketch at G, so constructed as to indicate pressure from 0 to 450 lbs., the spring being so strong that 450 lbs. produce a movement of the plunger equal to three-eighths of an inch. It is evident that as the difference in area of the ends of the plunger is one-tenth of an inch, one hundred pounds pressure from the water on this surface, as indicated by the balance, would equal a pressure of water of 1000 lbs. per inch, or a pressure ten times as great as that indicated by the balance throughout its scale. The only difficulty in the use of the gauge is that of fitting the plunger to the cylinder so that while it is perfectly free to move it is also perfectly water tight. This difficulty however has been overcome, and much advantage was also derived from Mr. Batchelder's suggestion for supplying the wear of the plunger and cylinder by depositing brass on the plunger through the galvanic process."

Connected with this gauge by a pipe is a strong wrought iron cylinder, sixteen inches long by four inches in diameter, in which the thermometer was placed, the opening being firmly closed by a screw plug. This second cylinder was immersed in a tub of water for the purpose of regulating the temperature. The thermometer once placed in the cylinder, is not again removed, the index being read by means of a mirror until the observations are completed. By the use of this apparatus, the effect of pressure up to 4000 lbs. per square inch was observed upon two thermometers, and the results are given below. The observations were made to indicate the effects of 500, 1000, 1500, 2000, 2500 lbs. pressure, etc. Seven series of experiments were made with thermometer No. 5, and five series with No. 10. The mean results show that a pressure of 1000 lbs. per square inch has no effect upon the thermometer; at 1500 lbs. the effect is less than one degree; and from 1500 to 4000 lbs. per square inch, the effect is to diminish the readings, the maximum effect being seven degrees.

The diagram exhibits the law of diminution by increase of pressure, and the depth corresponding to different pressures. The correction to be applied varies with the depth. For thermometer No. 5 it is only four tenths of a degree Fahrenheit at the depth of 600 fathoms. For thermometer No. 10, it is one degree at the same depth.

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At 1500 fathoms the corrections are respectively five and a half and seven degrees.

Nearly all the temperatures observed in the Gulf Stream have been taken at depths less than six hundred fathoms.

Table showing differences of readings of Saxton's Thermometer under pressure and free from pressure.

Thermometer No. 5.							Thermometer No. 10.						
No. of Series.	Pressure in Pounds.						No. of Series.	Pressure in Pounds.					
	1500.	2000.	2500.	3000.	3500.	4000.		1500.	2000.	2500.	3000.	3500.	4000.
	°	°	°	°	°	°		°	°	°	°	°	°
1	1·6	0·	3·75	0·	0·	0·	1	0·	2·0	3·25	4·5	6·25	8·25
2	0·	1·00	2·	2·8	4·5	5·5	2	2·0	1·0	3·5	4·5	6·0	7·25
3	0·	1·0	2·25	3·75	4·75	5·7	3	0·75	2·0	3·0	3·25	5·5	6·5
4	0·	0·5	0·50	2·0	3·6	5·5	4	1·75	2·0	3·5	4·75	5·5	7·25
5	0·	1·75	2·25	3·5	5·0	6·5	5	·75	1·75	1·75	3·75	5·0	6·75
6	0·	1·25	2·25	3·75	6·0	6·5	Means	1·00	1·75	3·0	4·25	5·6	7·25
7	0·5				3·5	4·25							
Means	0·3	1·1	2·1	3·2	4·5	5·6							

Art. XIX.—*On the Chemical Composition of Pectolite;* by J. D. Whitney.

A few years since I made some examination of specimens of a radiated fibrous mineral from Isle Royale, Lake Superior, which proved on analysis to be pectolite. A mineral, closely resembling pectolite, from Bergen Hill, New Jersey, which had been analyzed by L. C. Beck, and considered by him as identical with the stellite of Thomson, was examined at the same time and found to agree in composition with pectolite, as had been previously suggested by J. D. Dana. Both the stellite and Wollastonite of Thomson were referred by me, at that time, to pectolite,* a reference the correctness of which has since been shown by Messrs. Heddle and Greg, in a paper on the composition of the English varieties of this mineral.†

Notwithstanding so many analyses of pectolite have been made by different chemists, there has not been a sufficient accordance in the results obtained to justify a positive decision as to the real formula of the mineral, although that of Von Kobell has been generally adopted. It will be sufficient to refer to the various published analyses, to see that there is but an unsatisfactory degree of uniformity in their results, whether of specimens from American or European localities. Thus, for instance, in

* Journal of Boston Nat. History Soc., vi, 40.

† Philos. Mag., [4], ix, 238; also in Erdmann and Marchand's Journal, lxvi, 144.

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Von Kobell's analysis of the Monte Baldo pectolite, the silica is given at 51·3 per cent, while other analyses of Scotch and American varieties give as much, in some instances as 54 and 55 per cent of that substance. In the like manner, the amount of lime, as stated by different analysts, varies from 29·8 to 35·2 per cent, while there is even less agreement in the water, which is given at from 0·41 to 3·39 per cent.

The difficulty of procuring, in a perfectly pure state, a mineral which only occurs in a finely-fibrous condition is undoubtedly one of the principal causes of these discrepancies in the analyses; but it is also possible that the unusual care required for the correct determination of the silica in the very soluble class of minerals to which pectolite belongs may not, in all cases, have been appreciated. The great abundance and purity of the specimens of this mineral which have been obtained from the tunnel of the Erie railroad, recently excavated through Bergen Hill, seemed likely to obviate the first difficulty mentioned above. The results of three analyses indicated that this material was really of almost absolute purity, while no pains were spared to effect a complete and accurate separation of the various ingredients, and especially of the silica.

The pectolite dissolves more or less completely in chlorohydric acid, according to the strength and quantity of the latter. By using a considerable excess of rather dilute acid, all, or nearly all, the pulverized material may be dissolved into a clear liquid. As the attack is usually performed, a portion of the silica remains in solution and the remainder separates as a flocky precipitate.

The following experiments show the difficulty of estimating the silica correctly in this class of highly soluble silicates, and the necessity of unusual precautions in its determination.

On digesting the *ignited* mineral with acid until a perfect attack seemed to have taken place, the solution gelatinized on evaporation, and there was no perceptible gritty feeling when it was stirred with a glass rod or the spatula; on separating the silica, however, after evaporating to dryness, moistening with acid, and adding water, in the usual way, its amount was found to be equal to 62·10 per cent of the substance taken.

Another portion of the *unignited* mineral was attacked by acid, and the silica separated without evaporating to entire dryness; its amount equalled only 35·6 per cent. To procure the whole amount of this substance present in the mineral, and uncontaminated by any traces of the bases, it was found necessary to use the unignited substance for the attack with acid, carefully to evaporate to entire dryness over the water-bath, then to moisten the dried mass with strong acid and allow it to stand for some time before adding water and filtering. These precau-

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tions will give a perfectly pure silica, but not all of it, as two or three per cent will still remain in the solution, a part of which will go down with the ammonia precipitate, and the remainder be found after driving off the ammoniacal salts, (the lime having been previously separated,) and igniting the residuum.

The following are the results of three analyses of as many different specimens of the pectolite from the Bergen Hill tunnel.

	I.	II.	III.
Silica,	54·82	54·76	54·27
Lime,	33·12	32·88	32·83
Protoxyd of manganese,	·66	1·16	1·24
Protoxyd of iron,	·26		
Soda,	8·78	9·17	8·94
Water, by loss,	2·36	2·03	2·72
	100·00	100·00	100·00

The direct determination of the water on the substance dried at 80° C. gave, for II, 3·03, and for III, 2·75 per cent. Specimen III, from the Wheatley Collection, Union College, was apparently the purest; it was a fragment of a mass, the fibres of which were several inches in length, slightly divergent from a common centre, and being nearly transparent and evidently quite free from any admixture with quartz or any other foreign substance.

In analysis III the oxygen is as follows:

Silica,	28·942
Lime,	9·379
Soda,	2·306
Protoxyd of manganese,	·290
Water,	2·44

This gives as the ratio:*

	Ḣ	:	Ṅa	:	Ċa	:	S̈i
	1	:	1·05	:	3·83	:	11·84
or nearly	1	:	1	:	4	:	12

If we attempt to express this ratio by a formula, we have

$$\dot{N}a^2\ddot{S}i^4 + 4\dot{C}a^2\ddot{S}i^3 + 3\dot{H}.$$

The percentage demanded by this formula is given below, by the side of that required by Von Kobell's.

	Whitney.	Von Kobell.
Silica,	54·22	52·05
Lime,	33·73	35·33
Soda,	9·33	9·77
Water,	2·74	2·83

It is evident, from a comparison of the figures given above, that the formula now suggested agrees more nearly with the re-

* The atomic weights used are as follows:

Na=23 : Ca=20 : Si=21, H being =1.

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sults of the analyses of the Bergen Hill pectolite than any yet proposed. As written here, its relations to that of spodumene are to be noticed, as also to those of Wollastonite and pyroxene. The latter connection will be made plainer by writing the formula of pectolite thus:

$$\dot{R}^3 \ddot{S}i^3,$$

$\dot{R}$ being $(Ca\frac{4}{5} + \dot{N}a\frac{1}{5} + \dot{H}\frac{1}{5})$.

Northampton, Mass., Feb. 1859.

ART. XX.—*Notes on the Ancient Vegetation of North America;* by Dr. J. S. NEWBERRY. In a letter to Prof. DANA, dated Santa Fe, New Mexico, Oct. 15th, 1859.

Dear Sir:—I have just returned to Santa Fe after an absence of three months, spent in an examination of the geological structure of the country bordering the San Juan and Upper Colorado rivers, in Utah and New Mexico, connected with the War Department topographical survey under Capt. Macomb, Topog. Engrs.

The region visited proved interesting in many respects—beautifully picturesque and unexpectedly productive—covered with ruins, and once densely populated by a race that has now entirely abandoned it.

I would cheerfully give you a sketch of its remarkable physical and geological structure, but the results of the expedition will doubtless be published in detail by the War Department, and it is not proper that any part of them should now be given to the public. I will say however, in general, that our field of exploration includes an immense labyrinth of great cañons, scarcely less abysmal than those of the Lower Colorado in which we were last year involved—some of which are over a mile in depth—and even more varied and wonderful in character. The sections exposed in their walls permitted me to measure and examine all the strata between the base of the Carboniferous and the summit of the Cretaceous series; the latter formation attaining a thickness of 4000 feet, and occupying an immense area west of the main divide of the Rocky Mountains.

Our work this season connected on the south with that of the party with which I was associated last year under Lieut. Ives, T. E.—and combining the results of both expeditions, I have now a complete and detailed section of all the rocks composing the great central plateau of the continent, from the base of the palæozoic series to the summit of the Cretaceous. These strata are conformable throughout, and over 10,000 feet in thickness.

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The collection of fossils made, both animal and vegetable, is quite large, and, with considerable new matter, includes what is much better, many well known species of which the geographical range will be seen to be much greater than has been heretofore supposed.

I was agreeably surprised to find here on my arrival, the May, July and Sept. numbers of the Journal, published during my absence, by which I have been in a degree placed *au courant des affaires scientifiques*. In the articles in the July number from the pen of my friend Mr. Lesquereux, I have been much interested, as they refer to matters which have engaged much of my attention for some years. In the letter of Prof. Heer there is however a passage which seems to me to require notice; although, situated as I am, without specimens or books for reference, I am scarcely prepared to take up the discussion of the questions involved in it. This is the less necessary now, as what I have to say in reference to them will be found *in extenso* in the reports on the geology of the country west of the Mississippi which I have made or have in preparation for the general government.

The paragraph to which I allude is as follows:—

"Your views of the gradation of the flora of North America agree perfectly with what we find in Europe. This led me to believe that the plants of Nebraska belong to the tertiary and not to the cretaceous formation. It is true that I have seen only some drawings which were sent to me by Messrs. Hayden and Meek; but they are all tertiary types. The supposed *Credneria* is very like *Populus Leuce*, Ung., of the lower Miocene, and the *Ettinghausiana* seems hardly rightly determined. Besides it is a genus badly founded, and which has as yet no value. All the other plants mentioned by Dr. Newberry belong to genera that are represented in the Tertiary and not in the Cretaceous. And it is very improbable that in America the cretaceous flora has had the characteristic plants of the tertiary; and this would be the case if these plants did belong to the Cretaceous."

It will be seen that Prof. Heer in this paragraph makes several distinct statements, which for the sake of brevity, I will notice in the order in which they occur. They are,

1st. That the fossil plant I supposed to be a *Credneria* is very like *Populus leuce*, Ung.

2d. The *Ettingshausenia* (called erroneously *Ettinghausiana*) is wrongly determined.

3d. That the genus *Ettingshausenia* is badly founded and has no value.

4th. That all the other plants enumerated by me are represented in the Tertiary and not in the Cretaceous.

5th. That it is improbable that in America the Cretaceous flora had the characteristic plants of the Tertiary, as would be

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the case if the plants of which outline sketches were sent to Prof. Heer by Mr. Meek, were Cretaceous.

To which I reply,

1st. The plant I considered a *Credneria* is not *Populus luce* Unger, which, according to his descriptions in the *Foss. Flor. v. Sotzka* and *Genera et Species Plant. Foss.* has a toothed margin, while the leaf in question is entire.

I have recently obtained more and better specimens of this fossil, of which I have apparently three species, but as yet have not had opportunity to study them carefully.

They strongly resemble some of the species of that portion of the old genus *Credneria* to which Stiehler left the name (*C. integerrima*, &c.) when he established his genus *Ettingshausenia*. It may prove a new genus. Further study will alone determine this.

2d. That I was wrong in considering some of Dr. Hayden's fossils generically identical with those Stiehler designated by the name *Ettingshausenia*, I am by no means prepared to admit.

Prof. Heer has had but a single outline sketch of the plant, and can hardly speak decisively on the subject. When at Washington I had before me all the figures and descriptions of Stiehler, Zenker, Dunker, Bronn, and Unger, of the genus *Credneria*, and a large number of specimens in good preservation, for comparison. To me, and to Mr. Meek, who examined the subject with me, there seemed to be a marked correspondence in general form, texture and nervation, between our specimens and some lobate *Crednerias* (*Ettingshausenias* Stieh.). With these I regarded our fossils as generically identical, and shall continue so to regard them until the question—if question there be—can be definitely settled by a comparison of specimen with specimen—old world and new.

3d. Whether Stiehler was in error in establishing the genus *Ettingshausenia* upon a group of species of *Credneria*, I will not pretend to say, for I have nothing like the ample material possessed by him when he made the division; indeed this question has nothing to do with that now before us.

It is true that at the Smithsonian Institution I had access to nearly everything that has been published upon the genus *Credneria*, and it seemed to me the most natural thing in the world that Stiehler should give generic value to the cuneate or lobate form, and strongly marked yet finely reticulated nervation, which characterize the group of species of *Credneria* of which *C. cuneifolia* may be taken as a type, while he left the broadly rounded, entire, or merely toothed leaves, with a sparse and rectangular nervation, such as *C. integerrima*—leaves not very unlike those of *Coccoloba*—to which they have been compared—to retain the name of *Credneria*.

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The vindication of his accuracy may, doubtless, be safely left to Stiehler. At least it would be nothing short of arrogance for any one who had not before him a suite of the specimens compared by Stiehler, to review his work and pronounce it either erroneous or correct.

4th. The statement that aside from the so-called *Credneria* and *Ettingshausenia*, *all* the genera enumerated in my letter to Messrs. Meek and Hayden, are represented in the Tertiary and not in the Cretaceous, is at least surprising. I am almost inclined to infer from it that Prof. Heer, though confessedly the highest authority in reference to the Tertiary flora of Europe, has neglected to acquaint himself fully with that of the Cretaceous formation. He makes the statement doubtless in good faith, but he can hardly have seen Stiehler's paper on the Cretaceous plants of Blankenburg, and if he has not seen that he is certainly not yet prepared to discuss intelligently the claims of *Ettingshausenia* to be recognized as a good genus; nor indeed the Cretaceous flora in any of its aspects.

Whoever will take the trouble to examine Stiehler's paper (*Palæontographica*, 1857) will see in the enumeration of plants found in the Lower Cretaceous strata (Quader sandstein) *Populus*, *Salix*, *Acer*, and several other genera which Prof. Heer says are represented in the Tertiary but not in the Cretaceous.

The fossil flora of Blankenburg is indeed strikingly like that of our Lower Cretaceous formation, from which the plants that have given rise to this discussion have been derived, except that ours is more varied, and we have as yet found no palms or *Cycadaceæ*.

5th. In regard to the probability or otherwise that the Cretaceous rocks of America should contain a flora similar to that of the Tertiary, it may be said, that it is not now a question of probabilities but of fact, the evidence of the case being now before us, and in abundance.

In what has heretofore been written in reference to these fossil plants two great questions have been raised, 1st, as to their botanical affinities, 2d, as to their geological position.

As to their botanical relations—outline sketches of a few of the plants have been examined by Prof. Heer. By him they were decided to contain representatives of the genera *Liriodendron*, *Populus*, *Laurus*, *Sapotacites*, *Phyllites*, *Leguminosites*, &c., and were pronounced Lower Miocene.

The entire collection was placed in my hands for examination and description before I knew that Prof. Heer had been written to on the subject. I supposed I found among them *Liriodendron*, *Salix*, *Alnus*, *Populus*, *Platanus*, *Pyrus*, &c., with the Cretaceous genera *Credneria* and *Ettingshausenia* and considered them Cretaceous. That you may see on what evidence that opinion was

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based, I enclose a copy of my letter to Messrs. Meek and Hayden, which I chance to have with me.

Washington, D. C., Nov, 12th, 1858.

Messrs. MEEK and HAYDEN,

Gents: The fossil plants which you requested me to examine, I have looked over with great pleasure, and, in answer to your question as to the age of the strata from which they were derived, concur with you in the opinion that they belong to the Cretaceous epoch. They include, however, so many highly organized plants, that were there not among them genera exclusively Cretaceous, I should be disposed to refer them to a more recent era.

A single glance is sufficient to satisfy any one that they are not Triassic. Up to the present time no angiosperm dicotyledonous plants have been found in rocks older than the chalk, while of the eighteen species which compose your collection sixteen are of this character.

What was the general aspect of the flora of our Cretaceous continent we can only conjecture, as the specimens of it which we have, represent only its ruder and coarser elements,—the leaves of some of its deciduous trees, which, perhaps by an annual frost, were, as now in autumn, scattered on the surface of stream, lake, or sea, and, sinking, mingled with the sediment accumulating at the bottom.

In such an herbarium we could expect to find little else than the relics of some of the ligneous plants, and a very imperfect picture of the flora of the period.

The evidence furnished by your specimens is, however, good as far as it goes, and we are warranted in inferring from them the existence of a more highly organized flora during the Cretaceous period than has usually been attributed to it.

A flora so highly organized, embracing so many angiosperm dicotyledonous plants, should lead us to expect the discovery of what have not yet been found, plants of this rank in the Jurassic and Triassic rocks. Such a flora as is indicated by your specimens, could hardly have at once burst into being, but was doubtless preceded in the older formations by more or less highly organized plants, the prophetic types of those which followed them.

From the enumeration of the genera represented in your collections it will be seen that the flora of the Cretaceous epoch was not very unlike that of the temperate portions of our continent at the present time. The same thing may be said of the Miocene Tertiary flora of the Upper Missouri so fully illustrated in the collections of Dr. Hayden. In both the tropical and sub-tropical forms so common in the floras of the same period in Europe, are apparently wanting; indicating a greater relative uniformity of climate during the later geological epochs, and carrying the aspects of nature of the present, far back into the past. Thus it may be said of our plants as of our fishes, that many of them are "old-fashioned" types.

An interesting fact in this connection, to which I can only allude, is that the later extinct floras of Europe are more like the existing flora of North America, than is that now growing over the rocks which contain them.*

* Including as they do *Liquidambar, Liriodendron*, &c., now exclusively American.

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The species of your fossil plants are probably all new, though generally closely allied to the Cretaceous species of the Old World. From the limited study I have given them, I have referred them to the following genera:

Sphenopteris,	Cornus,	Salix,
Abietites,	Liriodendron,	Magnolia?
Acer,	Pyrus?	Credneria,
Fagus,	Alnus,	Ettingshausenia.
Populus,		

Of these the last two are exclusively Cretaceous and highly characteristic of that formation in Europe.

For comparison with the preceding list of genera, I subjoin a catalogue of the Cretaceous genera found at Blankenburg in the duchy of Brunswick, given by Stiehler in the *Palæontographica*, Sept. 1857.

	Credneria,	Pterophyllum,	Comptonites,
Algæ	Chondrites,	Flabellaria,	Populus,
	Halymenites,	Pinites,	Alnites,
	Delessertites,	Geinitzia,	Acer,
	Equisetum,	Araucarites,	Quercites,
	Pecopteris,	Salicites,	Juglandites.

I may say, in confirmation of the assertion that your fossil plants are Cretaceous, that I found near the base of the Yellow Sandstone series in New Mexico—called Jurassic by Marcou,—a very similar flora to that represented by your specimens, one species at least being identical with yours—associated with *Inoceramus*, *Gryphæa*, and *Ammonites*, of Lower Cretaceous species.

Yours, &c.,
J. S. Newberry.

Since that letter was written, I have added largely to my material illustrative of the American Cretaceous fauna and flora, having been for some months engaged in studying that formation over a large area, and where it exhibits an unequalled development.

Of the geological age of the deposits which contain the fossil leaves of which sketches were sent Prof. Heer, there cannot now be the slightest doubt. I have in my hands over sixty species of dicotyledonous plants obtained from the Cretaceous formation. At least half of these are derived from near the base of that system in New Jersey, Nebraska, Eastern, Middle and Western Kansas, New Mexico and Utah, collected by Prof. Cook, Mr. Meek, Dr. Hayden and myself. Some of the species are common to nearly all the exposures of the Lower Cretaceous sandstones, which I have examined, and everywhere serve for the accurate identification of these strata. Overlying the rocks containing all this flora, in the same continuous section, where the strata are conformable and undisturbed, both Dr. Hayden and myself have, in repeated instances, found many of the most characteristic fossils of the chalk, such as *Gryphæa Pitcheri*, *Inoceramus problem-*

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aticus, Ostrea congesta, Baculites ovatus, Ammonites placenta, Scaphites Conradi, Ptychodus Whipplei, &c.

The botanical character of this group of plants is, in all essential respects, just what I represented it to be in my letter to Meek and Hayden. Among them are certainly *Populus, Salix, Alnus, Platanus, Liriodendron, Fagus, Quercus*, &c., the most common genera in our present forests.

The plant regarded by Prof. Heer as identical with Unger's *Laurus primigenia* is not a *Laurus*, but a *Salix*, as Prof. Heer would have seen if the specimen had been sent him, instead of an outline sketch. As I have before said, his *Populus leuce?* is not that species. The plants which he calls *Sapotacites* and *Leguminosites* are of doubtful affinity, but certainly not referable to these genera. The latter has a nervation closely allied to that of some of the *Rhamnaceæ*. *Phyllites* is not, as Prof. Herr is made to say in Marcou's pamphlet on "American Geology," "peculiar to the Lower Miocene," but is a general receptacle for fossil leaves of all ages of which the botanical affinities are doubtful, just as *Carpolithes* is a general name for fossil fruits.

It is greatly to be regretted that Prof. Heer could not have applied his great knowledge to the specimens themselves rather than to outline sketches; or, at least, that he should not have been permitted to exercise his excellent judgment unbiased by erroneous oral testimony.

The remarks of Prof. Heer on the fossil plants from the Pacific coast described by Mr. Lesquereux, are exceedingly interesting as forming a new page in the botanical history of American geology, and yet the quite different flora which has come under my observation from the Miocene strata of another part of the continent proves that what he has predicated of the flora, and hence the climate of the continent, though doubtless true of the region where Dr. Evans' fossils were found, is not of universal application.

The study of the floras of the different geological formations has always seemed to me to promise much toward giving us a just idea of the physical geography of our continent, during the different geological epochs. Acting on this conviction in such parts of the continent as I have visited, the fossil plants found, and the nature of the sediments containing them—generally the direct debris of the ancient land—have been to me objects of special interest and attention.

The general results of these observations on the extinct floras of North America may be very briefly stated as follows:

1st. The flora of the Devonian and Carboniferous epochs in America was, in all its general aspects, similar to that of the Old World, which has been so fully described; most of the genera, and a larger number of species than at any subsequent period

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having been common to the two sides of the Atlantic. The relative number of identical species has, however, it seems to me, been somewhat overrated. In many of the species regarded as the same in Europe and America, the American plants present prevalent or constant characters which may serve to distinguish them. These differences, though frequently remarked by writers, have not been thought to have a specific value; yet it is quite certain that they are as tangible and important as those which now separate many American and European species of recent plants and recent or fossil animals. I have a conviction that the progress of science will considerably diminish the proportion of identical species; a closer scrutiny and more extensive comparison of specimens resulting in the discovery of constant, though inconspicuous characters which shall be ultimately conceded to be specific.

It is true also that in molluscous palæontology, recent geology and botany, the number of species common to the two continents has been considerably reduced of late years; a large number of American representatives of European species at first considered identical from their striking and obvious coincidences, having, on closer study, afforded constant, though less conspicuous differences.

2d. The Permian, Triassic and Jurassic rocks have hitherto furnished us but few species for comparison, but the material is increasing, and I have now on hand quite a collection which has not yet been studied. Enough is already known to show that the great revolution which took place in Europe at the close of the Permian epoch was matched by a parallel though less sudden change in the flora of America.

Here as there the Lepidodendroid trees, the *Sigillariæ*, the *Nœggerathiæ*, the *Asterophyllitæ*, and the great variety of ferns that gave character to the Carboniferous vegetation, were superseded by *Voltzia*, *Tæniopteris*, *Camptopteris* and a varied and beautiful Cycadaceous flora, in which were many species of *Zamites*, *Pterophyllum*, *Nilssonia*, etc., the representatives of those of the "Age of Gymnosperms," which culminated in the Jurassic epoch of Europe.

During this great interval the generic correspondence between the floras of Europe and America was perhaps as plainly marked as during the Carboniferous age, but the relative number of identical species was apparently smaller.

3d. At the commencement of the Cretaceous epoch the flora of the continent was again revolutionized and the vegetation of its temperate portions given the general aspect that it now presents.

This statement will surprise many, for the flora generally ascribed to the Chalk period is greatly different from that of the

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present. Unger has thus represented it, and Brongniart calls it a transition from the great Cycadaceous flora, of the Jurassic period, to the Angiospermous flora of the Tertiary. In Europe the Cretaceous flora was, apparently, more like that of the Lias and Oolite than in this country, for while the genera *Salix*, *Acer*, *Populus*, *Alnus*, *Quercus*, &c., were then introduced there as here, its general aspect was modified by the presence of numbers of *Cycadaceæ*, and its sub-tropical character attested by fan-palms.

We may find hereafter, in other parts of the continent than those in which I have examined the Cretaceous strata, fossils which shall assimilate our flora of that period more closely to that of Europe, but as far as at present, known, our plants of this age present an *ensemble* quite different. I have now some sixty to seventy species of Cretaceous plants collected in New Jersey, and in various parts of the great Cretaceous area of the interior of the continent, all of which indicate a flora very similar to that now occupying the same region; many, perhaps most, of the genera being now represented in our forests—such as *Liriodendron*, *Platanus*, *Acer*, *Populus*, *Salix*, *Alnus*, *Fagus*, &c. These specimens have been collected in localities included between the 36th and 41st parallels of latitude, but range from the 74th to the 110th of longitude. Nowhere within this area have I yet detected any traces of palms or any indications of a tropical climate. At the base of the Yellow sandstone series of New Mexico, (Lower Cretaceous,) I have found a varied and interesting flora, containing *Pterophyllum*, *Nilssonia*, *Camptopteris*, &c., with a few Angiosperm dicotyledonous leaves. This is evidently the point of junction between the Cycadaceous flora of the Jurassic age and that of the Chalk; for in the entire overlying Cretaceous strata, 4000 ft. in thickness, though Angiospermous leaves are abundant, those of Gymnospermous plants were nowhere discovered, nor any traces of palms, either leaves or stems. The sandstones of the Cretaceous series contain immense numbers of silicified trunks but they are for the most part coniferous.

4th. For the glimpses I have obtained of the Tertiary flora of North America I am mainly indebted to the kindness of Dr. Hayden, who has spent several years in most successfully exploring the geology, botany and zoology of the country bordering the Upper Missouri. Among his rich collections are fifty or more species of beautifully preserved fossil plants from the Miocene, which have been put in my hands for examination, and of which descriptions will be published immediately after my return to Washington.

Not having the specimens, or my notes on them, with me, I can speak only generally of the flora they represent. I remember, however, that they include species of *Platanus*,—one of which closely resembles Unger's great *P. Hercules* and is perhaps

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as large; *Populus*, *Acer*, *Castanea*, *Sapindus*, *Carpinus*, *Ulmus*, *Diospyros*, *Quercus*, *Salix*, *Taxodium*, and others which indicate a flora in all its general aspects similar to that now occupying the Valley of the Mississippi. A few plants in the collection would seem to have required a somewhat warmer climate than that which the localities where they are found enjoy at present; but there are no palms among them, nor any of the tropical genera *Cinamomum*, *Sterculia*, *Dombeyopsis*, &c., so common in the Tertiary strata of Europe.

In the enumeration of the Miocene plants of the Pacific coast, given by Mr. Lesquereux in the May number of this Journal,* I find also evidence of a marked and interesting difference of temperature during the Tertiary epoch, in different parts of the North American continent, under the same parallels of latitude. Mr. Lesquereux finds in Dr. Evans's collection Palms *Salisburia*, *Cinamomum*, &c., which indicate, at least a sub-tropical climate; a flora quite unlike that from the Miocene of the Upper Missouri, although, as he remarks, similar to that of the Miocene of Europe.

I am tempted to dwell for a moment on the interesting glimpses of the physical geography of our continent in geological times, which these facts and others that have come under my observation afford; for, to you, who have done so much toward the elucidation of its geological history, this cannot, I am sure, be a matter of indifference, but my letter has already grown to an unreasonable length. Let me then close with a few generalizations, referring you to my reports for all details of fact sustaining them.

1st. A large continental area occupied the place of the interior of North America from the earliest Palæozoic ages.

2d. During the Carboniferous epoch this land sustained a vegetation similar to that of the Coal period of Europe and Eastern America, though far less varied.

3. Through the Triassic and Jurassic ages the sediments from the land were strikingly like in mineral character to those of the same age in the Old World: and the flora was characterized by a preponderance of Cycadaceous plants, analogous to those of the Jurassic of Europe.

4th. In the Cretaceous age, the central nucleus of the continent was sufficiently extensive to furnish from its ruins arenaceous sediments that now cover more than half a million square miles. These sediments contain vast deposits of carbonaceous matter, mainly derived from the land plants which covered the continent. As far south as lat. 35° these plants were for the most part Coniferous or Angiospermous, and included many genera now characteristic of temperate climates.

Through the Tertiary epoch our continent had nearly the form and area it now has, the Tertiary deposits merely skirting its

* xvii, [2], 361.

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borders. The Marine Tertiaries are nearly limited to the shores of the present oceans, while the patches of strata of that age found nearer the centre of the continent are all, so far as I have observed or heard, of fresh water or estuary origin. Between the western base of the Sierra Nevada and the Mississippi there are, I believe no Tertiary beds not of this character, and the larger part of the great central plateau has never been covered with Tertiary or Drift sediments, but has, since the close of the Cretaceous epoch, been as now, dry land.

The facts which I have enumerated seem to indicate that over this ancient land the isothermal lines were curved much as now, and that during the Tertiary ages there was, perhaps, as great a difference between the climate of the Pacific and Atlantic water sheds as exists at present.

ART. XXI.—*Abstract of a Meteorological Journal, kept at Marietta, Ohio:* lat. 39°·25 N. and lon. 4°·28 W. of Washington City; by S. P. HILDRETH, M.D.—For 1859.—[Thirty-third annual report.]

1859. MONTHS.	THERMOMETER. Mean temperature.	Maximum.	Minimum.	Fair days.	Cloudy days.	Inches of rain and melted snow.	Prevailing Winds.	BAROMETER. Maximum.	Minimum.	Range.
	°									
January, . . .	33·00	63	4	17	14	3·10	W., S.W.	29·85	28·95	0·90
February, . . .	37·77	62	10	10	18	7·20	S.W., & E.	29·60	28·85	0·75
March, . . .	48·48	77	25	12	19	5·08	S.E., & S.W.	29·70	28·80	0·90
April, . . .	52·03	83	28	14	16	6·46	N., & N.W.	29·60	28·85	0·75
May, ...	67·20	90	46	21	10	1·56	S.W., & N.	29·60	29·10	0·50
June, ...	67·23	93	33	17	13	4·62	S., W., & E.	29·70	29·20	0·50
July, ...	74·23	102	49	23	8	1·08	E., S.E., & N.	29·73	29·20	0·53
August, . . .	72·10	95	46	15	16	4·46	S., & N.	29·53	29·30	0·23
September, . .	63·51	86	46	12	18	4·95	S., & W.	29·70	29·10	0·60
October, . . .	49·12	79	26	17	14	2·79	S.W., & N.W.	29·65	28·85	0·80
November, . . .	45·50	72	20	20	10	2·08	S.S.W., & E.	29·85	29·00	0·85
December, . .	30·48	71	5	12	19	5·17	N. & W.,	29·75	28·80	0·95
Mean for year,	53·38					Rain 48·55 inches.				

☞ *Remarks on the seasons.*—The mean temperature for the year 1859 is 53·38, which is somewhat above the average for this locality.

The amount of rain and melted snow is 48·55 inches. The average in a series of years, being forty-two inches, falling occasionally to thirty-two inches, and again rising, as in 1858, to near sixty-two inches, so that our climate is quite variable in this respect. The number of cloudy days bear testimony to the humidity of the year.

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Winter.—The mean of the winter months is 37°·14, which may be considered very mild for this latitude. The thermometer was at no time as low as zero, so that it did not make ice sufficiently thick for filling ice houses, only two or three inches being the extent of the best, and the main supply for summer use was brought from the heads of the Muskingum river. Steam boats were laid up only a few days, by the floating ice, during the winter. In the eastern states the cold was excessively severe. On the tenth of January, after a great snow storm on the eighth of that month, extending from the borders of Pennsylvania to Maine, the mercury sunk in Salem, Mass., to 23° below zero, in the city of New York to 11° below, and at Ogdensburgh, N. Y., to −38°. The extreme cold on the Atlantic coast was said to be greater than at any time during the last seventy years.

Spring.—The mean for the spring months is 55°·90, an unusual high range, being nearly four degrees above 1858, and more than ten above 1857, that being only 45°·89, so that there is a wide range in the temperature of our springs, which is most strikingly apparent in the blossoming of trees, especially that of the peach, there being a variation of not less than forty days in the opening of the fruit buds of this highly prized tree. The later the bloom is retarded the greater the chance for a crop, but so variable is the climate of southern Ohio, that only one season in three can be counted on for the production of this delicious fruit. The apple crop is rather more certain, and yet nearly every other year is a failure from the blighting effects of late spring frosts. But for this drawback it would be one of the most productive countries in the world in fruit, and the valley of the Ohio as celebrated pomologically as it now is for the growth of Indian corn. The unusual heat of the spring is chiefly attributable to the month of May, which was 67°·20, or six degrees above the average temperature, which is sixty-one degrees. The heat was nearly that of June, and rarely experienced, as there is commonly a difference of ten or twelve degrees in these two months. The peach was in blossom this year on the 28th of March, and the apple on the 12th of April. In 1857 the peach opened on the 2nd of May, and the apple on the 9th.

Summer.—The mean of the summer months is 71°·19, which is not much below the average, notwithstanding the uncommonly low temperature of June. The unprecedented occurrence of a severe and destructive frost as late as the fifth of June overwhelmed the country with fear and astonishment; at a period in the growth of wheat usually considered as past all danger of this kind, a sudden change of temperature in one night spread destruction and ruin to a large portion of the fields of this important cereal over all the central portions of the valley of the

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Ohio, and extending from Iowa to northern New York. Indian corn shared largely in this calamity. The warmth of May had hastened the growth of this plant in many fields to the height of twelve or eighteen inches. In all such cases the fields had to be replanted, but where it was only a few inches above the surface it recovered from the injury, and produced a fair crop. The wheat being in full head, and much of the grain in the milk, was entirely ruined. Potatoes were badly frosted, but in a good measure regained a healthy state. Peaches and apples, which in most orchards had attained the size of almonds, were so much damaged as to fall from the trees in a few days, and only certain favored localities ripened any fruit. So serious an injury from untimely frost has not been experienced since the first settlement of the state in 1788. In the year 1834, severe frosts visited Ohio as late as the middle of May, but the wheat crop was not so far advanced, being only in blossom, and by throwing up new stalks from the uninjured roots, produced finally an abundant harvest. Providentially the autumnal frosts of 1859 were retarded until near the close of October, and the late planted fields of corn were fully matured, to the great delight and wonder of the husbandman, for the failure of this grain would be a more serious calamity than that of wheat, as both man and beast largely depend on it for sustenance.

Autumn.—The mean for the autumnal months is 52°·71, which is rather below the average, but was sufficient to ripen all the late crops. Sweet potatoes were uncommonly good in quality, and abundant in quantity. Buckwheat was largely cultivated, partly in place of the common grain, and produced a great yield. The Catawba and other grapes ripened well, and abounded in saccharine principle, so necessary in making good wine. The smaller fruits were plentiful, so that on the whole we have more cause to be thankful, rather than to complain of the dealings of Providence in the past year.

Floral Calendar, &c.—February 25th, Bluebird heard; 27th, Yellow garden crocus in bloom; March 4th, Many birds of passage seen and heard; 9th, White crocus; 12th, Hepatica triloba; 14th, Golden bell or Forsythia viridis, Acer rubrum, (Red maple), Ulmus Americana; 16th, Hepatica acutifolia; 21st, Grass quite green in pastures; 22d, Magnolia conspicua, Claytonia Virg.; 26th, Red cherry, Balm of Gilead, and Sugar maple; 27th, Crown Imperial; 28th, Peach tree, Red Pyrus Japonica, Hyacinths; 29th, Sanguinaria Canadensis; 31st, Gooseberry.—April 1st, June berry; 2d, Dandelion, Pink colored Pyrus-Japonica, Cherry and Plum, Primroses; 4th, Flowering almond, Anemone nemorosa; 5th, Phlox divaricata, Dielytra cucul.; 7th, Annona triloba, Papaw; 11th, Burgundy pear, Trillium grandiflorum; 12th, Double flow.-Peach, Siberian crab, Spiræa prunifolia, Cercis Canadensis, or Red bud; 13th, Apple tree;

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14th, Chickasaw plum, Strawberry, Sedum ternatum; 17th, White ash tree; 22d, Cornus Florida; 24th, Dodecatheon Amer.; 27th, Harebell; 28th, Vernal snow drop; 30th, Tulips.—May 2d, Tree peony, var. papaw; 4th, Haw tree, Dicentra spectabilis; 5th, Mountain ash, Aquilegia Canad.; 6th, Magnolia tripetala, Viburnum, (Snow ball); 8th, European Horse chestnut; 10th, Rose colored peony, Yellow Harrison rose; 12th, Blackberry and Robinia Pseudacacia; 15th, Rose Acacia and Annual roses; 16th, Iris Persica, Crimson peony; 17th, Purple peony; 18th, Moss rose; 19th, White peony, also several new peonies from seed planted five years ago, bloom first time; 21st, Peas on table, planted in January; 22d, Syringa Philadelphica, Strawberry ripe; 24th, Bulbous Iris; 26th, Foliage of trees unusually rich and fine; 27th, Fragrant peony, and large rose colored; 28th, Linocera flexuosa; 30th, Erigeron annuum.—June 1, Star of Bethlehem; 2d, Common cherry, ripe; 5th, Severe frost, killing wheat, corn and fruit, made ice in a bowl of water, half an inch thick, a few miles west of Marietta; 12th, Canterbury bell in bloom; 19th, Red raspberry ripe; 23d, Pennsylvania lily in bloom; 27th, Wheat harvest begins in fields that escaped the frost, on high hills, or where protected by the fog from the rivers; 28th, June apple ripe.—July 5th, Chestnut tree in bloom; 8th, Sweet bough apple and Gravenstein ripe; 12th, Gladiolus flori; 13th, Blackberry ripe, but a large portion destroyed by frost, American broom in bloom.—August 8th, Catherine pear ripe; 13th, Watermelon ripe.—September 1, St. Michael pear; 15th, Seckel pear fully ripe.

In every month during the past year there has been more or less frost, as in the year 1816.

ART. XXII.—*Geographical Notices;* by DANIEL C. GILMAN. No. XI.

BIOGRAPHICAL SKETCH OF DR. KARL RITTER.—The death of Dr. Karl Ritter, the father of the modern science of Physical Geography, and one of the most eminent and beloved of the scientific men of Germany, has already been announced in this Journal. We present herewith a sketch of his life, translated and condensed from a highly interesting tribute to his memory which is attributed to the pen of Dr. Kramer of Halle, in the Berlin *Zeitschr. für allgemeine Erdkunde.*

CHARLES RITTER was born at Quedlinburg the 7th day of August, 1779. His father, a man of noble character, fine feelings and a pious mind, was physician to the Abbess of the Convent there, and was much esteemed for his skill. However, in consequence of the slanders of an envious individual, he lost the

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largest part of his practice, and, although his good name was restored after the lapse of two years, and his clients returned, grief and sorrow had so heavily weighed upon him during this time, that in the full strength of manhood, he succumbed to a typhous fever. He left an almost destitute widow with five little children, of whom the eldest, a boy, was ten years of age, the fourth, Charles, only five years old. This situation of the poor widow, a noble and highly educated woman, excited the utmost sympathy of her neighbors. All endeavored, either by words, or in a more substantial way, to make good the wrong which had been done to her husband. She found many sympathizers, away from home. Thus, the Prince of the adjoining Bernburg took care of the education of the eldest boy. Salzmann, the celebrated educator, a former associate of Basedow, had bought Schnepfenthal, and was about to open an educational institution there. He had made it a point to take a boy as his first pupil, gratis. A notice in a journal of the death of Dr. Ritter at Quedlinburg, who had left a widow with five little children, first attracted his attention. Soon after he sent two of his friends there, to make the acquaintance of the children and to see whether there was a boy amongst them, that would conform to his wishes. They decided in favor of little Charles. The mother, though with a sorry heart, assented, and, at the invitation of Salzmann, brought the child herself to Schnepfenthal. She was accompanied by one of her elder sons and Gutsmuths, then a candidate of theology and instructor of the children, who had not left them, although the mother had declared that she was no longer able to pay him his salary. A residence of a few days in Salzmann's house cemented the ties of mutual friendship and esteem, so that Salzmann, shortly before their departure, expressed a wish to keep the older boy also. To Gutsmuths he proposed to remain in Schnepfenthal as a teacher. This had been a secret wish of the mother, but she did not think it possible. Ritter accordingly came to Schnepfenthal, the first pupil of the new institution, and remained there for eleven years, until he went to the university. This lovely spot, which Ritter always considered his true home, was situated at one end of the 'Thueringer Wald' and was surrounded by a most charming landscape, having in one direction a view of a far extending fertile plain, richly adorned with cities and villages; in the other, there rose well timbered mountains of various shapes, intersected by fine valleys. All around was activity and life. Here he received from his early youth the most vivid impression of the glory of God's creation, of the variety of formations on the surface of the earth, and their special relations to the life upon them. Here Ritter grew up under the guidance of excellent men and skillful teachers. Those that ex-

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erted the greatest influence upon him, were Salzmann himself, Bechstein and Gutsmuths, the latter of whom continued here also, to take special care of young Charles, and probably implanted in him a love for geographical knowledge.

The method of instruction was that suggested by Basedow, and tried first in the Philanthropin at Dessau, but it was freed from those vain and needless peculiarities that adhered to it there. Classical languages were less studied, but the most attention was paid to all those sciences and accomplishments, which stand in direct relation to life, and among these the modern languages occupied a more prominent place, than anywhere else. To this an unusual impetus was given by scholars from different countries, who thronged, soon after the opening of the institution, to Schnepfenthal. By physical training, and by strengthening the character and intellect, a general and equal development of body and mind were especially aimed at, and, although practical rationalism pervaded the whole institution, darkening a little the deepest sources of true blessings, there still reigned piety, love, and the purity of high moral sense. Under these influences all those noble qualities of Ritter's heart and mind were developed, that distinguished him so much in after days. The future lay dark before him, and he had not decided upon his course in life, but he felt a strong desire to study, of which however there was as yet no prospect. His mother, though married again several years after the death of her first husband to the celebrated pedagogue Zerrenner, was not able to provide for him. But Providence interposed here also. A rich merchant from Frankfurt on the Main, associated with the large firm of Bethmann, Mr. Hollweg, visited the institution at Schnepfenthal and became very much interested in the young Ritter. After hearing of his circumstances, he declared himself willing, on the recommendation of Salzmann, to furnish the necessary means of study, upon the condition however, that Ritter, after the completion of his studies, should enter Hollweg's house as instructor of his children.

So Ritter went, at the age of 17 years, to the University of Halle, and was matriculated November 2, 1796, as *studiosus cameralium* under the prorectorate of Curt Sprengel. Here he remained for two years. Halle was then the centre of great scientific activity. F. A. Wolf especially was then in the height of his renown. Ritter did not pursue a specific course of studies, as his previous education had not been directed towards that channel, which however he sometimes regretted in later life. He often mentions A. H. Niemeyer, to whose circles he had access, and in whose house he lived, and who exercised upon him an important influence in improving his mind and inciting him to farther study. Niemeyer occupied then a prominent place

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in the pedagogic world, and that work by which he became most known, "Grundzuege der Erziehung und des Unterrichts" (or, Principles of education and instruction), first appeared in 1796, and must have been of special interest to Ritter, as he was himself preparing for the calling of an instructor.

In 1798 Ritter left Halle and entered Hollweg's house as instructor of his four children, especially of the two boys, one six, the other three years old. It was a great change for the young man of 19 years, to step out from the quiet circles in which he formerly had moved, into the midst of a world quite unknown to him, and to move among the aristocracy of a mercantile city. He had to struggle with many difficulties. But he went to his task in all earnestness, and with the ardor of a true and powerful mind, conquering all impediments so completely, that he gained results such as but few instructors can boast of. This was especially true in the case of his younger pupil, the other having died in the bloom of his youth. Ritter conducted the education of the former until he went to the University, and this pupil is the present minister of clerical, educational and medicinal affairs in Prussia, von Bethmann-Hollweg. Equally successful was he in the education of a son of the celebrated S. Th. Soemmering, and out of this relation of teacher and pupil grew the most intimate friendship and love, which lasted for life. During his stay in Hollweg's house Ritter came in contact, and even into nearer relations, with many eminent men, and by the intercourse with them his ideas expanded and became freer and more independent. Amongst those that exerted the greatest influence in this respect, S. Th. Soemmering must be mentioned above all, a man of great genius and deep scientific knowledge. Ritter thus speaks of him in the introduction to the second edition of his 'Erdkunde :' 'If in the explanation of the laws of the geographical relationship of all animated nature there should be prominent some interesting opinion and view, then the author is indebted for this whole tendency of investigation to the long, instructive, and I say it with pride, familiar intercourse with a noble man, S. Th. Soemmering, a man who is an honor to his century and nation; for his spirit filled others also with the premonition of the depth of nature, which his own genius had penetrated into its most hidden mysteries.' Ritter was also befriended at Frankfort by J. G. Ebel, the author of the classical work on Switzerland. This was not only of the highest importance for him during his repeated travels to Switzerland in relation to the knowledge of this country, but it also impelled him to farther study. Ritter, speaking of Ebel, says in the above-mentioned introduction: 'What the present work may have of vivacity and warmth, it owes to an intercourse of many years with this excellent man at the time I commenced it.'

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Ritter's mind moreover was much aroused by the daily intercourse with men of equal aim, and inspired with the same ardor for the education of youth, such as E. Mieg and J. B. Engelmann. Besides he sometimes came into transient but important intercourse with men of eminence who travelled through Frankfort. He met (to mention only a few names) Alexander von Humboldt and Leopold von Buch in Hollweg's house. But even life itself in this old interesting city, uniting so many instructive elements, showing so many different relations of a most various character, and being situated in the midst of the district of the largest river in Germany, always invited to new observations, wanderings and study.

Ritter used with the greatest ardor all these opportunities to acquire information. The time of his sojourn at Frankfort was a time of the most various studies. So he applied himself with much zeal to the classics, and read with the assistance of his friends, F. C. Matthiae and J. F. Grotefend, then at the head of the gymnasium at Frankfort, the most important works of the Greeks and Romans, but the tendency of his mind towards geography always appeared with marked prominence. In order to become entirely at home in this department, he not only studied thoroughly the most important works on the subject, but he also made observations of his own in frequent excursions to different parts of the country. The ability to draw with the greatest ease those objects in a landscape which were important to him, and so to fix them forever, was of much service to him. He always brought a number of characteristic sketches home from his journeys, which served both for himself and others as illustrations of his observations. This tendency towards geography was manifested in his first contributions to the 'Neuen Kinderfreund,' edited from 1803–1806 by Engelmann in connection with his pedagogic friends, but it became yet more apparent, when in 1806 he published his six charts of Europe, and not long after, in 1811, when his geography of Europe (2 vols.) appeared. In both works the peculiarity of his geographical perception is thus early indicated. They are the groping essays, the *incunabula* of what was lying in his mind. But, before his ideas could come to maturity and light, other preparations were to be made. As such, in different respects, must be considered the journeys which he, from the year 1807, repeatedly undertook with his pupils to Switzerland and Italy, and the last of which, commenced in 1811, comprised several years. These travels must indeed have been a rich source of instruction to his observing and powerful mind, that was so well prepared and matured by assiduous study and labor. Just these countries are the most expressive representations of the most important and most various geographical types which Europe has to show.

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Switzerland, the most important parts of which he crossed in different directions, impressed him deeply with the grandeur and glory of a majestic and infinitely rich nature, which invited irresistibly to the study of this gigantic structure. Italy, on the other hand, which he passed through down to its southern point towards Sicily, furnished him important information in reference to volcanic activity, exhibited to him the relation of land to sea, showed the effects of climatic differences and the close connection of the nature of the country with the development of its people. Those treasures of art, which Italy possesses so abundantly above all other countries, must have added their proper share also to his store of knowledge laid up in his naturally fine and carefully educated mind.

During his travels in Switzerland he met many men of eminence and note; amongst them Pestalozzi, von Tuerk, Niederer and many others of that district. He spent many happy and instructive hours in their society, and between him and many of them a mutual friendship was established. He always remembered Pestalozzi, whom he often visited at Iferten, with reverence and gratitude, and had a picture of him in full length in his study.

The most important point in Switzerland for him was Geneva, where he remained from the middle of the year 1811 for more than twelve months. This city was then particularly noted for the active part it took in the cultivation of science, especially natural science, and was much distinguished by the fine tone of its society. Saussure, the first man of the city and the state, had shortly before died, and his pupils, men of European fame, such as M. A. Pictet, de Candolle and others, were considered the centres of the higher circles. With the first, Ritter became very intimate and to him he owes many valuable hints. St. Gervais, close at the foot of Mont Blanc, was another very interesting point for Ritter, as it offered to him an opportunity of observing the nature of high mountains in all their details. From this spot he made that tour around the Mont Blanc, of which he gives such an interesting and instructive description, in explanation of the bas-relief executed by Kummer.

During his travels in Italy, Rome most especially attracted his attention. It was not only the centre of numerous monuments of history and art, but he also met here men like Thorwaldsen, Overbeck, Cornelius, and others, who by their genius and love for the arts, had raised them again to a most flourishing state. By intercourse with such men, Ritter's insight into the nature of Art was much enlarged.

Thus variously enriched, he returned home, and soon commenced that work, which was the chief production of his whole life, and which will make his memory immortal. In order to

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prepare it, he went in 1814 to Goettingen with his two pupils, who at that time were beginning their academical studies. Here he used all the means of learning within his reach, he searched out the rich treasures of the library, was in active intercourse with the masters of science, (Hausmann especially was dear to him,) and did not disdain to enter again as a student the auditoriums of the professors and to hear lectures on the most various subjects. After a residence of two years he went to Berlin (1816) and there gave his work its last finish, after which it was put to press. Next year he went again to Goettingen to superintend the publication of his work, of which the first part in 1817 with the title, 'Erdkunde im Verhaeltniss zur Natur und Geschichte des Menschen oder allgemeine vergleichende Geographie als sichere Grundlage des Studiums und des Unterrichts in physikalischen und historischen Wissenschaften,' or, Geography in relation to the nature and history of man, or general comparative geography as a safe foundation in studying and teaching physical and historical sciences. In this work geography had been entirely remodelled and changed. It had indeed been raised to the rank of a true science, constituting the link between the natural sciences and history. The first part contained Africa and part of Asia; a year after, the second part appeared, which completed Asia.

We abstain from giving a detailed description of this work, as it is well known, and as the method, in which geographical matter is treated here, has been adopted throughout the whole scientific world. Ritter's aim is briefly indicated by its title; a more detailed account of its leading ideas however is given by himself in his introduction to the second edition of the first volume, which appeared in 1822. Ritter's intention was, to give with the greatest accuracy a vivid image of the formation of the superficies of the earth in its horizontal and vertical dimensions by means of a conscientious and careful use of all existing sources, and to represent and explain the characteristic qualities of its parts and their relation to each other and to the whole earth, but at the same time to make it serve as a substratum of all animated nature, and as a foundation and condition of the development of the different nations and the whole human species in their manifold mutual relations to one another.

This was a stupendous task, but Ritter performed it marvellously well. Its execution required a combination of great and varied talents, such as rarely ever have been or will be found, cultivated by deep and assiduous study. In it we see powerful and truly ingenious displays of general geographical intuition and combination, we perceive a care, that indefatigably penetrates into the deepest recesses and most minute details, we find evinced an extensive knowledge of the natural sciences

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and a perfect command of extensive historical materials, and lastly, a truthfulness and thoroughness of learned inquiry combined with the rare gift of a rich, fresh, vivid and expressive representation. Truth and knowledge of the living God were the springs that actuated his mind and after which he aspired. Hence his humility, hence his close and perfect application, his concentration upon the subject before him. No difficulty ever deterred him in his investigations, although the matter before him was continually and vastly accumulating. His work was to him, as he wrote in his diary, when, after a long interruption, he again commenced his labor, 'his song of praise to the Lord.'

When Ritter had completed the first two volumes of his geography (Erdkunde), in connection with a work that was the direct result of his Asiatic studies, 'Vorhalle Europaeischer Voelkergeschichten vor Herodotus um den Kaukasus und an den Gestaden des Pontus,' or, 'Vestibule of the history of European nations before Herodotus around the Caucasus and at the shores of the Pontus,' he received in the year 1819 a call to be Professor of history at the Gymnasium of Frankfort. In the fall of the same year he married. In September 1820 he accepted a call as Professor extraordinarius of history in the Military School and the University of Berlin.

At Berlin the second half of his life commences, and here the richest fruits of his former labors and preparations matured. Berlin was indeed the most favorable city for such an object as he had in view. Nowhere were the means for study and instruction so abundantly and generally supplied. Both the University and the Military School displayed great scientific activity, which was kept alive by eminent scientific men, into whose circle Ritter soon entered as a highly esteemed member. His lectures were soon well attended at both institutions. Besides he kept up an active intercourse with other scientific celebrities, among whom Leopold von Buch, but especially Alexander von Humboldt may be mentioned, whom he highly esteemed, and with whom he lived on terms of the most intimate friendship. Moreover, Ritter lived here in the midst of his nearest relations; one brother lived in Berlin, another near the city, and among his dearest friends, was his former pupil, Hollweg, then also professor at the University. All this encouraged him much, and instigated him in various ways to still farther study and advancement. Ritter's activity was now, besides the duties of his office, chiefly directed towards preparing a second edition of his Erdkunde, of which the first volume appeared in the year 1822. It was much enlarged and in every respect more complete than in the first edition, although now only comprising Africa. The continuation of this work however suffered a long interruption

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and the chief reason for it was, that his official duties claimed his attention more and more. Accordingly he entered, though only for a short time, as a member, the scientific examining commission for history and geography, and soon after the death of his friend Woltmann he also undertook the historical lectures at the Military School.

In 1825 he was appointed Director of Studies to the corps of cadets. Besides, he instructed Prince Albrecht of Prussia in history for many years, and received, especially during the winter months, frequent invitations from the Crown-Prince to lecture on history and geography before him and some of his nearest relatives and friends. Similar invitations came also from other sides, and he very often complied with them. Thereby a very considerable part of his time and strength was claimed, as he was always wont to apply the utmost care and scrupulousness to all his subjects. Nevertheless by his extraordinary diligence, which was a necessity to him, assisted by a robust healthy body and by his collectedness and freshness of mind, he still found leisure to work and to promote and advance his scientific labors, which were never lost sight of. The external fruits of these labors were however confined in that period, to his reports in the Academy of Science, of which he was a member after 1822, and to brief essays, like the one on India in the Berlin Almanac for 1824. Many of the results of his studies were communicated to the geographical society, which he, in 1828, had founded in connection with several friends, and of which he was the chief supporter. Those travels which he regularly undertook during the long fall vacations were of the highest importance for him in every respect. They not only served him for bodily and mental recreation, but were also very useful for the advancement of his geographical studies, whether he was occupied in the observation of nature itself, or by investigations in important geographical centres, as Vienna, Paris, London, and other places. These journeys extended in very different directions over the countries of Central Europe and sometimes occupied the larger part of summer. The most extensive and important were: a tour to Greece, Constantinople, through Bulgaria, Wallachia, Sieben-Burgen and Hungary; repeated travels to Paris, through the southern, and at another time, western part of France, and the Pyrenees; through Belgium and Holland; through Denmark, Sweden and Norway.; to London, and through a part of England. He often visited and explored, but always in different directions and with different objects, the middle and southern part of Germany, the system of the Alps in its different parts, Switzerland, and the northern part of Italy. The results of these journeys were a great variety of impressions and observations (mostly recorded and narrated in his detailed

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and highly interesting letters to his family), oral communications of a most varied character, and the establishment of manifold personal relations and connections.

In the year 1831, Ritter withdrew from all business and labor foreign to his geographical studies. He felt that if he would advance geographical science, which he considered the task of his life, he must concentrate his powers. Thus, when he again had leisure, the fruits of his studies became more apparent. There now appeared, from the year 1832, in quick succession, that series of volumes on Asia, of which he concluded the nineteenth a few weeks before his death. This work will be a lasting monument of his genius, and a standard work for all ages, however great the progress of geographical science may be hereafter.

The author's name grew in proportion with the progress of the work, his acquaintances increased in all civilized countries of the world, and his influence upon the course of geographical investigation and science was greatly augmented. He became one of the most important personal centres for the science, since he possessed an immense store of knowledge, and a sound judgment, and took a most active interest in all questions relating to this subject. He entered into everything, even if trifling or troublesome, with an amiability and urbanity that never tired.

He received marks of acknowledgment and distinction of all kinds. Most of the learned societies, in and out of Europe, made him a member, many Orders were given to him, and his Sovereign gave him frequent proofs of his personal favor during the many years of his residence at Berlin. Ritter occupied undoubtedly, as savant and author, one of the most exalted positions among his contemporaries. But he was not less great as a teacher also. There were few lecturers who exercised such an invariable power of attraction as he did. When in 1820 he first announced his lectures on general geography, no hearers came forward, and in the course of the term only a few presented themselves. Ritter began his lectures, but even in the next semester hearers were yet scarce, and this must not be wondered at, as but a few of the students had heard anything of Ritter, and the great majority of them considered geography as something hardly worth the hearing. This state however soon changed, and already in 1823 Ritter wrote in his diary: "Full auditory, I must take a larger one." The numbers of his hearers increased from year to year, so that sometimes even the largest auditory scarcely could hold them. Ritter was now looked upon as the one whose lectures had to be attended by every student of a high scientific aim. Which of his numerous hearers does not remember with gratitude the pleasant and instructive hours of his lectures? Ritter showed a most perfect

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tact, acquired by many years' experience; he knew how and what to select from the immense store of matter over which he had a perfect command; he knew what was best adapted to a verbal exposition and from what the greatest benefit would be derived. His delivery showed that he had deeply and thoroughly mastered his subject, which he always elucidated by drawings, placed on the black-board with great ease. Every one of his hearers felt the importance of the subject, perceived Ritter's deep scientific researches, and was delighted at the results, which were made so accessible, and at the improvement gained by means of them. His lectures were always instructive and always excited to farther study. His delivery was dignified, but everywhere and under all circumstances unassuming. His purity of mind, his modesty and amiability shone forth everywhere, and exercised a peculiar charm especially on those that came in closer contact with him. None ever approached him without meeting a most friendly and hearty reception. No effort in science, however imperfect, was made, that he did not acknowledge and encourage by his counsel and assistance. Egotism was entirely foreign to him; he was the truest and most affectionate friend; in his family most tender-hearted and loving, and his greatest pleasure was to see those around him happy. He himself was without children, but he was a father to many that were comparatively strangers to him. His mildness of temper exercised a most soothing influence upon all; his peace of mind, pervading his whole nature, could not easily be shaken, even by severe losses, such as the death of a dear sister and of his beloved wife in her full strength, both occurring in the course of a few days. These noble qualities of mind were the precious fruits of a strong and living faith. Ritter was a Christian in the full meaning of the word, although never saying very much on the subject and never raising himself up as judge of the religious belief of others. The holy word of God accompanied him everywhere, and to confirm its truth by the results of his investigations was his highest joy. His own words found after his death, are the best testimony: 'Although at present, while preparing for a journey to the western part of France and the Pyrenees, I am healthy and well, life nevertheless lies in the hand of God, whose mercy and grace has guided my fate so wonderfully and gloriously, that I cannot but sing to him, the Allgood, praise and glory with all my power, in all my thoughts and actions. Should it not please him to let me return to my beloved family and to my calling, but should he assign to me another place in his heavenly kingdom, that I may obtain happiness, a happiness which already here sometimes has moved me to tears of joy, then I ask my friends not to grieve over my going home, for all that the Lord does is done well.

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My eternal fate my Savior in his great mercy will decide. In deep acknowledgment of my infirmities and sins, I am still full of trust and confidence, since I know that my Redeemer lives, who will make his people partakers of the mercy of the Eternal and Just one.'

Ritter's health was generally good. His constitution was strong and was hardened by exercise. His numerous travels, often on foot, renewed his strength, when weakened by close application to his fatiguing scientific labors. In his last years, however, many infirmities were felt. The Teplitz medicinal springs had relieved him several times, and Ritter tried them again in July 1859, but this time without relief. Great heat and hot baths seem to have weakened him more, and this weakness was still more increased by frequent hemorrhages from the bladder; he lost his appetite, and his strength begun rapidly to fail, even when his appetite partially returned. He died September 28, 1859, at 10 o'clock A. M., and was interred October 1, in the Marienkirchhof by the side of his beloved wife, who died in 1840.

MR. LENTZ'S REPORT OF HIS EXPLORATIONS IN PERSIA AND AFGHANISTAN.—We have received through the Smithsonian Institution, Washington, reports of the meetings of the Imperial Geographical Society of St. Petersburg, Oct. 7, and Nov. 4, 1859. From the former of these we translate the following account of the Russian expedition under Messrs. Khanikoff and Lentz into Afghanistan and Persia.

Mr. R. Lentz, who took part in the expedition to Khorassan, presented some interesting information in regard to the scientific results of his travels during the sixteen months which he passed in Persia and Afghanistan.

The main object of Mr. Lentz in his travels was to determine the geographical position and elevation of the places, which he visited; to investigate the three elements of terrestrial magnetism (declination, inclination and force of tension); to ascertain the heights of the mountains; and finally to make meteorological observations.

The expedition arrived at Astrabad, in the province of the same name, early in April 1858. As Mr. Khanikoff, to whom the direction of this expedition was confided, had gone to Teheran, Mr. Lentz employed his time, most profitably, during the absence of the former, in observing the movement of the chronometers and in ascertaining the absolute longitude of the city of Astrabad. He also determined at this place, in the same manner as at Ziared, the three magnetic elements, ascertained the height of some of the summits in the Albourz chain, and made meteorological observations.

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Towards the middle of May, the expedition left Astrabad, and, having passed Albourz, stopped at Schakhroud. Mr. Lentz observed here, as everywhere in his travels, the movement of the chronometers, and determined again the three magnetic elements and the latitude of a great number of peaks in the Albourz chain.

On the first of June, when Mr. Khanikoff had returned, the expedition directed its course towards Meschel, the actual capital of Khorassan. During the whole route Mr. Lentz took great pains to determine the greatest possible number of geographical points and ascertain the elements of terrestrial magnetism. The five weeks of his sojourn at Mesched were spent in works of this kind, and he also made an excursion in the neighborhood in order to effect astronomical and barometrical observations.

The expedition left Mesched in the first part of August and reached Herat during the first days of September; having already determined a considerable number of geographical points, the position of which had been previously unknown.

In Khorassan, Mr. Lentz was chiefly occupied in determining with the greatest possible accuracy the absolute longitude of the most eastern point touched by the expedition. From Herat Mr. Lentz advanced to Tebbes, which is situated at the eastern limit of the salt desert of Khorassan, and then to Birdia-Sand, and succeeded in collecting many valuable additions to science.

In the midst of February 1859, the expedition left Khorassan and took its course towards Lasch, a fortified city and the capital of a little state of the same name. The observations made in this country are particularly interesting. They show that the terrestrial surface rises gradually from Herat to the passage Senhighé-Sia, near the city of Sabzor (Kingdom Hérat), where this elevation reaches a height of 5000 English feet; from this point the country gradually descends to a lake, the waters of which, however, have still an elevation of 1200 feet.

The expedition stopped, in its course, on the eastern shores of lake Zaré, near the place, where the river Kharoud or Adraskan empties into it. Mr. Lentz determined the absolute height and the geographical position of this point. The appearance and the dimensions of the lake change continually, sometimes the northern portion of it is dried up, and only its southern part is seen; sometimes the reverse takes place, according to the quantity of water, furnished by the three principal rivers, which empty into it, viz.: the Kharoud and the Ferraroud in the north, and the Khilmend, which flows not far from Kandahar, in the south. Sometimes it happens, that the waters of the lake divide into two parts, one towards the north, and the other towards the south, and are kept separated by a band of completely dry land as was the case at the time of the expedition. This at least is the ac-

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count given by some Persian travellers, who had recently left the eastern shore of the lake and declared that they had crossed it by following this tongue of land.

Beyond the village of Nekh, the expedition came to a desert of 250 versts in extent. This they crossed in its narrowest part between the villages of Serri Tschakh and Dekhi-Seïf, at a place where in a length of 200 versts no trace of water was found. After a journey of four days they reached Kirman.

This place might be considered as entirely unknown up to this time. It appears from barometrical observations made there, that from lake Zaré (1200 feet high) the surface of the earth rises again up to the villages of Nekh and Serri-Tschakh (4000 and 5000 feet high), when the country gradually descends to its lowest point (900 feet high) in the desert at a place called Schakhri-Lut, but rises again as far as Khubbis, which is situated at the foot of the mountains, at an elevation of 1500 feet, and reaches its maximum (8000 to 9000 feet) at the top of these mountains, then it falls again towards Kirman, which however is still found to be 5500 feet high.

Over the whole area, which extends from Esd to Ispahan, (the point toward which the expedition directed its course,) the countries which border south and east upon the great salt desert, are 3000 to 4000 feet above the level of the sea; the same is the case with those countries which separate Schakroud from Mesched along the northern side of the desert; only in two places more considerable heights are found.

From Ispahan the expedition passed through Zerghendé, 10 versts from Tehran, went beyond the village of Firouska, reached the provinces of Mazanderan and Astrabad, and followed the course of the river Talar, which empties into the Caspian Sea.

Mr. Lentz determined, during his travels in Persia and Afghanistan, about two hundred geographical points; at twenty-eight points he could determine the three magnetic elements (intensity, declination and inclination); at twenty-nine other points, however, he observed only two of them. He ascertained with the aid of the trigonometrical calculus, the heights of about two hundred mountain summits, and about four hundred others were measured by him, and his travelling companions, with the aid of the barometer. Thus Mr. Lentz traced a profile of the whole country, which the expedition visited. Mr. Lentz gives also some interesting results of his meteorological observations, which he never neglected during the whole voyage. The barometrical observations show, with an incredible regularity, the slightest variations of the atmospheric pressure; the variations of temperature are also very regular, and it is interesting to state, that the temperature reached constantly its maximum about four hours after noon, and not two hours, as it is generally the case in our climate.

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Throughout Khorassan the air is usually very little charged with vapors and its average hygrometrical state varies between 20 and 30 per cent. At Schakhroud, Mr. Lentz found only 17 per cent of moisture in the atmosphere, and in the desert near Kirman, only 14 per cent."

SCHLAGINTWEIT'S ETHNOGRAPHICAL COLLECTIONS.—Mr. Joh. Ambr. Barth, of Leipsic, has offered for sale a large collection of plaster-casts taken from the heads, hands and feet of individuals in the different castes and tribes of India, and has published a carefully prepared catalogue of the series. We make the following extract from his announcement:

"Messrs. Hermann, Adolphe and Robert de Schlagintweit, the enterprising travelers in India and High-Asia, having, since the year 1854, had charge of a scientific mission from the India House, have been enabled during their travels, in addition to their researches in physical geography and geology, to devote much of their time to ethnology.

"The various countries through which they passed, some of which have hitherto been but little explored, and others never reached by Europeans, afforded peculiarly advantageous opportunities for pursuing their ethnological researches.

"Besides measurements and photographs, they, in collecting their materials, made also casts of the features of living persons taken in plaster of Paris; 275 casts of faces were thus made, and 37 of hands and feet.

"The moulds have been reproduced by galvanoplastic deposits of copper, which gives without the least contraction the most minute irregularities of the skin with great perfection. This first series however was found not sufficiently strong, and the attempts which have been made to produce the heads in as great perfection as possible, have led to a different method, consisting in making strong metallic casts of zinc the basis, coated with a galvanoplastic deposit of copper, varied in color according to the different degrees of color of the native tribes. To exclude as perfectly as possible the change of the tints by the gradual oxydation of the copper, a thin stratum of colorless varnish has been put most carefully over the heads.

"Together with these casts, measurements of the various proportions of the skull and the body have been taken, which will be spoken of in detail in the work, which Messrs. de Schlagintweit are about to publish under the auspices of the President and Council, now at the head of Indian affairs, who having the same interest in science as the Court which preceded them, have found this collection an object of particular importance.

"This work, 'Results of a scientific Mission to India and High Asia,' is independent of this collection: it is in progress of being printed and is published by F. A. Brockhaus, publisher at Leipzig.

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"The ethnographical part of Messrs. de Schlagintweits' work will chiefly treat of characteristic features obtained by well defined measurements, constantly referring to the casts made. Many other individuals, together nearly 400 persons, have been carefully measured, and the zealous labors of other distinguished, particularly Indian, ethnologists, amongst whom we name Buist, Carus, Cunningham, Davis, Walter Elliot, Falconer, Hodgson, Hooker, Humboldt, Morton, Latham, Owen, Rawlinson, the Stracheys, Sykes, &c., will be found to have been carefully studied for the purposes of scientific generalization.

"We scarcely need add how important objects these facial casts will be for all those who take an interest in such researches, while the interesting nature of the objects themselves, as well as the careful and novel mode of their execution, will render them a most beautiful and important addition to public and private museums.

"This collection has met with great approbation; we mention as particularly important the well known personal interest of the late Baron v. Humboldt."

ADOLPHE SCHLAGINTWEIT'S DEATH IN TURKISTAN.—Our readers are already acquainted with the fact that one of the bold brothers, whose expedition to the Himalayas has attracted the attention of the whole scientific world, fell a victim to his energy. From the surviving brothers, Hermann and Robert, we have received a printed document in which are given all the official reports which have yet reached them in respect to the fate of Adolphe. The conclusions at which they have arrived are thus concisely stated.

"The information from India and Russia, collected from natives by European officers of the adjoining districts, concur but too accurately in establishing the fact, that Adolphe Schlagintweit was killed at Káshgar in Turkistán (Central Asia) in August, 1857, falling a victim to his scientific mission.

"He was recognized as a European after having passed the Karakorúm and Küenlüen, in disguise, where before us no European had ever travelled; he had taken a route more westerly than ours, and had succeeded in penetrating far into Central Asia.

"The reports which have reached us are so various, that they do not of course all agree, as to the immediate cause and particulars of his death; yet it is evident from all of them, that the political condition of these countries, and the circumstance of the deceased being recognized as an officer of the Indian Government, notwithstanding every precaution, essentially contributed to his tragic end. Even with the lively sympathy ever so energetically evinced by England, in the fate of scientific travelers, it will scarcely be possible to succeed in bringing the murderers of our brother to account.

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"According to some reports he perished in consequence of having taken up the cause of some captive Bhot-Rajpúts, British subjects, interceding for them, that they might not be executed or sold as slaves. Other accounts state the immediate cause of his death was, his having been recognized as a European, and fallen by the hand of fanatic Mussălmáns.

"Notwithstanding our most zealous exertions for some months past, in endeavoring to obtain his manuscripts, drawings, etc., we have not yet been successful in learning anything definite about them: still, however, many very important geographical communications have been made to us by his followers, and we are not without hope that from the active sympathy which the Indian Government has always displayed in our scientific mission to India and High-Asia, nothing will remain untried that can tend to the rescue of his last papers."

LETTER FROM DR. LIVINGSTONE.—At the January meeting of the American Geographical and Statistical Society of New York, the following letter from Dr. Livingstone, the celebrated explorer in South Africa, was read by D. W. Fiske, Esq., Librarian of the Society:

Tette Zambesie, Feb. 22, 1859.

"*My Dear Sir*—Having been elected a member of your Society, I take the liberty to send you a short account of our attempt to open the interior of Africa, in the hope that, though it may not appear interesting to your members, it may, at least show my good will and desire to perform a corresponding member's duty.

We entered the delta of the Zambese in May, 1858, taking the most southerly branch we could find, but after ascending about seventy miles we found it impossible to enter the Zambese by that, as the points of junction were filled up with reeds and other aquatic plants. You may have a clearer idea of the region if you bear in mind the fact that the Zambese has in the course of ages formed a delta, which juts out into the ocean, and forms the most prominent part of the coast. The prevailing winds of these quarters beat, almost constantly, against the head of the promontory. These, aided by the oceanic currents, have helped to dam up the main stream, but the pent-up waters have escaped sideways. The main stream called Gualeo enters at the point of the promontory most exposed. To it we went after leaving the southern branch, but saw no possibility of entrance during three days, though her Majesty's ship Lynx has since found a channel in it, after a search of ten days. We then proceeded to examine the side branches and the very first we came to called Kongone, was all we required. There are other good ports, but all in the side branches. There are also communicating branches between these, and those within the influence of the tides are generally deep.

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Having got into the main stream, we found that we had, in going to it and spending a month there, allowed the water to fall considerably. It was, also, so very much lower than usual that the Portuguese prophesied that we could not ascend ten miles. It was said, also, that war was raging, and no one would be allowed to go up, even if he could. Our ship drew nine feet seven inches, and she was under engagement to go to Ceylon. We, therefore, to avoid detention in the river, sent her off, and went up to the seat of war in a small steamer, drawing two feet six inches. We had no difficulty with the "rebels," as they were called—indeed, we got pilots from them, and continued ever after on the best of terms with the Portuguese. They were called "rebels," as they had all been runaway slaves, and bore the marks, in brands on their chests, of their former servitude. Slaveholders here must be civil, for it is so easy to run away, that if slaves go to the Landius, who are of the Zulu family, they never deliver them up. I have never heard of but one case to the contrary, and the owner—a great favorite of theirs—was obliged to give them his full value. This is a digression, but I may finish off by saying that the Portuguese governor attacked the rebels, and they retired before him, there being plenty of iron for all parties.

We continue carrying on luggage up the river till November, when it reaches its lowest point; and with care a flat-bottomed boat would do business even then. We know it now at its very worst, and, as it spreads out to from one to three miles in breadth, it is in many of the crossings not more than two and a half or three feet. Just now the water stands twelve feet above low water mark in November, and we are all quite sure that during at least eight months in each year a steamer of four or five feet could trade without embarrassment. The reason why so little has been known about the Zambese river, has been the branching in the stormy promontory by which it was hidden from navigators. And their easy chair geographers, dreaming over the geography of Ptolemy, actually put down the Zambese as flowing into the sea at Quilimane, which, in his days, it probably did, though not a drop of Zambese water, in ordinary circumstances, reaches that part. Had some branch of the Anglo-American family planted their footsteps on its banks, we are such a babbling newspaper set, the world would have known all about it long ago; and no one would have ventured to play with this river as has been done, making it lose itself and flow under the Kalobaro desert. You may form a better idea of its size if I tell you of one of the branches. We ascended the Shire lately, fully a hundred miles from the confluence, and found it with a two fathom channel all the way up. It varied from 80 to 150 yards in width, and contains no sand banks. It flows in a

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beautiful fertile valley, about twenty miles high, and fringed with mountains of great beauty, well wooded to the top. Mora M. Vala we ascended, and found it 400 feet high. (This, by mistake, is placed on the wrong side of the Shire, in my map.) It was well cultivated on the top, and had several fine little fountains, the waters of which were slightly chalybeate; they have a hot sulphurous fountain at the base, (temperature 174° Fahrenheit). The people had many sweet potatoes, holcus sorghum, and other grains, and pine apples, lemon and orange trees. They were very hospitable, and independent. The vegetation is very different from the plains, and so is the climate; yet with all these disadvantages, no use has been made of it as a sanitorium by the Portuguese, and as far as we can ascertain, this river has never been explored by Europeans before. One part of the luxuriant valley of the Shire is marshy, and abounded in lagoons, in which grow great quantities of the lotus plant. The people were busy collecting the tubers, which, when boiled or roasted, resemble chestnuts. They are thus Lotophagi, such as are mentioned by Herodotus. Another part of the valley abounds in elephants. My companions estimated the numbers we saw at eight hundred. Herd upon herd appeared as far as the eye could reach; and noble animals they were. We sometimes chased them in our little steamer, for the shore branches off occasionally and forms islands. The upper part of the valley is well peopled, and many of the hills are cultivated high up. But never having seen Europeans before, they looked on us with great suspicion. They watched us constantly, well armed with bows and poisoned arrows, ready to repel any attack, but no incivility was offered when we landed, nor were our wooding party molested. We obtained what may be considered reliable information that the Shire actually does flow out of Lake Nyanga. We were brought up by a cataract, but five days beyond this point the water is smooth again, and Arabs come down in canoes from Nyanga thither. Seeing the suspicions we had aroused, we deemed it unsafe to leave the vessel and go overland. But no collision took place. The greatest coward fires first, so, thinking we had as much pluck as they, we did not lift a gun, though we were ready to fire, or rather shoot. We did nothing to make us ashamed to return, and mean to do so next month; and if we have their confidence we may go farther. They had abundance of provisions, and sold them at a cheap rate. Also cotton of two kinds—one indigenous, short in the staple but very strong, and woolly to the feeling; the other very fine, and long in the staple. We brought a number of specimens of their spindles and yarn, and it was quite equal to American uplands; did not offer them any American seed. The cotton plant is met with everywhere, and though burned down annually springs up again as fresh and strong as

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ever. They grow sugar cane too, bananas, &c. The men are said by the Portuguese to be very intelligent, but very mild. The women wear the lip ornament, round one of which I put my pen. The slit is made in the upper lip, at first, by a ring in childhood. The ends are gradually pressed closer together, and cause absorption till a hole is made. This is enlarged by bits of reed, until in a lady of fashion a ring, either hollow or cup shaped, is inserted, and the edge of the lip protrudes beyond the perpendicular of the nose at least an inch. I am thus particular in case our own ladies, who show a noble perseverance when fashion dictates, may wish to adopt lip ornaments.

Above this we have a rapid, called Kebra, or rather Kaora-basa. When the water is low it shows a deep grove, with perpendicular sides. When steaming up this the man at the lead kept calling "no bottom" at ten fathoms, and the top of the walls of the grove towered from 50 to 80 feet above our deck. It is from 60 to 80 yards wide, but at this season is comparatively smooth. There were some cataracts in it which high water obliterates. This steamer is too weak to ascend. She being only ten horse power, and her plates $1\frac{1}{16}$th of an inch thick, we dare not try her in the rapids. We shall work down here some time yet. I long to lead back my faithful Mackalolo, who are still at Telle, though thirty of them died of the small pox, and six were killed by a neighboring chief.

I shall refer to one point more before concluding. We were warned by the fate of the Niger expedition not to delay among the mangrove swamps of the Delta—the very hot beds of the fever. We accordingly made all haste to get away, and we took daily a quantity of quinine. The period of the year I selected, though not the most favorable for navigation, was the most so for health; and, thank God, our precautions were successful.

The Kroomen from Sierra Leone have had more of it than we, until a short time ago, when, it being the most unhealthy season of the year, and even to the natives, three of us have had touches of the complaint, but are all now quite well. I have never had a day's illness since my return. We find, too, that so far from Europeans being unable to work in a hot climate, it is the want of work that kills them. The Portuguese all know that so long as they are moving about they enjoy good health, but let them settle down and smoke all day, and drink brandy, then—not a word about brandy in the fever that follows—the blame is all put on the climate. I am, &c. DAVID LIVINGSTONE.

KRAPF'S RESIDENCE AND TRAVELS IN EASTERN AFRICA.—Messrs. Trübner & Co. of London announce as nearly ready for publication a work which is likely to rival in interest the recent volume of Dr. Livingstone. We refer to the narrative of a Missionary Residence in Abyssinia by Dr. J. L. Krapf—one of

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the agents of the Church Missionary Society of London. It will be recalled by our readers that it was by him and his comrade Rebmann that intelligence was first given to the civilized world of the possible existence of snow-covered mountains near the equator—the famous Kilimandjaro. The observations of these missionaries have given rise to many warm discussions, and the correctness of their opinion has been earnestly disputed. In the forthcoming volume we may anticipate that this controverted point will be examined with thoroughness and detail. Aside from this discussion, the work of Dr. Krapf will abound in interesting comments upon his missionary life. His land journeys, which were mostly upon foot, extended 9000 miles. We quote the following from the prospectus of this work.

"Two things may be said of Dr. Krapf which can be affirmed of no other modern African traveler. He has traversed Abyssinia from north to south and from east to west; and further, he has explored the whole coast of Eastern Africa, from Suez to the 10th degree of south latitude, and inspected every place of importance to be found on it. Such journeys and voyages would alone bestow a high value on a volume like the present one, which communicates their most important results. But more than this, the large and interesting country which stretches from the Equator to the 5th degree of south latitude was, from the eastern coast inwards, all but a *terra incognita*, until it was traversed, on foot, by Dr. Krapf, and by his colleague and fellow-worker, the Missionary Rebmann, whose experiences are also included in this work. From the Mission-station at Rabbia Mpia, on the coast, these brave and fearless men prosecuted journeys for at least three hundred miles into the interior, exposed to every possible peril and privation. These journeys were repeated by different routes—the dangers incurred on one seeming only to stimulate to self-exposure to greater dangers on another. Rebmann's three journeys to Dschagga, Krapf's two journeys to Usambara, and two more to Ukambani, in the course of which they explored regions and visited—Bible in hand and Gospel on lip—populations never before seen by European, have rarely been exceeded in interest—religious, adventurous, and geographical. The story of Dr. Krapf's abandonment and wanderings in the wilderness, during his second journey to Ukambani, carries the reader back to the old days of adventurous travel. Scarcely in the whole annals of modern missionary effort has there been anything equal to the spectacle displayed in this section of the volume, of two individuals, each isolated, pursuing again and again, on foot, without external encouragement of any kind, and in the face of every possible obstacle, journeys among ignorant and savage heathen, far away from help, or the hope of help, and confiding solely in the guidance and support of Providence. The

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splendid geographical and ethnological results which were among the rewards of these daring pilgrimages will be found fully chronicled for the first time in the present volume."

SPEKE'S EXPLORATIONS IN EASTERN AFRICA.—At a recent meeting of the Royal Geographical Society of London, Capt. Burton and Capt. Speke both gave a narrative of their explorations in Eastern Africa, which are of particular importance, as our readers are well aware, in connection with the long disputed problem of the sources of the Nile. So much interest has been manifested everywhere in this expedition that we regret that our limits will not permit us to reprint entire the discussion to which these two papers gave rise in that learned association. Sir R. I. Murchison, Col. Sykes, Mr. Macqueen, Mr. Galton, and other well known gentlemen presented their views upon this important topic, a report of which will be found in the Society's Proceedings, vol. iii, No. 6. From the same source we extract the following statement of the remarks of Capt. Speke.

"The region traversed by Captain Burton and myself is divisible into five bands. They all run parallel to the coast, and each of them is characterised by special geographical features. The first is low land between the coast range and the sea. Its breadth is about 120 miles, and its average slope not more than 2 feet per mile. Forests of gigantic trees, and tall grasses, cover its surface. The second band is the coast range of mountains. These are hills in lines and in masses, intersected by valleys, through which the rivers of the east coast find their way. This range is easily crossed, and nowhere exceeded 6000 feet, adjacent to the line of road taken by our travelers. It is capable of cultivation, though neglected, because the slaving forays to which it is subjected drive away the inhabitants. The third band reaches to Unyanyembe. It is a dry plateau, with a slight inclination toward the interior, and ranging in height between 3000 and 4400 feet. Tributary streams, running southwards to the Ruaha, intersect it. The fourth zone is a continuation of the above, but it is better watered, and is studded with granite hills. Here is the water-parting between the streams that run eastward to the Indian Ocean, and westward to the Tanganyika Lake. The Nyanza Lake is situated in this band. The fifth band is a remarkable slope, that inclines to the shores of the Tanganyika. It sinks no less than 1800 feet in 45 miles; it is exceedingly fertile, but harrassed by marauders of the Watuta tribe.

On arriving at Ujija, the party found that the only boats to be had were wretched canoes; while the troubled state of the country rendered it unsafe to explore the lake unaccompanied by a large escort. There was, however, a small sailing craft belonging to an Arab, on the other side of the lake, which would be large enough to contain the entire party; and Captain Speke started

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to hire her, with seventeen savages, as a crew, and four of his own men. He first coasted to Kabogo, a bold promontory usually selected as the starting point, when the lake has to be crossed, and reached it in five days. He describes the shore as wild and beautiful, affording many convenient harbors, and requiring but a little art to make it quite a fairy abode. There were no inhabitants, but an abundance of game,—hippopotami, buffaloes, elephants, antelopes, and crocodiles. The passage across the lake, a distance of 26 miles, was made rapidly and safely, and Captain Speke was cordially welcomed by the Sultan of the country on the opposite side. The owner of the sailing boat was there also, and was ready to afford every assistance; but he himself was on the point of starting on an ivory expedition 100 miles into the interior, and the crew of his sailing boat were, at the same time, his armed escort: he could not therefore spare them. What made the disappointment doubly vexatious, was that this Arab desired Captain Speke's companionship in his intended journey, and he promised the boat on his return. Had Captain Speke been unfettered by time, this would have been an excellent opportunity of farther travel. As it was, he was obliged to go back to Ujiji without the sailing boat, and proceeded with Captain Burton to a more extended exploration of the Tanganyika Lake, which lasted a whole month. The mapping of its southern portion depends on information given by this Arab.

On returning to Unyanyembe, Captain Burton's continued illness again made it necessary for Captain Speke to proceed alone to the northward to explore the Lake Nyanza. He went with thirty-three men, through a line of populous country, less visited by strangers than that which he had hitherto traveled on. There were numerous petty sovereigns who were hospitable enough but very troublesome. The view of Lake Nyanza, with its numerous islands, reminded Captain Speke of the Greek archipelago. The islands were precisely like the tops of the same hills that studded the plains he had just traveled over. In fact, the lake had the features of a flooded country rather than those of a sheet of permanent water, with well marked banks. Its water is sweet and good: those who live near it drink no other.

Captain Speke's explorations did not extend beyond its southern shores. The more northern part of his map is based on native information, especially on that of a very intelligent Arab, whom he has previously met with in Unyanyembe, and whose data, so far as the shores of the lake, were found by Captain Speke to be remarkably correct. This Arab had traveled far along its western shores. In thirty-five long marches he reached the Kitangura river, and in twenty more marches, Kibuga, the capital of a native despot. Between these two places he crossed

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about 180 rivers, of which the Kitangura and the Katanga were the largest. The former is crossed in large canoes; the latter, though much larger and broader, is crossed during the dry season by walking over lily leaves; but in the wet season it spreads out to an enormous size, and is quite unmanageable. The rainy season is very severe in these parts. No merchants have gone farther than Kibuga; but, at that place, they hear reports of a large and distant river, the Kivira, upon the banks of which the Bari people live. This river is believed by Captain Speke to be the White Nile."

Sir R. I. Murchison in reviewing the labors of the two explorers, remarked that "they have, by means of astronomical observations, fixed the position, the longitude and latitude of these two great lakes, and have shown you that whilst one is like other lakes, of which we had previously heard, situated on a great plateau, the other is situated at such an elevation that, as Captain Speke has explained to you, it may very possibly be found to feed the chief sources of the Nile. I will not now argue that difficult question, because I am quite sure there is one gentleman here, if not others, who may dispute that inference. I will, therefore, first call attention generally to the great importance of these discoveries. My friends here have not only traversed the district and furnished us with a good picture of the manners and customs of the inhabitants, but have also brought home rock specimens which enlighten us as to the fundamental features of this country; and to these rocks I will for a moment advert. Captain Burton placed before me this morning certain specimens which show me that at an elevation of upwards of 3000 feet above the sea and towards the interior there are fossilized land shells, showing that from very ancient periods the lands have maintained their present configuration. These deposits, whether purely terrestrial or lacustrine, have been consolidated into stone, and show that the existing internal condition of Africa is that of ages long gone by, as I took the liberty of pointing out to the society some years ago, when treating of Livingstone's first explorations. Another striking feature in connection with this great zone of country is this. You will observe that our friends spoke of remarkable herds of oxen on the banks of the lake Tanganyika, and tribes of people between that vast lake and the coast range, who are a thriving, peaceful, agricultural population, whilst the adjacent districts in the north and south are frequently disturbed by wars for slave-hunting purposes. This is a great fact as indicating a broad line of route by which we may hope hereafter to establish intercourse with the interior country. There is another important fact, though I do not think Captain Speke alluded to it, namely, the absence of that great scourge of parts of southern Africa, the *Tsetse* fly.

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With regard to the physical geography of the country, it is remarkable that all the adjacent rivers fall into the great Tanganyika lake, which was formerly supposed, on the contrary, to afford the sources of the Zambesi river. All theory, therefore, on this subject is now set at rest. Lastly, we come to the subject which is likely, as I said, to give rise to much discussion, and that is the theory upon which I think my friend Captain Speke may rest his claim to our most decided approbation. On my own part I am disposed to think that he has indicated the true southernmost source of the Nile. Now, in saying this I do not mean to deny that the great mountains flanking the lake on the *east*, of which a point or two only is marked on the map before us, do not afford the streams which flow into this great lake. That must probably be the case on the east, just as Captain Speke ascertained from the Arabs that the so-called "Mountains of the Moon" feed the same lake from the west by other streams. You must here recollect that the same Arab sheik who gave him the information which turned out to be correct concerning the existence of the lake Tanganyika also told him of the existence of the Nyanza, which lake was found to be exactly in the position indicated. As Captain Speke has determined that this great lake Nyanza is nearly 4000 feet above the sea, it may well, indeed, be the main source of the White Nile. Everything (as far as theory goes) being in its favor, this view is farther supported when we reflect on the fact that the tropical rains cause these upland lakes and rivers to swell and burst their banks at a period which tallies very well with the rise of the Nile at Cairo. These, then, are grounds which I think must go to strengthen the belief of Captain Speke, and I may, therefore, repeat what I stated at the anniversary, that highly worthy as Captain Burton was to receive a gold medal, not only on account of this great expedition which he led, but also for his former gallant and distinguished expeditions, Captain Speke, who now sits at your Lordship's left hand, is also entitled to a gold medal of the Royal Geographical Society."

H. Schlagintweit on the Salt Lakes of the Himalayas.—At a recent meeting of the Royal Geographical Society of London, Mr. H. Schlagintweit exhibited some chromo-lithographic sketches of the Himalayan Mts., and in commenting upon the remarkable erosion which takes place upon that range, he spoke as follows of the salt-lakes which form a peculiarity of that region:

"Another consequence of the erosion is the gradual drainage of fresh water lakes, or their conversion into salt water lakes. It is very characteristic for the Himalayas, and in this respect they differ essentially from most other mountain systems in the world, that hardly any fresh-water lakes now occur. The only

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few lakes of any considerable extent which have been made known by Captain Strachey, Captain Speke, and Major Cunningham, as well as those we visited besides, are all salt water. But the explanation we think we must give of this phenomenon is different from the explanation formerly given. Some have thought that a raising of the country might have caused a general drainage. We think that supposition rather improbable, from the recent strata round these salt lakes being all horizontal, and the outlets of these salt lakes being in a different direction in reference to the horizon. If any raising of the country had effected the drainage of the salt lakes, the effect would have been a perfectly different one, according to the position the outlet of these lakes had in reference to the points of the horizon, a modification which is nowhere met with.

"The Tso mo Ri ri and the Tso mo Gnalari, the two great salt lakes of Rupchu and Pankong, of which drawings are presented, happen to be a good example of two large lakes, being about equally salt, with differently directed former outlets, and with quite horizontal banks of detritus and of watermarks along their circumferences. The gradual progress of the erosion of the valleys seems to us to be also the chief cause of the gradual transformation of freshwater lakes into saltwater lakes in Tibet.

"By this progressive excavation thousands of square miles, still marked as former lakes by the form of the surface, have been emptied, and the consequence is that the local evaporation could no more keep the equilibrium with the precipitation; in consequence the lakes, of which parts remained undrained on account of their greater depth, now gradually became more and more salt."

JOURNAL OF THE ROYAL GEOGRAPHICAL SOCIETY OF LONDON.—We have just received the twenty-eighth volume of the Journal of the Royal Geographical Society of London. Like the previous parts of this series it is full of important contributions to our knowledge of the physical geography of every country where British enterprize is manifested. We have heretofore quoted from the anniversary address of the President, Sir R. I. Murchison. To many of the other articles we shall have occasion to allude. The following is a statement of the contents of the volume:

Articles—1. Journal of the North Australian Exploring Expedition; under the command of Augustus C. Gregory, Esq. (Gold Medallist, R.G.S.); with Report by Mr. Elsey on the health of the party.—2. Notes on the Physical Geography of Northwest Australia; by Mr. James S. Wilson, Geologist to the North Australian Expedition.—3. Journey from Colesberg to Steinkopf in 1854–5; by Robert Moffat, Esq., F.R.G.S., Government Surveyor at the Cape.—4. Journey from Little Namaqualand eastward, along the Orange River, the Northern Frontier of the Colony, &c.,

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&c., in August 1856; by Robert Moffat, Esq., F.R.G.S., Government Surveyor at the Cape.—5. A Coasting Voyage from Mombasa to the Pangani River: Visit to Sultan Kimwere: and Progress of the Expedition into the Interior; by Captains Richard F. Burton, commanding the East African Expedition, and J. H. Speke, F.R.G.S.—6. Explorations in the Desert East of the Haurán, and in the Ancient Land of Bashan; by Cyril C. Graham, Esq., F.R.G.S., &c.—7. Contributions to the Knowledge of New Guinea; by Dr. Salomon Müller,—8. On the supposed discovery, by Dr. E. K. Kane, U.S.N., of the North Coast of Greenland, and of an Open Polar Sea, &c., as described in the 'Arctic Explorations in the years 1853, 1854, 1855;' by Dr. Henry Rink, M.D., Inspector in Greenland for the Danish Government.—9. The Yang-tse-Keang, and the Hwang-Ho or Yellow River; by William Lockhart, Esq., F.G.R.S.—10. Extracts from a Journal kept during a Reconnaissance Survey of the Southern Districts of the Provinces of Otago, New Zealand; by J. Turnbull Thomson, F.R.G.S., Chief Surveyor.—11. Observations relative to the Geographical Position of the West Coast of South America; by Carlos Moesta, Director of the National Observatory, Santiago de Chile, May 29, 1856.—12. Excursion made from Quito to the River Napo, January to May, 1857; by Dr. William Jameson.—13. Description of the State of San Salvador, Central America; communicated by John Power, Esq., F.R.G.S., of Panama.—14. On the Latitude and Longitude of some of the principal places in the Republic of Guatemala; by A. van de Gehuchte.—15. On the Fine Regions of the Trade Winds; by Thomas Hopkins, M.B.M.S., Vice-President of the Manchester Literary and Philosophical Society.—16. Remarks upon the Amount of Light experienced in high Northern Latitudes during the absence of the Sun; by Captain Sherard Osborn, R.N., C.B., F.R.G.S., Officier Légion d'Honneur, &c.—17. Notes on the River Amúr and the adjacent Districts; by MM. Peschurof, Permikin, Shenurin, Vasilief, Radde, Usoltzof, Pargachefshi, &c.

Illustrations.—1 and 2. Map to illustrate the Route of the North Australian Expedition, and Mr. Wilson's Paper on the Physical Geography of N.W. Australia.—3 and 4. Map to illustrate Mr. Moffat's Journey from Colesberg to Steinkopf; and from Little Namaqualand Eastward, along the Orange River.—5. Map to illustrate the Progress of the East Africa Expedition.—6. Map to illustrate Mr. Cyril Graham's Explorations East of the Haurán, &c.—7. Map to illustrate Dr. Rink's Paper on Dr. Kane's Arctic Explorations.—8. Map to illustrate Mr. Thomson's Survey of Otago.—9. Map to illustrate Capt. Sherard Osborn's Paper on Light in the Arctic Regions.—10. Map to illustrate Notes on the River Amúr.

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ART. XXIII.—*On the Species of Calceola found in Tennessee: Calceola Americana;* by Prof. J. M. SAFFORD.

FOR many years it has been known that a species of *Calceola* occurs in the marly and glade-forming limestones of Western Tennessee. This species has been considered to be identical with the European *C. sandalina* of Lamarck, an error (for such I hold it to be) which has contributed much to the confusion that has existed with reference to the age of the limestones mentioned. Individuals of the species are frequently found upon marly glades of Decatur, Perry, Wayne, and Hardin counties. The identity of the species with *C. sandalina* (and a few other determinations of the same kind) once taken for granted, it was an easy matter to designate the rocks of these glades "Devonian."

Since my attention has been called particularly to this species, I have regarded it as distinct, and now propose for it the name *Calceola Americana.*

In the first place, its different geological position would, at least, indicate a distinct species. It is without doubt an Upper Silurian fossil, and moreover belongs exclusively, so far as my observations have extended, to the Niagara Period.* The grey marly limestones of *the glades*, although much alike lithologically, are generally easily separable, by their fossils, into two beds, the lower one representing, in part, the Niagara Period, and the other the Lower Helderberg. It is to the former of these that our *Calceola* belongs. Among its associates are *Orthis elegantula*, *Platyostoma Niagarensis*, *Caryocrinus ornatus*, *Eucalyptocrinus decorus*, &c. *Halysites escharoides* and *Cladopora reticulata* have been observed in a local coralline limestone resting upon the bed containing the *Calceola.*

In the second place, the characters which separate it from *C. sandalina* are well marked. In general form, it is much like the European species, but differs in the following particulars:†

1. In *C. sandalina* the central cardinal process or tooth of the large valve is divided longitudinally by a shallow linear groove, making the tooth apparently double; in all my large valves of *C. Americana* this tooth is not grooved, but, on the other hand, is rounded and smooth along its summit; it is moreover longer and larger than in the European species.

* The *Niagara Period*, as here used, is equivalent to the New York rocks from the Oneida Conglomerate to the Niagara Group inclusive.

† In making these comparisons I have before me forty specimens of the American species and seven of the European. Of the first, one is an excellent specimen with both valves united, two are good specimens of the small valve, and the remainder are large valves. The small valves are seldom found. I have seen altogether four of them. Of my European specimens, one is entire with both valves, another is an excellent small valve, and the rest are large valves all in good condition.

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2. In my specimens of *C. Americana* the rows of punctures, so conspicuous on the internal surfaces of *C. sandalina*, are not seen.

3. Within the largest valve of our species, in the older individuals, there is adjoining the hinge line, and on each side of the cardinal process, (but separated from the latter by a deep groove,) a prominent callosity. In very old specimens, these callosities nearly fill up the back portion of the cavity of the shell, and, at the same time, nearly obliterate the striæ or ridges which run forward from the hinge line. Most of the inner surface of the large valve has an irregular wavy appearance, indicating a vesicular structure, which, in fact, the mass of the valve has.

4. The small valve (the dorsal), so far as I have seen, has externally no proper cardinal area; its *apex* is not immediately over the hinge line, but is removed about one-fourth of the length of the valve towards the front, the cardinal edge being bevelled off from the apex to the hinge line. The lines of growth are prominent along this bevelled edge; so they are too on the cardinal area of the large valve.

5. The external surface of *C. Americana*, is obscurely marked longitudinally in front by striæ, which, so far as they have been seen, are coarser and less numerous than in *C. sandalina*.

There are other points of difference which appear to be constant, but those given are sufficient to characterize the species. Upon a future occasion the fossil will be illustrated by the proper figures.

Lebanon, Tenn., Feb. 1, 1860.

Art. XXIV.—*The Great Auroral Exhibition of August 28th to September 4th*, 1859.—3d Article.

In the two preceding numbers of this Journal* we have given observations of the Aurora of Aug. 28th to Sept. 4th, from numerous places in North America. We now continue our record of the phenomena, and intend in a subsequent number to present a summary of the observations made in other parts of the world. We are indebted to Mr. Benj. V. Marsh, of Philadelphia, for a considerable number of the following notices.

1. *Observations at Montreal* (lat. 45° 31′), *by* Dr. Archibald Hall.

August 28th about $8^h\ 20^m$ P. M. the sky was about seven tenths obscured by massive cumuli, when in the interval between them I observed streamers of a ruddy tint passing from the south towards the zenith. The wind was N.N.W. and blowing

* Vols. xxviii, p. 385; xxix, p. 92.

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rather stiffly. About 10 P. M. the streamers seemed to converge towards the zenith in all directions, and to possess a deep ruddy tint. There was a large cumulus cloud in the W.S.W. and from a clear space beneath it a streamer shot upwards and distinctly traversed the cloud, illuminating it vividly. The same phenomenon was witnessed by another observer at the other end of the city.

At 2h 10m A. M. Sept. 2d, a brilliant aurora was seen in the vacant space between masses of huge cumuli and lasted until 3h 30m A. M. The sky was at first of a bright coppery red tint, and the light emitted so great that it was possible to read moderately large print by it. This space became interspersed with streamers of a rich roseate hue stretching to the zenith. The manifestation was chiefly observed in the W.S.W.

Sept. 2, at 9h 40m P. M. we had another auroral display. The streamers were mostly white, springing from three well-defined arches, stretching between the N.E. and N.W. They flickered magnificently about 10h 20m P. M. in the zenith, where they formed a huge corona having a tent-like appearance. These displays have been the finest seen here for many years, and it is to be regretted that on the two first occasions, clouds should so far have concealed them from our view.

2. *Observations at Montreal* (lat. 45° 31′), *by* Prof. CHARLES SMALLWOOD, LL.D.

Aug. 28th at 9 P. M. we had a splendid aurora extending over nearly the whole horizon with the exception of a small space in the south and S.W., varying in color from a pale yellow to deep orange and violet or crimson, and nearly as light as when the moon is at its full. The aurora was first noticed between 8h 30m and 9h P. M., and this appearance lasted, with modifications, till nearly sunrise.

On the following night, Aug. 29th, there was also a fine display, but not to be compared in brilliancy to that of the previous evening. The sky was on this occasion cloudless, and a few streamers were occasionally seen tinted with a pale violet color.

The most remarkable incident was the unusual amount of atmospheric electricity present. At 9 P. M., Aug. 28th, the electrometers indicated a maximum of 250 degrees in terms of Volta's electrometer No. 1, of a positive character (but almost constantly varying in intensity); an amount equalled only during the thunder storms of summer, and the heavy snow storms of winter. The amount during the following day and night indicated a maximum of ten degrees, which is however somewhat above the usual average.

The appearances would lead to the opinion that the clouds might have been the medium of conducting the atmospheric

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electricity to the earth, for the indications of the electrometers were such as are observed during the passage of clouds charged with electricity, and this phenomenon seems to have extended to the wires of the electro-magnetic telegraph.

The following day and night indicated a small increase on the usual amount of electricity, which may be owing to the continued presence of the aurora, or in some measure to the decrease in temperature.

Similar indications of the electrical state of the atmosphere during the aurora were never observed here, although its effect on the magnetic telegraph has been before witnessed.

3. *Observations at St. Paschal* (lat. 47° 40′ N., long. 67° 40′ W.), *communicated by* Prof. C. Smallwood.

It was about 10 P. M. Aug. 28th, that the aurora was first noticed here. It was a magnificent display which threw out streamers from the zenith all around the horizon, and the light was nearly that of the day. I believe it was visible at Lake St. John on the Saguenay, lat. 48° 8′, long. 71° 9′.

4. *Observations at Halifax* (lat. 44° 39′), *by* Lieut. N. Home, *of the Royal Engineers.*

Aug. 28th at 5 P. M., I remarked a long narrow belt of cloud from E. to W. having a peculiar orange-white appearance.

At 8 P. M. I observed this cloud (which in the interim appeared to be stationary) suddenly to become luminous, particularly at its eastern extremity. This cloud was about 10° wide, and appeared to extend from horizon to horizon; no other clouds were visible.

Soon after 8 P. M. two arcs of light N. and S. appeared, that to the south being the brightest. Under both these arcs the heavens were dark; but observers were uncertain as to whether the darkness was cloud or not. No stars were seen below the arcs, although quite visible above them.

At 9¼ P. M. the appearance was as if these two arcs were a small circle of the sphere, dipping to the south at an angle (measured by sextant) of 15° to the horizon, and 12° above it. The corona being formed at a point (by sextant) 10° south of zenith. There was only one band or arc of light, and that was continuous around the whole heavens. There were two remarkable patches, one due west, at an elevation of about 36°, having a red color; and the other east by north, at an elevation of 25°, having an orange color. These points were brightest during the whole display.

Two distinct sets of streamers appeared to be formed; one set from the arc of light, the other from the corona, which seemed to be constant or nearly so; as during the five hours I watched

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the aurora, there appeared to be always light in or near the zenith, and always in the arc. The streamers were the variables, and appeared to work from W. by N. to south. I think they worked along from E. to W., but another observer said from W. to E. To the south they were so vivid and rapid it was not easy to tell.

A volume of light, as if a quantity of burning spirit was poured over the heavens, appeared to stream across from north to south quite independent of the streamers. The corona sent down rays, but it seemed to be only half way; the streamers from the arc meeting them and toothing in, appearing to alternate, short and long ones.

5. *Observations at Grafton, Canada West* (lat. 44° 3′ N., long. 78° 5′), *by* JAMES HUBBERT.

Aug. 28th at $8^h\ 30^m$ P. M. my attention was attracted by the peculiar appearance of the southwestern sky. Streamers and flashes of light of a pale yellow and red color were rising, sailing towards a point 8° south of the zenith, and meeting others from the N.W. and north. By $8^h\ 53^m$ the whole northern and eastern sky was a blaze of lurid light, which seemed most dense in a band seven degrees wide, extending from N.W. to S.E., along which there was a constant succession of streamers and nebulous patches, exhibiting every shade of white, yellow and red. Columns were now darting up from all parts of the horizon. The aurora hung along the south, in a line at a maximum height of 17°. This from $8^h\ 50^m$ to 9^h was very perfect; while a similar arch but much less regular was formed in the north, reaching to the east. The latter had an altitude of 27°, and like the other seemed to rest on a dark bank. The first corona that I observed was formed at 9^h, at an altitude of 70°. It was imperfect and vanished almost instantly; but was soon replaced by another in nearly the same spot. This in turn gave place to another still more complete. From $9^h\ 15^m$ to $10^h\ 15^m$ the drapery was gorgeous in the highest degree. A diffused light made surrounding objects very distinct. Cocks crew, and the animal world seemed to think that day was dawning.

I noted constant changes which were little more than a repetition of the above till 3^h in the morning. The corona was distinct from $12^h\ 37^m$ to $1^h\ 5^m$. The color was white, merging into every shade of yellow, crimson, scarlet, purple, and sometimes tinged with green. I listened with great earnestness, and once or twice thought I heard a rustling noise, but I think it must have been the wind. When the wind was hushed, as it was at intervals in the latter part of the night, not a sound could be heard. Just at 10^h the aurora, after nearly disappearing, became intensely brilliant, equalling the light of the moon at the

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last quarter. The aurora continued till daylight, when it gradually faded away.

The evening of Aug. 29th was clear; and at $8^h\ 45^m$ the aurora was again visible, but very much less extended and brilliant than on the preceding night. There were thin, misty clouds of a nebulous appearance, with occasional streamers of a pale white light, sometimes merging into red.

On the night of Aug. 30th I observed no unusual appearance.

Aug. 31st the sky was covered with a dense mass of clouds; but the existence of the aurora was evident from the clearness of the night. After midnight the clouds disappeared, and the display was magnificent. All the characteristics of the night of the 28th were repeated; but the arch was rather lower along the southern horizon. A fiery bank was formed in the south, from which rays were constantly darting upward, and the whole sky was a gorgeous canopy of crimson and gold. This was most vivid from $1^h\ 15^m$ to $1^h\ 45^m$, but was continued till almost daylight.

Sept. 1st was cloudy, and I saw no indications of the aurora.

Sept. 2d there were dense clouds, yet the aurora might be occasionally seen. It was confined to the N. and N.E., and was particularly bright from $9^h\ 51^m$ to 11^h.

Sept. 3d was clear. At $8^h\ 50^m$ the aurora appeared in the N.N.E. and W. The light was yellow and white, with traces of crimson and green. At 10^h an imperfect corona was formed, but almost instantly disappeared. Others followed, but none of them were complete.

Sept. 4. The same phenomena were observed, but much diminished in brilliancy.

Sept. 5. No trace of the aurora was visible.

6. *Observations at Rochester, N.Y.* (lat. 43° 8′), *by* Prof. C. DEWEY.

The aurora of Aug. 28th was exceedingly splendid both before and after midnight, with the corona a little south of the zenith; and exhibited many colors, with red or crimson predominant.

Sept. 1st. The aurora began late in the evening, and exhibited the usual appearances.

Sept. 2d at 1 A. M. it was cloudy, but very bright and red in the N.E.; the light increased rapidly and extended. At 2 A. M. there was a magnificent glow of red over the southeast, south and southwest; yellowish green, green and crimson, forming a gorgeous display quite down to the south horizon as seen from the housetops. A splendid corona was formed just south and east of the zenith, with splendid coruscations from towards the horizon up to the zenith. The streamers shot upward towards where the corona was formed, but none went to it.

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Sept. 3d. The aurora was considerable at 9 P. M., and over the north, streamers were shooting upwards. At 10 P. M. there was a bright red space in the N.W. or W.N.W. with white and greenish-white bands on each side. The flashing of light upwards soon began, and the streams or clouds of aurora were splendid. At 10½ P. M. the corona began a little S.E. of the zenith, and was very splendid, towards which the streaming upwards was on all sides but less from the south. At 11 P. M. it nearly disappeared. This aurora was equal to that of November, 1837.

7. *Observations at Newburyport, Mass.* (lat. 42° 48′), *by* Dr. Henry C. Perkins.

The aurora of Aug. 28th was the most splendid ever witnessed at Newburyport by the present generation. About 7½ P. M. the eastern sky seemed to outvie the western, but with reversed colors, the pink of the morning taking the place of the golden hues of the setting sun. In a few moments these hues were repeated in the northeast and the west, and the yellowish-white luminous arch had passed the zenith and was fast covering the southern sky, and at 7¾ P. M. had enveloped Antares. At 7h 52m the star Tau Scorpii was at the southwestern edge of the luminous fringe. At 9 P. M. Lambda Scorpii marked its southern border. At 9¾ P. M. the northern border of a luminous arch passing from the east to the west, was marked by Nu in the right foot of the Swan, while the whole southern and southwestern heavens were glowing with streamers rushing to the pole of the Dipping-needle, the whole northern heavens being entirely destitute of the auroral light. At this juncture, in an instant as it were, the merry dancers sprang up from the northern heavens, and at 10 P. M. the whole celestial vault was glowing with streamers, crimson, yellow, and white, gathered into waving brilliant folds, a little to the south and east of the zenith, affording a canopy of the richest tints and most magnificent texture. The light was examined by the polariscope, and found not polarized. The stars were so lost amid the effulgence as to render it somewhat difficult to make out the constellations. Print might be read by the aid of a small lens, and the time ascertained from the watch by the simple light of the aurora.

During the evening of Sept. 1st the aurora was quite bright, and about a quarter to one (Sept. 2) it spread very rapidly, and soon enveloped the whole heavens. At about one the spectacle was magnificent, a perfect dome of alternate red and green streamers being formed, and the light being so great that ordinary print could be read as easily as in the day-time. It continued till morning.

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8. *Observations at Lunenburg, Mass.* (lat. 42° 35′), *by* Prof. WILLIAM B. ROGERS.

The aurora of Aug. 28th has rarely been equalled in this latitude, and the meteor was repeated with more or less splendor for the eight following nights. The displays of Sept. 1st and 2d were scarcely inferior in beauty to that of the 28th, while that of Sept. 2d, in some of its features, was the most interesting of them all.

On the evening of Aug. 28th, throughout most of the northern half of the sky, the stars were dimmed by what seemed to be a luminous haze, which in some places quite eclipsed their light, and which itself glowed changefully with a golden and crimson coloring. In the earlier stage, the obscure space on the northern horizon had not assumed the usual arched form, and was sufficiently translucent to show a few flaky clouds, floating within its confines. At $8^h\ 20^m$ this dark space had become more opaque, and had moulded itself into a symmetrical arch, bounded by a broad luminous band.

At $9^h\ 30^m$ the display attained its highest magnificence. The dome of the heavens was hung around with white and golden and rose-tinted streamers converging from all quarters towards the magnetic pole. Over the glowing stripes of this marvellous pavilion there came broad flushes of the richest crimson light, until it suffused all the upper part of the sky, and the whole southern quarter except a narrow space next the horizon.

At $10^h\ 30^m$ nothing remained of this wonderful spectacle but a faint auroral arch low down in the north, accompanied by a few dim streamers.

The aurora recurred in great splendor between 1 and 2 A. M., Aug. 29th, when the crimson color was particularly remarkable. At $3^h\ 30^m$ A. M. there was a fine auroral arch in the north, with a long array of streamers rising from it.

Sept. 2d, a clear sunset was followed by a peculiar greenish and purplish light extending round the horizon, even beyond the north. Over the northeast quarter, the air to the height of 30° had a dark opacity, which had the effect of arresting the light coming from beyond.

At $7^h\ 30^m$ P. M. an irregular obscure space began to form along the northern horizon. At $7^h\ 50^m$ a faint arch of white light made its appearance, resting on the horizon a little north of the E. and W. points, and culminating some distance below the pole star. This continued to rise until 8 P. M., when its apex was within a few degrees of the pole.

At $9^h\ 20^m$ a low luminous segment showed itself on the horizon beneath the arch. The latter now resolved itself into an array of bright streamers, with equidistant shadowy spaces between them.

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At 9h 30m the streamers had extended and grown brighter, while the low luminous segment, diffusing itself upward, had merged into the outer arch, which now reached nearly to the pole star. At this moment the arch began to send off successive waves of light, rapidly following one another towards and beyond the zenith. In a few seconds this wave movement gave place to more rapid and seemingly broken pulsations, flitting upwards in close succession through the northern, eastern and western quarters of the sky, and visible, though less distinctly, in the south. This wonderful appearance exhibited everywhere a convergency of the lines of motion towards a point considerably south of the zenith.

When these luminous phenomena were at their height, every spot to which the eye was directed, except the southern quarter near the horizon, was traversed by quickly successive flashes of white, greenish, and pale roseate light, all seemingly moving upwards.

At 10h 30m the pulsating movement again extended over all the northern and part of the southern half of the sky. Innumerable waves of white, yellowish and purplish light chased each other from every quarter towards the magnetic pole, while the crimson flush spread wider and higher from the west.

The various phases of this aurora recurred according to a somewhat uniform order of succession. First, the dark segment on the northern horizon took a regular arched form, and as it rose, became bounded above by a broad luminous curve, at the same time developing one or more bright concentric arches within. The streamers now shot forth from all parts of the luminous zone; and as these increased the upper arch faded away, as if it had expended itself in producing them. And now the lower arch took its place, to be obliterated in its turn by a like seeming process of exhaustion. At length, one of the grander effusions of light coming on, the whole arch was broken up, and the dark segment below was reduced to a shapeless mass. Then there occurred a comparative pause in the phenomena, until the dark segment again took form, with its one or more luminous bands, and a like cycle of development was repeated.

9. *Observations at Steubenville, Ohio,* (lat. 40° 25′), *from the Steubenville Daily Journal.*

The magnificent auroral display of Aug. 28th was unusually interesting. 1st. It covered a much larger space of the heavens than any we ever saw before, at least since 1835. 2d. It lasted from dark until daylight, appearing with the first approach of darkness and only disappearing as daylight gradually overpowered it. 3d. Instead of an arch, shooting up rapid and various colored rays, its first appearance was that of a luminous mist,

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with barely perceptible rays along its southern border, and moving with the rolling motion of clouds, rather than the straight darting motion usually seen in auroras. 4th. It varied in intensity more than any we have ever seen before, twice fading nearly out, and remaining so for nearly half an hour or more, and then kindling up with greater brilliancy than before.

About $7\frac{1}{2}$ P. M. it was a barely perceptible light in the northeast. As the darkness deepened, this luminous spot grew brighter, and moved to the south, till a little before 8 P. M. when the light spreading from it met that coming from the west, and formed an arch about half way between the zenith and southern horizon, and there its advance ended, and it began instantly fading out. It retreated just as it had advanced, only more rapidly, and at $8^h\ 10^m$ there were left only the two centres in the northeast and northwest with a fitful gleam between them. During this retreat, portions of the luminous cloud broke off and floated for some minutes far away from the main body, surrounded by deep darkness, like islands. One of them, and the most beautiful, was a long bright bar in the south, which extended more than half way across the sky from west to east, with a wide sea of darkness between it and the parent cloud, which gradually melted away and disappeared to the westward.

At 9 P. M. the light advanced again, this time with a blood-red tinge in the eastern and western portions, and passed clear to the south as before, but shooting up many and variously colored rays, sometimes from the east, sometimes from the west, sometimes from the north, and from all parts of an irregular luminous arch that bent over the northern horizon about twenty degrees above it. This display faded away in an hour, and at $10\frac{1}{2}$ P. M. there was no light that would attract attention, more than is frequently seen in the north.

About 3 A. M. it blazed up with redoubled brilliancy, shooting up white rays far above the zenith, and making the earth as light as a full moon behind a mist could have done. This time the rays seemed to dart up in broad masses, giving the sky the appearance of being covered with slabs of light, which were tinged with red in the zenith, and rested on a broken irregular arch in the north that in some places fell to the horizon, and in others rose in angular openings to thirty degrees above. During this last display, the pulsations of the aurora were beautifully marked, the rays shooting up in a sort of volley, many hundreds together; while broken and separate masses of luminous cloud were seen in various parts of the sky.

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10. *Observations at Burlington, N. J.*, (lat. 40° 5′), *by* BENJAMIN V. MARSH.

Aug. 28th an arch of light rose in the north, passed the zenith and descended to within about 20° of the south horizon by 8^h 30^m P. M. Soon after this, the whole space overhead was occupied by a dense unbroken cloud of milky whiteness. There was however up to this time a considerable number of small black clouds moving southward, which soon afterward disappeared entirely. These clouds were very thin, and we were for a while in doubt whether they were not patches of clear sky; but by watching their effect upon the stars, we satisfied ourselves that they were clouds.

Still later, about 20° above the south horizon, there was a dense whitish arch a few degrees in width, its lower margin being regular and well-defined. About 30° or 35° above the north horizon was the top of another arch, wider than the first, but not so regular or well-defined. Between these two arches were numerous streams and fragments of white auroral cloud.

Between $9\frac{1}{2}$ and $9\frac{3}{4}$ P. M. there was a perfect corona; the streamers on the south side were short, and mostly white, and moved pretty rapidly westward. Their number at one time was probably five or six. At one time the central space was perfectly clear; but afterwards the streamers ran through it to its centre.

11. *Observations at Crawfordsville, Indiana*, (lat. 40° 3′), *by* Prof. JOHN L. CAMPBELL.

Aug. 28th, the aurora began about 7^h 30^m P. M. with an unusual white light in the form of an arch in the north. At 8^h 45^m P. M. the white light appeared in two brilliant spots about 60° on each side of the magnetic pole.

At 9 P. M. streamers of white, red and pink light in circular currents about the magnetic pole (variation 5° 45′ east) extending beyond the zenith.

At $9\frac{1}{4}$ P. M. the streamers were concentrated into brilliant ones passing nearly along the magnetic prime vertical.

At 10 P. M. streams of white light were formed in the east, and rapidly passed westward, a little south of the zenith. These streams or clouds were entirely separate from each other, and the more northern band, and possessed a real motion. The time occupied in passing was about one second. Not less than twenty flashes passed over. They were formed about 30° above the eastern horizon, and disappeared about 60° above the western. After 10 o'clock the white light in the north became more brilliant, and tinged with red, extended very far towards the south. We traced the red tinge on the east to within 40° of the

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south point, and on the west to within 50° of the same. At 11 P. M. the aurora was still bright in the north.

Aug. 29th, 2 A. M. Very brilliant streams of white and red light filled the northern hemisphere. These streams were perpendicular to the horizon in the north, and were inclined at regular decreasing angles towards the east and west to about 70°. Brilliant flashes passed across the heavens, originating in the northeast, and passing in a southerly direction vanished in the southeast.

At $2^h\ 45^m$ A. M. Heavy bank of red light ten degrees north of east. Patches of white light in north with occasional streams.

At 3 A. M. the whole northern hemisphere was filled with streams of white light with the same inclination as at 2 A. M. At $3^h\ 15^m$ A. M. the auroral storm was at its height. Flashes of red and white light each instant flew across the northern hemisphere. At $3^h\ 30^m$ A. M., a bright band of white light covered the hemisphere except low down in the north; and the incessant flashes in the northeast and east still continued. Long streams of light flashed across the entire hemisphere. The lower part of the band passed through the heavens at an elevation of 40°. In the zenith was displayed a brilliant red bank. In the east, red and white flashes were very brilliant—better defined but not so rapid in transition as at $3^h\ 15^m$ A. M.

At $3^h\ 45^m$ A. M. Magnificent corona in the zenith. Central portion spiral, red and white, changing instantly to a beautiful rose color, with spiral streams shooting forth into all parts of the heavens; the most brilliant streams flowing east and west. The heavens were completely covered with these streams of light.

At 4 A. M. the white light in the north was still very bright, but the dawn obscured the eastern bank.

12. *Observations at Philadelphia,* (lat. 39° 57′), *by* Charles J. Allen.

Soon after half past 8, Aug. 28th, the southern margin of the luminous auroral curtain was well defined, and its position between two fixed objects carefully noted. It was afterwards ascertained by actual measurement that this gave an elevation of about 22½ degrees above the southern horizon.

13. *Observations at Sandy Spring, Md.,* (lat. 39° 9′), *by* Prof. Benjamin Hallowell.

The elevation of the southern margin of the luminous auroral curtain above the southern horizon, Aug. 28th, about 9 P. M., I thought, and mentioned to those with me, was about the meridian altitude of the equator, say 51 degrees.

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14. *Observations at Stockton, California*, (lat. 38° 10′), *from the San Joaquin Republican.*

The aurora of Aug. 28th first appeared about 9 P. M., when a faint white light commenced about north and extended to about east by north. About 9¼ P. M. great streams of red and blue shot up all along the northeastern horizon, but they appeared to shoot highest about mid-way of the light. These streams would faint and brighten in such a wonderful manner, that we imagined some painter in the skies drew his great brush from the horizon up to 40 or 50 degrees, dipped with vermillion, then with sky blue, and then with white and flesh color.

15. *Observations at Sacramento, California*, (lat. 38° 34′), *by* THOMAS M. LOGAN, M.D.

I have observed the aurora only at five different times during a residence of nine years at Sacramento, viz., Dec. 16, 1857, Oct. 27, 1858, Aug. 28, 1859, Sept. 1, 1859, and Oct. 18, 1859. I know of but three other well authenticated instances of the phenomenon having been witnessed in California; one by Geo. H. Goddard at Sonora, Jan. 19th, 1852; and two by Henry Gibbons, M.D., at San Francisco, Jan. 19th, 1852, and Feb. 19th, 1852. This shows the infrequency of its appearance in this State.

The aurora of Aug. 28th, 1859, commenced at 9 P. M. and ended about 3 A. M. next morning. The appearances exhibited during this extended period were so various as to render it impossible to note the particular hours of the different changes. In its perpetual movements and fantastic changes were recognized all of the characteristic features that mark this phenomenon, from its close resemblance to the aspect of the sky before sunrise, to the formation of the luminous arc, darting forth palpitating rays towards the zenith, of white, pale red, and deep blood color. This last mentioned feature was seen in its greatest glory at about midnight; and lambent streamers about this time were noticed to shift gradually from west to east, and vice versa. The summit of the arc was not more than six or eight degrees above the horizon, and appeared to coincide with the magnetic meridian. The lower segment of the arc was not as obscure as in the other auroras observed by us. The most remarkable feature during the whole display, was the long continued gleaming of a dark rose or carmine illumination, particularly at the western extremity of the arc; this rosy light passing occasionally along the belt with a fluctuating movement towards the opposite end. The whole northern sky at one time seemed to be a cupola on fire, supported by columns of divers colors, relieved and intensified by dark shadows or rather streaks. The sky remained almost entirely clear the whole night. This au-

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rora was observed in all parts of the State, and very generally throughout the whole north Pacific region.

The aurora of Sept. 1, 1859, was first observed about 10 P. M. There was seen first a warm glow in the northwest, and two white silvery clouds in the north. Soon the light extended in all directions, until the entire firmament was suffused with a ruddy light so bright at times that the hour could be distinguished on the dial of a watch. At midnight a splendid glowing corona was seen extending from the eastern to the western horizon, and the whole southern hemisphere appeared to be in one continuous blaze. These ever changing phenomena continued to manifest themselves until lost in the dawning day.

16. *Observations at St. Louis, Mo.* (lat. 38° 37′), *from a St. Louis Journal.*

On the night of Sept. 1st we had a most beautiful exhibition of the aurora. The view did not approach its highest grandeur until after 11 o'clock. At first there was a hazy appearance, embellished here and there by faint streaks and tremulous touches of light. Then the wavy pencillings grew stronger and broader, and the light spread until it had crept up to the zenith, when half of the world seemed enveloped in a sheet of mellow flame.

17. *Observations at Louisville, Ky.* (lat. 38° 3′), *from the Louisville Journal.*

One of the most magnificent auroras ever witnessed in this latitude was seen about 9 o'clock, Aug. 28th. The whole heavens, from the northern horizon to the zenith, were brilliantly illuminated with a rose-colored light, and the flashes were very vivid. The northwestern sky was the portion most brilliantly illuminated, but in the northeast, the rosy flush was exquisitely beautiful.

18. *Observations at Charleston, S. C.* (lat. 32° 46′), *from the Charleston Mercury.*

Aug. 28. The northern heavens were brilliantly lighted until about 2 o'clock Monday morning, when the aurora faded entirely away. On the morning of Sept. 2d the auroral exhibition far surpassed any former instance observed in this city, for the general extent and diffusion of the lights.

19. *Observations at Bermuda* (lat. 32° 34′), *from the Bermuda Royal Gazette.*

The aurora of Aug. 28th appeared to ascend from a few degrees above the northern horizon a great distance upwards towards the zenith, assuming a variety of shades and beautiful

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colors; sometimes in tremulous sheets of pale yellow, changing gradually into a deep crimson, or shooting upwards in streams of light resembling those frequently observed from the setting sun. It covered at times the entire space between N.W. and N.E., leaving the sky from the horizon to its apparent base, perfectly clear. Towards 10 o'clock its brilliancy gradually died away, but it continued more or less visible till the dawn of day.

Sept. 2d, between 2 and 3 A. M., the aurora displayed itself in greater splendor than it did on the 28th. Many persons were awakened from their slumbers by the intense light which entered their chambers.

20. *Observations at Savannah, Ga.* (lat. 32° 5′), *from the Savannah Republican.*

On the evening of Aug. 28th we had a brilliant display of the aurora borealis. The northern sky, for an extent of some forty-five degrees, was luminous with a mass of red light, from whence shot up towards the zenith the usual streaks, at times vivid and beautiful.

Sept. 2d, about 1 A. M. the aurora again appeared and was of a very intense and beautiful color, being a mixture of pink, gold and purple. After it had reached an elevation of about 45°, it seemed to dissolve in the centre, and spread out both east and west. About 2 o'clock it formed a complete arch overhead, from N.E. to S.W. About 3 A. M. it gathered in the zenith, and sent out bright fiery flashes in every direction. It was far more magnificent than the aurora of Aug. 28th.

21. *Observations at Mobile, Ala.* (lat. 30° 41′), *from the Mobile Daily Register.*

The aurora showed itself a little east of north about 7½ P. M. Aug. 28th, and kept up the exhibition until about 9½ P. M., when its paling light died out. It was of a reddish hue, inclining to yellow, and its flickering light assumed a kind of pyramidal form, shooting up into the heavens, nearly to the zenith. Then the centre seemed to grow dim, and a division took place, its right wing moving to the extreme north, where the left wing after a short time joined it.

Between 12 and 3 o'clock on the morning of Sept. 2d, the aurora was repeated upon a scale of beauty and grandeur never before witnessed in the south. A bright pink colored light shot up from the northern horizon, and darted off into beautiful rays, flickering and brightening until they reached the zenith, and soon encircled the hemisphere like a belt from east to west. After about three-quarters of an hour, during which time the aurora occasionally furnished light enough to read by, the bright and beautiful light suddenly clothed the entire firmament.

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22. *Observations at New Orleans, La.* (lat. 29° 57′), *from the New Orleans Daily Delta.*

A grand auroral display appeared between 8 and 9 o'clock, Aug. 28th, in the northern horizon. A column of light first shot up into the sky, which soon spread up towards the zenith, and around the horizon, and made one of the most magnificent appearances that the sky has ever exhibited.

About 11 P. M., Sept. 1st, the aurora reappeared and continued until 3 or 4 o'clock in the morning. Nearly the whole visible northern hemisphere was covered with a fiery, blood-reddish, though transparent vapor. The deepest color was on the east and west, a space around the polar centre seeming to be the only non-illumined portion of the northern heavens. Over this crimson ground, spears and pencils of pale flickering light shot up at intervals from the horizon, converging at a point near the zenith. The whole sky along those lines was at once luminous and tremulous. In a moment vast segments of arches would rise, and then suddenly disappear.

23. *Observations at Galveston, Texas* (lat. 29° 17′), *by* Prof. C. G. FORSHEY.

Aug. 28th, as early as twilight closed, the northern sky was slightly lurid, and at times lighter than other portions of the heavens. At $7^h\ 30^m$ a few streamers showed themselves. Soon the whole sky, from Ursa Major to the zodiac in the east, was occupied by the streams or spiral columns that rose from the horizon. Spread over the same extent, was an exquisite roseate tint which faded and returned. Stately columns of light reaching up about 45° from the horizon, moved westward about one degree for every ninety seconds of time. There were frequent flashes of lightning, apparently from distant clouds, along the whole extent of the aurora; but no clouds were visible, except a single streak near the horizon. At 9 P. M. the whole of the streaking had faded, leaving only a sort of twilight over the northern sky, and we ceased our observations.

At 3 A. M. Aug. 29th, I awoke and perceiving that it was very light outside, rose, and found the whole northern heavens again on fire. Such a display I have never seen equalled since the aurora of Sept. 1, 1839. The whole distance before-named was tinted with the roseate hue; darker, nearly crimsoned at the two flanks. In the centre, near the meridian, stood a stupendous pyramid of white light with its apex near the zenith. On either side at some twenty degrees, stood a pyramid of rosy light, each about sixty degrees in height and in exactly symmetrical positions. Scarcely had I sketched the outline of this noble spectacle when the columns drifted westward and faded.

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Another fine display of the aurora commenced about 10½ P. M. Sept. 1st, and lasted until near daylight the next morning. A dusky red, like the reflection of an immense conflagration, overspread almost the entire heavens, beyond the zenith, far down towards the southern horizon.

24. *Observations at Sea* (lat. 28° 30′, long. 79° 30′), *Barque Pride of the Sea.*

Sept. 2d, at 12^h 35^m A. M., a bright spot or cloud appeared in the N.W. which shot up rays resembling the aurora, and in thirty or forty minutes formed an arch across the horizon from N.W. to N.E., which became lighter as it arose, and at 1^h 15^m A. M. it was light enough to read the smallest print without a light. At the time the horizon was cloudy, but overhead was clear, the larger stars being just seen. At 2^h 15^m the arch passed over to the southward, when it became dark again.

25. *Observations at Key West* (lat. 24° 32′), *from a Journal.*

A brilliant exhibition of the aurora was witnessed at this place Aug. 28th, and a still more brilliant one on the morning of Sept. 2d. The whole northern half of the heavens was tinged with crimson, red as blood. Occasional flashes of blue and white light shot up towards the zenith and then slowly melted away.

26. *Observations at Havanna, Cuba* (lat. 23° 9′), *by* M. ANDREAS POEY.

In his former communication (Am. Jour., vol. xxviii, p. 406) Mr. Poey stated that during the auroras of Aug. 28th and Sept. 1st he was unable to obtain any indications of atmospheric electricity. In a later communication he states that neither at the time of these auroras, nor on the preceding or following days, was there the smallest interruption or disturbance experienced on the electro-magnetic telegraph lines of Cuba.

27. *Observations at Inagua, Bahama Islands* (lat. 21° 18′), *from the New York Journal of Commerce.*

The aurora of Aug. 28th was distinctly seen from this place, and was supposed to have been a large fire in the neighborhood. It was remarkably brilliant, but was not attended by that flashing appearance which is sometimes noticed in higher latitudes.

28. *Observations at Cohe, Cuba* (lat. 20°), *by* GEORGE F. ALLEN.

On the night of Sept. 1st a Spanish mechanic who worked for me called me out of bed to see the great light in the northern sky. He was much struck with it, and said the people in St. Jago de Cuba would think the end of the world was at hand.

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I found a display which would have been considered more than ordinary even in the latitude of New York. It resembled the auroral displays occasionally seen in New York when more than usually brilliant. The same rosy light, on a darker horizon, fading off into yellower and whiter as it spread upwards, variegated occasionally with white streamers. It extended horizontally, according to my rough estimate, about one-third or two-fifths of the horizon, and upwards about two-fifths of the arch of the visible heavens. It was a very brilliant display, and surprised me much by its brilliancy in that latitude.

29. *Observations at Kingston, Jamaica* (lat. 17° 58′), *from the New York Herald.*

An extraordinary light appeared in the north on the night of Sept. 1st and the morning of Sept. 2d. It appeared as if there was a colossal fire on earth which reflected its flames on the heavens. The whole island was illuminated. The light was seen at Montego Bay (lat. 18° 21′) at 10 P. M., but it was not observed at Kingston until 1 A. M. Sept. 2. It continued until 5 A. M., when it gradually disappeared. It looked as if Cuba was on fire, and many believe that a portion of this island had been destroyed by a conflagration. Other persons were of opinion that the light was that of an aurora, but the aurora has never before been seen in this latitude. A similar fire was observed on the north side of Jamaica Aug. 28th.

30. *Observations at Guadeloupe, West Indies* (lat. 16° 12′), *from L'Institut.*

On the 2d of September, from $1\frac{1}{2}^{h}$ till daylight, an Aurora Borealis was seen at Guadeloupe to the great astonishment of the population. Its ruddy light was noticeable in the interior of the houses. At the centre of this vast conflagration were noticed two rays of whitish light which rose parallel to each other, passing a little to the left of the pole star. The aurora attained its maximum of brightness at 3 A. M.

31. *Observations at La Union, San Salvador* (lat. 13° 18′), *from the Gaceta del Estado.*

On the night of Sept. 2d, a most extraordinary phenomenon was witnessed. About 10 o'clock, a red light illuminated all the space from north to west, to an elevation of about 30° above the horizon. The light was equal to that of day-break, but was not sufficient to eclipse the light of the stars. The sea reflected the color, and appeared as if of blood. This lasted until three in the morning, when a dense black cloud arose in the east, and commenced to spread over the colored portion of the heavens,

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presenting a most curious spectacle; for in the parts where the cloud was not dense enough, the red light shone through, and formed a thousand fantastic figures, as if painted with fire on a black ground.

In the city of Salvador (lat. 13° 44′) the same phenomenon was visible, occupying the same space in the heavens, and the red light was so vivid that the roofs of the houses and the leaves of the trees appeared as if covered with blood.

February, 1860.

ART. XXV.—*Correspondence of Mr. Jerome Nicklès, dated Nancy, November 10th*, 1859.

Biography.—Cagniard-Latour.—We have already given some notice of this physicist, so lately lost to science; the following details are taken from an autobiography, which gives a very interesting account of the circumstances which led him to some of his discoveries. His researches may be classed under four heads; acoustics, mechanics, chemistry, and general physics having successively occupied his attention. His first invention (1809) was a pneumatic Archimidean screw, which is now in common use for conveying gases under liquids, and has received the name of the *Cagniardelle*. The ingenious inventor simply inverted the action of the ordinary screw of Archimedes, making it revolve from right to left. As Arago remarked in the chamber of deputies in 1844, during a discussion of the law of patents, although the *Cagniardelle* is nothing more than Archimedes' screw reversed, it is not less true that 2000 years had passed before any one conceived the idea of making this simple change and rendering it available in mechanics as a pneumatic machine.

The *Siren* (1819) as is well known, is an instrument for measuring the vibrations of the air which constitute sound. If, as had been supposed by physicists, the sounds produced by musical instruments are due to the regular succession of impulses given to the air by their vibrations, it was evident that a mechanism which would enable us to strike the air with the same rapidity and regularity should in like manner produce sounds. Reasoning in this manner, Latour was led to the invention of this well-known and beautiful instrument.

In 1822 he published his experiments on the combined action of heat and pressure upon certain liquids, such as water, alcohol, ether and naphtha. He imagined that the dilation of a volatile liquid must have a limit beyond which, notwithstanding the compression, it would pass to the state of vapor, provided that the capacity of the vessel permitted the liquid to expand beyond its maximum of dilation. The remarkable results to which he was led by this reasoning are well known.

In 1837 he published with Mr. Demonferrand the description of an acoustic pyrometer, by which the authors proposed to render the measurement of all temperatures appreciable through the medium of sound. In the same year he examined the pressure to which the air in the trachea is exposed during the act of sounding. He had previously been employed

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in investigating the pressure to which the air in the lungs is exposed when employed in sounding certain reed instruments, and had found it for the clarionette equal to the pressure of the atmosphere, *plus* a column of water of 30 centimeters. In order to extend these inquiries to the human larynx, it was necessary to find a person having an opening in the trachea, and yet able to produce vocal sounds at will. After long search, Cagniard-Latour found such a man, who was for his purpose as precious as the subject with the permanent gastric fistula became for the well-known experiments of Dr. Beaumont. In the same year he made known his chronometric balance, designed to measure the dynamic effects of machines in motion.

Next appeared a memoir on the alcoholic fermentation, of which these were the principal results:

1st. The yeast of beer is made up of little globular bodies, apparently vegetable, and capable of reproduction in two different manners. These bodies seem to act upon a solution of sugar only when in a state of life, and he hence conceived it probable that it is by a vital process that they transform the sugar into carbonic acid and alcohol. This investigation was suggested by a question long before proposed as the subject of a prize by the French Academy of Sciences, in the seventh year of the Republic, viz. What are the characters which distinguish among animal and vegetable matters, those which serve as ferments and those which are subject to fermentation?

Cagniard-Latour now resumed his researches upon vibrating bodies, and succeeded in producing a sound by causing a glass rod to oscillate easily between two metallic columns. The peculiarity in the sound thus obtained was that the number of vibrations indicated by it corresponded only to one-half the synchronous number of simple oscillations of the rod, although the apparatus was arranged in such a manner that each movement backwards and forwards should produce two strokes of equal intensity, by the alternate blows upon the two columns. The experiments made with this instrument enabled him to give the theory of the production of sounds by vibrating cords. During the same year (1840) he studied the production of grave tones, like those of the human voice, and made various researches to discover the mechanism of the human voice.

In 1851 he laid before the Academy a memoir upon the *moulinet à battemens*, demonstrating some new acoustic phenomena. In two previous papers published in 1830 and 1831, upon the sounds produced by solid bodies turning with great velocity, he had shown certain facts relative to the musical tones produced by the friction of the axle of a wheel against its supports. Subsequently he conceived the idea that a solid of revolution, a cylinder for example, arranged so as to turn vertically around its axis upon two center-holes, might give rise to pulsating sounds (beatings), although moving with a feeble velocity, provided it received in the lower center-hole the friction of the revolving axis of a winch turning in an opposite direction to the cylinder. It was to the instruments constructed according to these ideas that he gave the name of the *moulinet à battemens*.

We have also from Cagniard-Latour an investigation on the action of heat on different kinds of wood enclosed in hermetically sealed glass

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tubes. Sir James Hall, in his experiments upon the saw-dust of pine wood and of horn in sealed gun-barrels, had observed that the mixture underwent fusion and was cemented into a sort of coal. Similar results were obtained by Latour with thick glass tubes.

These are not the only researches which we owe to this lamented physicist; in our previous letter we mentioned that while connected with the Government powder works in 1814, he made some useful improvements in that department, especially in the glazing of powder. We also spoke of his very light, portable, and efficient flour-mill, which consisted of a steel rasp moving vertically with an alternating motion between two fixed rasps, also of steel. During the severe winter of 1816, the streams being all frozen and the mills stopped, Cagniard-Latour was directed to have an immense number of these hand-mills constructed, and thus in a few days the public were saved from the fears of a famine. We have also seen the part which he took in the establishment of gas-lighting in Paris. He was besides the constructer of an aqueduct, a model of its kind, suspended between two rocks, and formed of a single span 200 metres in length. When we consider these varied achievements, we learn with surprise that it was only in 1851 that he became a member of the Academy of Sciences.

The aurora borealis and its theory.—The late brilliant auroras have called attention to De la Rive's theory, of which we have formerly spoken, and which is explained at length in his *Traite d'Electricité.* Great perturbations were observed along the telegraphic lines over the European continent, similar to those remarked some years since by Matteucci in Tuscany, and Highton in England. The most remarkable fact in these electrical disturbances is that they were produced by a continuous current, while those of a thunder storm are instantaneous, and only mark points upon the paper in Morse's apparatus; the aurora of the 29th August traced continuous lines of greater or less length. These effects lasted for several days after the aurora.*

It is fortunate that the aurora of the 29th was carefully studied by a man so competent as Coulvier-Gravier. This observer, who has studied the heavens for nearly 60 years, and has so much advanced the sciences of Cosmography and Meteorology, was found that night as usual at his post in the observatory which the government prepared for him twenty years since at the Luxembourg palace. The phenomenon was in all its splendor at $2^h\ 45^m$ A. M.; its extent included more than 100°, and M. Coulvier-Gravier declares that he had never seen it more beautiful during the long period of his observations.

The observations during the late auroras support the theory of De la Rive, which he has thus defined. The vapors constantly rising from the sea, and especially from the equatorial regions, carry with them to the higher regions of the air a great quantity of positive electricity, to which they serve as the vehicle, leaving the surface of the globe negatively electric. Borne to the poles by the currents which always prevail in the higher regions of the atmosphere, these vapors carry with them their electricity, and thus give to the whole atmosphere a positive electric condition, which diminishes from above downwards. This positive electricity tends unceasingly to combine with the negative electricity of the earth,

* See p. 92, this volume.

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directly through the stratum of air, but more especially at the two poles, where the currents of vapor carried by winds converge and are condensed.

According to Mr. De la Rive the aurora of the 29th August was a natural consequence of the great drought which had prevailed over the continent. The dryness of the air had prevented the positive electricity from neutralizing itself directly with the negative of the earth. From this accumulation of electricity there finally took place towards the polar regions a discharge much more intense and much more rapid than usual, which constituted the brilliant aurora in question. A fact which tends to show that the aurora is an electrical and not a magnetical phenomenon, is furnished by the ozonometrical observations made by Mr. Perigny, at Versailles. From the 28th of August to the 2d of September, he found the air to contain a quantity of ozone, relatively large, and more abundant by night than by day.

Human Remains in the Drift.—For the last twenty years it has been known that axes of flint, evidently wrought by human skill, were found in beds of the drift at Amiens, associated with the bones of extinct species of animals. This discovery, made by a learned antiquary, Mr. Boucher de Perther, had been regarded as doubtful, and it was supposed that sufficient precaution had not been observed in conducting the explorations. Recent discoveries made in a cavern at Brixham, near Torquay, in England, however, recalled attention to the observations of Mr. Perther, and Mr. Prestwich, with several other geologists, accordingly visited Amiens in order to make the excavations necessary to decide this important question. Every precaution was of course taken to prevent errors or deception, and Mr. Prestwich could find nothing at Abbeville, but at Amiens he was more fortunate; one of his companions, Mr. Flower, in examining a bed of gravel at six meters from the surface, and evidently undisturbed, extracted with his own hands a fine axe, more than five inches long. After this Mr. Prestwich having been informed that a similar discovery had been made in 1737, at Haxne, in Suffolk, visited the place, and learned that some years since wrought flints were still found in abundance, although rare at present. He however succeeded in finding two axes, similar to those of Amiens, but of less perfect finish. An analogous fact has just been verified by French geologists, who have found these axes in Picardy, associated with remains of *Elephas primogenius*, *Rhinoceros tichorhinus*, *Equus fossilus*, and an extinct species of *Bos*. The conclusion from all these facts is that man was cotemporaneous with these several species of large animals now lost, and known to us only by their fossil remains.

Curare in the treatment of Tetanus.—This important question has elicited much discussion at the Academy, and many contradictions and differences of opinion. In 1850, Mr. Claude Bernard showed that the curare, or arrow poison, acts by paralyzing the system of motor nerves; following up this observation, an Italian physician, Villa, of Turin, made in 1854 a series of experiments, in the course of which he showed that curare exerts upon the nervous system an action so completely antagonistic to that of strychnine, that the two poisons may be neutralized by each other. Dr. Villa having been attached to the French hospitals during the late war, was induced to apply the curare in the treatment of three cases of trau-

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matic tetanus one of which recovered. It was the case of a soldier wounded by a ball in the right foot. The curare dissolved in water was applied to the wound, with the effect of diminishing the pain and suspending temporarily the tetanic spasms, which, however, returned. After fifteen days of this treatment the patient left the hospital completely cured. The experiments of Mr. Villa have been repeated in the hospitals of Paris; but as yet only a single case of cure has been reported out of several failures; the experiments, however, continue. According to a letter from Sir Benj. Brodie, of London, to the Academy of Sciences, the application of curare as a specific against tetanus, was unsuccessfully made upon horses at London in 1815, by Dr. Sewell, professor at the Veterinary College. Such are the principal facts in the question as far as yet made known.*

The new alloys of Platinum.—We recall the interesting researches of Messrs. Deville and Debray on this subject only to mention their industrial applications. Hitherto it had been supposed that the presence of iridium impaired the quality of platinum, but the labors of Deville and Debray have shown that on the contrary alloys of these two metals may be prepared which are greatly superior to pure platinum, presenting greater strength and rigidity, and resisting better both heat and acids. Thus the alloy contaiuing 21·3 of iridium is highly malleable and scarcely attacked by aqua regia. As the quantity of iridium is less, the alloy becomes softer, and one containing 10 or 15 per cent. is peculiarly fitted for chemical vessels. These alloys are now largely wrought in Paris; retorts for the manufacturers of sulphuric acid have been made, having the strength and rigidity of rolled iron.

Messrs. Deville and Debray are at present making some trials at the French mint, for the Russian government, to determine the fitness of the new alloys for coinage. They have found that those containing 20, 10, 7½ and 4 per cent of iridium, take the impression of the dies with great perfection. The same is true of the natural alloy, which is obtained by directly fusing crude platinum, and retains only the iridium and rhodium in combination with the platinum, the other metals having been removed by volatilization or oxydation. The platinum workers of Paris are now manufacturing and selling the new alloys, and, contrary to the wishes of the discoverers, are exacting higher prices than for pure platinum.

Rifled Cannon.—The invention of these guns appears to be due to a former captain of artillery, Mr. Tamisier, who was in 1842 charged with the course of instruction in musketry at Vincennes, where he applied himself with great assiduity to the study of various questions connected with his profession, with results which have contributed very much to the improvement of the system of musketry instruction in the army and in the arms of precision. After studying the effects of elongated projectiles in rifles and muskets, Mr. Tamisier was led to construct a rifled mortar, with cylindro-conical shells. The duke of Montpensier, then colonel of artillery, at once saw the importance of this new project, and after causing many experiments to be made, ordered at his own expense, in 1847, the construction by Capt. Tamisier of elongated balls and shells. These however were not tried until 1850, from which time up to 1853, Mr.

* See experiments on two new varieties of South American arrow poison by Drs. Hammond and Mitchell, Am. Jour. Med. Sci., July, 1859, (this Jour. [2], xxviii, 303).

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Tamisier continued his experiments at Vincennes and at La Fère. The first experiments were made at Vincennes, on the 15th of July, 1850, when it was shown that rifled six-pounders with elongated projectiles, carried much farther and with greater exactness than ordinary guns of smooth bore. On the 14th August, 1851, Capt. Tamisier repeated his experiments at the polygon at Vincennes, before the President and the Minister of War. A six-pounder rifled with three grooves projected balls of five kilogrammes to a distance of 1500 meters with a charge of only 700 grammes of powder. The government then ordered further experiments to be made, which were conducted at La Fère, a fortified place in the Department of the Aisne, where greater secrecy could be secured than near Paris. The trials were there made by Col. Trenille and by Col. Virlet, now directors of the School of Artillery at Metz, and led to a complete solution of the problem, so that the army of Italy was able to bring into the field more than 200 rifled guns of a calibre of 84 millimeters, requiring for service and transport only two-thirds the men and horses hitherto necessary, and carrying balls of four kilogrammes 3500 meters with such precision that at this distance they would all fall in a rectangle of 80 meters in length by 40 meters in breadth.

Acclimatation.—In our last letter we spoke of a number of Arabian camels, which the Society of Acclimatation had procured in Algeria, to be introduced into Brazil. We learn that they were safely landed there after a voyage of 28 days.

Photo-Chemical Researches: Persistent Activity of Light.—In this Journal for March, 1859, p. 257, we have described the beautiful experiments of Niepce de St. Victor, upon the persistence of the effects of insolation. According to Mr. Laborde, the curious results obtained by Mr. Niepce are due not to radiation, but to a veritable emanation from the card-board, which has been impregnated with tartaric acid, and exposed to the sun-light. The principle subsequently evolved from this card-board, which acts upon sensitive paper prepared with a silver salt, is, according to Mr. Laborde, no other than formic acid. This is well known to be a product of the oxydation of tartaric acid, especially when in contact with peroxyd of lead. He found that a concentrated solution of tartaric acid mixed with peroxyd of lead, and placed in a dark place, evolved vapors which reddened litmus and discolored prepared paper after a very short time. There is, then, in the case of insolated tartaric acid, an emanation and not a radiation; but Mr. Niepce obtained similar results with nitrate of uranium spread upon porcelain and insolated, in which case it is not easy to conceive of an emanation of vapor. However this may be, the facts announced by Mr. Niepce remain, and the only difference of opinion is as to their interpretation.

Maritime Canals.—Besides the canals proposed at Panama and Suez, the question of one between the Caspian and the Black Sea is now being discussed in Russia. The construction of such a canal was ordered by Peter the Great, after having been commenced a century before under the Sultan Selim II, upon the suggestion of his Grand Vizier, Mohammed Sokolli, who was for Turkey what Colbert was for France. The canal was commenced in 1569, and during two years there were employed upon it 5000 janissaries and 20,000 prisoners.

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The canal now projected will unite the Don and the Volga, the first of these falling into the sea of Azof, and the second into the Caspian, near Astrachan, at a point designated on the maps as Tzaritsin. The two rivers are separated by a distance of only 55 kilometers. The importance of a canal which will connect the Caspian with the Mediterranean will readily be seen, and it must at the same time be confessed that it will be much more easily executed than the tunnel under the channel proposed by Mr. Thornè de Gamond, since the canal, being exclusively on Russian soil, will be a question for engineers, with which politicians will have no concern.

For the last thirty years, it has been proposed to make a sea-port of Paris by a ship-canal leading from the Channel; the introduction of railways caused the project to be for a time abandoned, but it is now again discussed. Among the plans proposed is one which appears in the *Ann. des Sciences* of the 8th October, 1859. It is intended to excavate at the mouth of the Seine, near Havre, a harbor 1000 meters long and 200 wide, with a depth of 12 meters. The canal, 150 kilometers in length, will follow the course of the Seine, and will have the same depth as the harbor, which will be furnished with gates to preserve the water at a proper height. A double line of railway will accompany the canal, one line serving for passengers and freight, and the other for towing vessels, which will thus make the voyage in four hours.

The position and direction of the canal will be such that the west winds, which are the most frequent at Paris, will help vessels in coming up, while the water of the Seine, to be let in by sluices, will aid them, by their current, in descending. The great difficulty in this enterprise will arise from the tunnels required, the total length of which will be from twenty to twenty-five kilometers. These will be vaulted, and with a height of 30 meters by at least 50 meters in breadth. Mr. Piorry estimates that the expense of this work may amount to a billion (1,000,000,000) of francs.

Engineers are also occupied with a plan for joining the English Channel with the Mediterranean, by taking advantage of the river Rhone, the Saône, the Yonne, and the Seine. The details of its execution having only a local interest, we spare our readers their recital.

SCIENTIFIC INTELLIGENCE.

I. CHEMISTRY AND PHYSICS.

1. *On two new series of Organic Acids.*—Hentz has studied the action of methylate of soda and similar substances upon chloracetic acid, and has obtained interesting new acids in which hydrogen may be considered as replaced by the deutoxyds of methyl, ethyl, etc. The acid resulting from the action of methylate of soda on chloracetic acid has the formula $C_6H_6O_6$, which is that of lactic and paralactic acids, but is not identical with either of these. To this acid the author gives the name of methoxacetic acid; it is monobasic, and gives beautifully crystallized salts. The acid itself is easily prepared by decomposing the zinc salt with sulphu-

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retted hydrogen, and distilling the liquid after separating the sulphid of zinc. The boiling point rises gradually till it becomes constant at 198° C., when the acid passes over as a colorless liquid, with a sour smell, resembling that of acetic acid.

When monochloracetate of soda is boiled continuously, an acid liquid passes over, which, when saturated with baryta, yields on evaporation a crystallizable salt having the formula of glycolate of baryta. The author proposes for the acid contained in this salt the name of oxacetic acid; it is isomeric, but not identical with the glycolic acid. Its formula is $C_4H_4O_6$.

Ethylate of soda acts violently on monochloracetic acid; the products of the reaction are chlorid of sodium, and the soda salt of a new organic acid, homologous with the two last described, and which the author terms ethoxacetic acid. This acid is volatile without decomposition and appears to boil at a lower temperature than the corresponding methyl compound. Its formula is $C_8H_8O_6$.

Amylate of soda acts in a similar manner on monochloracetic acid. The new acid formed in the reaction is an oily liquid which has the formula $C_{14}H_{14}O_6$.

Phenylate of soda, under similar circumstances, yields phenoxacetic acid, as an oily liquid which crystallizes at a low temperature, and distils over without decomposition. The analyses appeared, however, to show that the phenyl alcohol employed contained benzalcohol, and that the product examined was therefore a mixture of the homologous acids $C_{16}H_8O_6$ and $C_{18}H_{10}O_6$.

When the soda salts of organic acids are heated with monochloracetic acid, chlorid of sodium is formed, and new organic bodies which the author proposes to study. It is easy to see that all the acids belonging to the formic series will yield similar new acids with the peroxyds of the alcohol radicals homologous with hydrogen, and that in this manner a very great number of new compounds may be obtained.—*Journal für prakt. Chemie*, 78, *p.* 174.

Note.—According to Kolbe's view, lactic acid is to be regarded as propionic acid, in which one equivalent of hydrogen is replaced by one of HO_2, so that its rational formula is $\left.\begin{matrix} C_6H_4(HO_2)O_2 \\ H \end{matrix}\right\} O_2$. Upon this view the oxacetic acid of Heintz must be identical with glycolic acid, which appears not to be the case. The question may doubtless be decided directly by examining the products of the action of water upon chloropropionic acid, which ought thus to yield lactic acid directly, since we should have the reaction

$$C_6H_4ClO_5HO + HO_2H = C_6H_4(HO_2)O_5HO + HCl.$$

Heintz's acids may be more simply regarded as derived from the formic series by simple replacement of hydrogen by the peroxyds HO_2, $C_2H_3O_2$, $C_4H_5O_2$, &c. Dichloracetic and trichloracetic acids ought to yield analogous products in which two or three equivalents of hydrogen are replaced by two or three equivalents of peroxyds which need not be of the same radical. The number of possible acids would thus almost or quite equal that of the ammonias or ammoniums. W. G.

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2. *On the chemical constitution of Isethionic acid and Taurin.*—By the distillation of isethionate of potash with perchlorid of phosphorus, Kolbe has obtained a new acid which has the empirical formula $C_4H_5ClS_2O_5$, and which he terms chlorethyl-sulphuric acid. This acid yields taurin by the substitution of NH_2 for Cl, and ethyl-sulphuric acid $C_4H_6S_2O_6$ when the chlorine is simply replaced by hydrogen. Kolbe draws a parallel between the derivatives of carbonic and sulphuric acids, which is best illustrated by the following tabular view:

$C_2O_2 . O_2$	$S_2O_4 . O_2$
Carbonic acid.	Sulphuric acid.
$HO . (C_4H_5)(C_2O_2) . O$	$HO . (C_4H_5)(S_2O_4), O$
Propionic acid.	Ethyl-sulphuric acid.
$HO . (C_4H_4Cl)(C_2O_2), O$	$HO . (C_4H_4Cl)(S_2O_4), O$
Chloropropionic acid.	Chlorethyl-sulphuric acid.
$(C_4H_4Cl)(C_2O_2), Cl$	$(C_4H_4Cl)(S_2O_4), Cl$
Chlorethyl-carbon-chlorid.	Chlorethyl-sulfochlorid.
$HO . (C_4H_4NH_2)(C_2O_2), O$	$HO . C_4H_4NH_2)(S_2O_4), O$
Amidoethyl-carbonic acid. (Alanin)	Amido-ethyl-sulphuric acid. (Taurin).
$HO . (C_4H_4(HO_2)),(C_2O_2),O$	$HO . (C_4H_4(HO_2))S_2O_4, O$
Oxethyl-carbonic acid Lactic acid.	Oxethyl-sulphuric acid. Isethionic acid.

The author promises a more detailed account of the compounds and reactions referred to in the above brief preliminary notice.—*Ann. der Chemie und Pharm.*, cxii, 241.

3. *Researches on the atomic weight of Graphite.*—Brodie has communicated an exceedingly interesting and suggestive memoir on the atomic weight of graphite, considered as an allotropic form of carbon, the fundamental idea being that the different modifications of the same substance may exhibit a difference in equivalents, as well as in their ordinary chemical and physical properties. The author finds that graphite, when heated with nitric acid and chlorate of potash, increases in weight, and ultimately yields a light yellow crystalline substance. The details of the process are as follows: a portion of graphite is intimately mixed with three times its weight of chlorate of potash, and the mixture placed in a retort. A sufficient quantity of the strongest fuming nitric acid is added to render the whole fluid. The retort is placed in a water-bath, and kept for three or four days at a temperature of 60° C. until yellow vapors cease to be evolved. The substance is then thrown into a large quantity of water and washed by decantation nearly free from acid and salts. It is then dried in a water-bath, and the oxydizing operation repeated with the same proportion of nitric acid and chlorate of potash, until no farther change is observed. This is usually after the fourth time of oxydation. The substance is then to be dried, first in vacuo, and then at 100°. By placing the mixture in a flask exposed to sunlight, the change takes place more rapidly and without the application of heat.

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The formula of the body thus obtained is $C_{22}H_4O_{10}$, or, as the author writes it, $C_{11}H_4O_5$.

Its crystals belong either to the right or oblique prismatic system. It is insoluble in water, containing acids or salts, and very slightly soluble in pure water. It unites with alkalies, and the crystals have an acid reaction: ammonia converts it into a transparent jelly, but the substance is not dissolved. Acids separate it from this combination, as a gelatinous mass resembling silica. Treated with deoxydizing agents, it is readily decomposed. When a solution of sulphate of ammonium or of potassium is poured upon the dry substance, a crackling sound is heard, and a body is formed resembling graphite.

The crystals are decomposed with ignition on the application of heat, gases being evolved, and a black residue left, which resembles finely divided carbon. This substance the author proposes to term graphic acid.

When graphic acid is heated in naphtha to about 270°, water and carbonic acid are given off, while the naphtha takes a deep red color. The residual substance resembles graphite and has the formula $C_{44}H_2O_5$, or with the author's equivalents, $C_{22}H_2O_4$. When this substance is heated in a current of nitrogen to a temperature of 250°, water is given off with a little carbonic acid; the substance remaining is found to have the formula $C_{132}H_4O_{22}$ or $C_{66}H_4O_{11}$. This body may be exposed for several hours to a red heat in a current of nitrogen without losing all its oxygen and hydrogen.

The author compares graphic acid with a remarkable compound of silicon discovered by Buff and Wöhler, which has the formula $Si_4H_4O_5$, and which was obtained from the graphitoid form of that element. The properties of the two substances agree very closely, whence it may be inferred that the graphite compound is the same term in the system of carbon as the silicon compound in the system of silicon. The total weight of graphite which in the compound is combined with atoms of hydrogen and of oxygen is 132. If we assume that this weight is like the corresponding weight, 84 of silicon, to be divided into four parts, we arrive at the number 33 as the atomic weight of graphite. Representing this weight by the letters Gr, the formulas of the substances $C_{11}H_4O_5$, $C_{22}H_2O_4$, and $C_{66}H_4O_{11}$ become $Gr_4H_4O_5$, $Gr_8H_2O_4$ and $Gr_{24}H_4O_{11}$, where $O=16$.

According to the law of Dulong and Petit, the specific heats of the elements are inversely as their equivalents. The elements are divided into two classes, one in which the product of the specific heat into the equivalent is about 3·3—the other in which this product is 6·6. The specific heat of carbon in the form of graphite—0·20187—presents a remarkable exception to the law, if we take its equivalent as 6 or 12, but if we assume the atomic weight of graphite as 33, we have for the product of the specific heat into the atomic weight, the number 6·6 which is according to the law of Dulong and Petit.

The relation which exists between the atomic weights of boron, silicon and zircon, and that form of carbon for which a place may be claimed as a distinct element, *graphon*, is precisely the kind of numerical relation which is found to exist between the weights of analogous elements. We have

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Boron	11
Silicon	21
Graphon	33
Zircon	66

These considerations lead to the inference that graphite functions as a distinct element, forming distinct combinations with a distinct equivalent, viz: 33. How far this inference may be extended to the allotropic forms of other elements, experiment alone can decide.—*Quart. Journal of Chem. Soc.*, vol. xii, p. 261.

[*Note.*—With respect to the numerical relations between the eqs. of boron, silicon, graphon and zirconium which Brodie points out, it may be remarked that boron—at least with the equivalent 11—is triatomic, as shown by the density of the vapor of BCl_3 and other considerations. It cannot, therefore, with this equivalent, belong to the same natural group with silicon and zircon, which are diatomic, as shown by recent investigations. Marignac has established the isomorphism of SnF_2+KF with SiF_2+RF, while Troost and Deville have shown from the vapor-density of chlorid of zirconium that its true formula is ZCl_2=2 vols. or Z_2Cl_4 if we assume that all compounds correspond to 4 vols. in a gaseous state. The vapor density of $SiCl_2$ also agrees with the supposition that silicon is diatomic, supposing it to represent 2 vols. The true equivalents of silicon and zirconium become therefore respectively 14 and 44 or 28 and 88, if we admit the 4-volume theory. The equivalents of carbon, silicon and zirconium are then to each other as 6, 14 and 44, or as 12, 28 and 88, the common difference being 8 or 16 nearly. The formula $Si_4H_4O_{10}$ was deduced by Buff and Wöhler upon the supposition that the equivalent of silicon is 21, the element being triatomic as assumed by Berzelius. But if we take 14 as the true equivalent, the formula for the same compound becomes $Si_6H_4O_{10}$, and comparing with this the formula $C_{22}H_4O_{10}$ we have 132 parts by weight of carbon, representing 6 eqs. of graphon instead of 4, as assumed by Brodie. This gives 22 as the equivalent of graphon, instead of 33. If now we multiply the spec. heat of graphite as found by Regnault, namely 0·201, by 22 we have 4·4 so that the spec. heat of an atom of graphon does not obey the laws of Dulong and Petit, as the product should be either 3·3 or 6·6. It may, however, be remarked that the spec. heat of graphitoid silicon has not yet been determined, and that there may be other classes of elements whose atoms have the intermediate spec. heats 4·4 and 5·5. The formulas of Brodie's compounds become, if we take the equivalent of graphon as 22—$Gr_6H_4O_{10}$, $Gr_{12}H_2O_8$, $Gr_{18}H_4O_{22}$, (taking O=8 and not with Brodie as 16). No probable relation can be pointed out between the numerical values of the equivalents of graphon and of other elements, until we know to what natural group graphon belongs, since it is not certain or even very probable that the allotropic modifications of the same element belong to the same group.—W. G.] W. G.

4. *On the Cause of Color and the Theory of Light;* by Mr. JOHN SMITH, M.A. (Read by his brother, Dr. R. A. Smith).—The author, in attempting to explain certain natural phenomena, could not satisfy himself by applying the principles of either theory of light, and said that many natural phenomena indicated beats or vibrations in the luminous ether very

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different from what science taught. That is, that there were greater intervals between them than Newton had demonstrated and scientific men believed. He therefore endeavored to contrive experiments by which he would be able to make as many revolutions or beats in a second as he considered the effective vibrations of light were repeated in a second of time, and argued that by certain contrivances to produce light and shade in alternate vibrations he should produce color. A series of experiments was subsequently undertaken, which led to the conclusion that varieties of color are produced by pulsations of light and intervals of shadow in definite proportions for each shade of color. That is, supposing white light to consist of the motion of an ether, blackness to consist of an entire absence of motion, then a certain color, blue, red, or yellow, will be produced by the alternate action of the light and the shadow. The author used shadow in the positive sense as the sensation was positive.

On pursuing the inquiry, he first caused a small parallelogram cut in card board to revolve over a black surface with a rapidity which he considered equal to the vibration of light. By this motion he obtained a distinct blue, while at another time in different weather he obtained a purple. He then made a disc with several concentric rings, which he painted respectively $\frac{1}{8}$, $\frac{2}{3}$, $\frac{3}{4}$, and $\frac{1}{2}$ black, leaving the remainder white, and on making this disc revolve the rings became completely colored. There was no appearance of any black or white. In a bright day with white clouds in the sky, the rings were colored respectively a light yellowish green, two different shades of purple, and a pink. By using discs of a great variety of shapes and different proportions of white and black, the author said that he produced successively or together all the colors of the rainbow, although he had not yet arrived at the exact arithmetical determination of the amount of light and shade needful for each color.

These experiments were made before the Society by the light of a paraffin oil lamp with a reflector. The author said that they were much more brilliant by sunlight.

There was another set of experiments which the author considered as very effective, and especially as being easily made and described, but requiring strong sunshine to show them. These were made by casting a shadow of a particular figure on a white wall or on a sheet of paper, so as to produce alternate beats of light and shadow when put in revolution. The figure became colored of different shades, and because these could be seen on the wall, like the spectrum from the prism, he called them spectra by reflection.

He mentioned also that the colors may be produced by making a black disc, with figures cut out of it, revolve before a white cloud or white screen.

There were many others which he had no time to enumerate, much less to describe, but he described some of the figures which produce the phenomena which are perceived when looking through transparent solids.

The author considered that his theory gave an entirely new and simple explanation of the phenomena of refraction through the prism, and summed up as follows:—

The experiments prove the homogeneity of the ether.

They prove the undulatory hypothesis, but oppose the undulatory theory.

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They enable us to dispense with the different refrangibilities of the rays of light, as taught by Newton.

They help to explain many of the phenomena of what is called the polarization of light.

They give a new explanation of prismatic refraction, and explain in a plain and simple manner many very interesting natural phenomena.

Startling, he said, as these conclusions are to those who are conversant with the subject of light, he thought he was perfectly warranted in drawing them from his experiments. The general process of reasoning could not, however, be given in a short abstract.—*Ordinary Meeting, Oct. 4th, 1859, Manchester Literary and Phil. Society.*

TECHNICAL CHEMISTRY.

1. *Vegetable Parchment.—Papyrine.*—The interesting substance obtained in 1846 by Poumarède and L. Figuier (Comptes Rendus, xxiii, 918; see also this Journal xxviii, 431,) by immersing bibulous paper in partially diluted sulphuric acid—called *papyrine** by its discoverers—which with the exception of a few comparatively unimportant applications in France, where it was used for the shelves on which silk-worms are reared, &c., had excited scarcely any interest other than that naturally attaching to it as a chemical curiosity, until patented (Dec. 6, 1853) in England, by Gaine, (see *Rep. of Pat. Inv.* [E. S.] xxiv, 151) and manufactured by the well known house of De LaRue & Co., of London, has recently been investigated by Prof. A. W. Hofmann, (Ann. Ch. u. Pharm., Nov. 1859, cxii, 243; from a report to Messrs. Thos. De LaRue & Co.) In its prominent properties it resembles ordinary parchment very closely: indeed the two can hardly be distinguished from each other except on close inspection. Both exhibit the same peculiar pale, yellowish tint, the same degree of translucency, the same half fibrous, horn-like texture. Like animal parchment, the artificial product is not easily torn: it may be repeatedly bent or folded without exhibiting any special appearance of breaking in the creases formed. Like ordinary parchment it is extremely hygroscopic, and becomes more pliable by absorbing moisture. When wet with water it comports itself like untanned skins, swelling up to a slippery mass through which water cannot pass except by endosmose: the coherence of the substance is not at all impaired by thus soaking.

Vegetable parchment is best prepared by immersing unsized paper during a few seconds in oil-of-vitriol which has been diluted with half its volume of water, and immediately afterwards washing it in a dilute solution of ammonia; a thorough washing with pure water completing the process. Hofmann has ascertained by direct experiment that not less than one-fourth volume, or more than one-half volume, of water must be used with one volume of monohydrated sulphuric acid, in preparing the acid bath. The paper must not be immersed too long, nor should the temperature of the bath be higher than about 15° (C.)=[59° F.] A considerable amount of practice is moreover requisite before one can obtain a perfectly satisfactory product. When paper is transformed into vegetable parchment it undergoes no appreciable increase in weight. The action

* Should not this term, which has an undoubted right of priority, be preserved as the scientific name of the substance?—[F. H. S.]

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of the sulphuric acid is purely molecular, the ultimate chemical composition of the paper—*cellulose*—remaining unchanged. [As already stated by Poumarède and Figuier *loc. cit.* and by J. Barlow, Proc. of the Royal Inst. 1857, ii. 411]. The result of the momentary action of sulphuric acid in this instance is comparable with that which a longer action of this acid upon woody fibre produces, viz.: formation of dextrine, a substance well known to be isomeric with cellulose. Indeed, the vegetable parchment may be regarded as a middle term between dextrine and cellulose.

The samples of parchment-paper examined by Hofmann [and by Barlow] contained no trace of free sulphuric acid; small portions of sulphate of lime and of sulphate of ammonia being the only soluble impurities present.

There is no apparent reason why the parchment-paper should not endure for an indefinite length of time. It is evident that if its destruction were dependent in any way upon the chemicals used in preparing it, decomposition would set in at once. Nothing of the kind occurs, however. Specimens of the factitious parchment which have been in Hofmann's possession during four years being undistinguishable from those recently prepared.

From experiments made in order to ascertain the strength of parchment-paper, as compared with that of true parchment and of unsized paper, it appeared that while strips of unsized paper broke when subjected to a weight of 15 or 16 pounds, similar strips of vegetable parchment supported 74 lbs., and those of ordinary parchment 75 lbs., before breaking. The cohesive force of unsized paper is thus increased five-fold by the treatment with sulphuric acid. It was also proved by experiment that for equal *weights* of the two substances parchment-paper exhibited about three-fourths the cohesive power of animal parchment. It also appeared that while the strength of strips of parchment-paper taken from different sheets was nearly constant, that of strips of animal parchment, even when cut from a single piece, was extremely variable, owing to the differences in thickness to which it is liable.

Parchment-paper although not quite so strong as ordinary parchment, is nevertheless more capable than the latter of withstanding the action of chemical agents, and especially of resisting the action of water; it may be left in this liquid for days, or even boiled in it, without undergoing any change, other than the increase of volume already alluded to, its original cohesion, and indeed all its properties being regained on drying. As is well known, animal parchment is soon converted into glue when boiled with water.

Since the parchment-paper contains no nitrogen, it is much less liable than ordinary parchment to putrefy when exposed to moisture, and will probably be less subject to the attacks of insects. Not only may the new parchment be substituted for that ordinarily employed for legal documents, &c.; but from its cheapness it will probably soon be used for ledgers and other important records—possibly for bank-notes—instead of the more perishable paper now employed. ["It will take the place of ordinary paper in school books, and other books exposed to constant wear." "It also promises to be of value for photographic purposes, and for artistic uses, in consequence of the manner in which it bears both oil and water-

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color."—*Barlow.*] Its strength and power of resisting the action of moisture seem also specially to adapt it for the use of architects and engineers —particularly for working-plans liable to receive rough usage; also for the envelopes of letters and for cartridges. In thin leaves it affords an admirable tracing paper. As a material for binding books it will without doubt be extensively used. The ease with which it receives both printers' and ordinary writing ink is remarkable. For chemical laboratories it affords a most convenient material for fitting together retorts, condensers, and the like; while its power of resisting the fluids used in galvanic batteries suggests that it may be useful for diaphragms, &c. It is already used by tons, instead of bladder, as a covering for jars containing preserves, marmelades, etc.

Parchment-paper has been successfully manufactured on the great scale for a year or more by the firm of De LaRue, the numerous difficulties which presented themselves having been fully overcome by the perseverance of one of its members—the distinguished chemist Warren De La-Rue. [Specimen sheets of the parchment-paper accompany Hofmann's memoir.—F. H. S.].

2. *Weighing of Moist Precipitates;* by FERDINAND F. MAYER.—Mr. Ch. Mène, of Creusot,* gives a mode of weighing which does away to a great extent with the tediousness and difficulties attending the drying of many precipitates, especially in volumetric analysis. He washes the precipitate thoroughly by decantation, and then introduces it carefully into a bottle, the exact weight of which, when filled with distilled water at a certain temperature, is known. Since the precipitate is heavier than water, the bottle when filled again will weigh more than without the precipitate, and the difference between the two weights furnishes the means of calculating the weight of the precipitate.

In case the precipitate settles but slowly it may be collected on a filter, and together with the filter, after washing, be introduced into the bottle, in which case the weight of the filter and its specific gravity, supposing any difference should exist between its own and that of water, is to be taken in account. Precipitates soluble in or affected by water may be weighed in some other liquid.

This method, of which the above are the outlines, is spoken of in the Jahresbericht der Chemie for 1858† in rather disparaging terms, and I consider it not more than justice to the method, if not also to Mr. Mène, to prove its correctness, the more so as I have applied the principle on a large scale as far back as 1855.

I engaged in that year in the manufacture of carbonate of lead from refuse sulphate of lead, by treating the latter in a pulpy condition with carbonate of soda. The sulphate of lead I used contained very varying proportions of water and soluble impurities, from which latter it had first to be freed by washing. It was then in the state of a thin pulp, and the difficulty was to find the amount of dry sulphate of lead, as it was a matter of importance to use as little carbonate of soda, and to obtain as pure a carbonate of lead and sulphate of soda as possible. This could only be done by weighing it as a whole, or in portions; but as the

* Journal de Pharmacie et de Chemie, Oct. 1858.

† Jahresbericht der Chemie, by Will and Kopp, for 1858, p. 6.

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drying of a tubful of sulphate of lead (from 500 to 1200 pounds) was impracticable, and sampling not less so, since the upper strata contained a much larger proportion of water than the lead at the bottom: I contrived the following method, which enabled me to leave the management of the process in the hands of a workman.

I took a strong oaken pail, weighing eight pounds when empty, and caused a black mark to be burnt in horizontally around the inside of the pail, two inches below the rim, up to which mark it held twenty pounds of water. I reasoned as follows: The specific gravity of sulphate of lead being 6·3, the pail if filled up to the mark would hold 126 pounds of pure sulphate of lead. The specific gravity of water being 5·3 less than that of sulphate of lead, it followed that if there was one pound of water in the pailfull of moist sulphate, the pail would weigh 5·3 pounds less than 126 (+8, the tare of the pail) =120·7 (+8); if there were two pounds of water present, the weight would be 115·4 (+8), and so on. This enabled me to calculate a table, giving in one column the actual weight of the pail when filled with moist sulphate, and opposite in a second column, the amount of dry sulphate corresponding to the gross weight. The weight of dry sulphate was thus found as accurately as could be desired, although the amounts varied in practice from 30 to 105 pounds.

It is nothing but an application of the Archimedean theorem, that, when a solid body is immersed in a liquid it loses a portion of its weight, equal to the weight of the fluid which it displaces, or to the weight of its own bulk of the liquid.

This, as I suppose, is precisely the principle applied by Mr. Mène. *The precipitate he obtains by a certain chemical manipulation is a substance of known composition and specific gravity.* Supposing it to be sulphate of lead, and the bottle, when filled with water at the normal temperature, to weigh 70 grammes =50 grammes of water, and 20 for tare. After introducing the precipitate and filling again with water it weighed 71·06 grammes. Now, as the specific gravity of sulphate of lead is 6·3, or as the weight of a cubic measure of sulphate of lead is 6·3 times that of a cubic measure of water, and as the space of one part by weight of water is taken up by 6·3 parts by weight of sulphate of lead, it follows that the quantity of sulphate of lead in the bottle, which has taken up the space of one part by weight of water, increases the original weight of the bottle (filled with pure water) by 5·3. To find the amount of water displaced it is only necessary to divide the overweight (1·06 grammes) by 5·3=0·2, which, added to the overweight 1·06+0·2 gives 1·26 grammes as the weight of the precipitate.

Hence the rule, which is of great convenience in volumetric analysis, that *to find the weight of a moist precipitate, which is a compound of known specific gravity, weigh it in a specific gravity bottle or some other vessel of known weight when filled with water, or any other liquid, at the normal temperature, again fill it with the water or other liquid, divide the excess of the new weight by the specific gravity of the substance, less that of the water or other liquid (that of water being =1) and add the quotient to the overweight, which gives the weight of the precipitate.*

The editor of the Jahresbericht appears to have overlooked the fact that the precipitates weighed in this manner are definite compounds, the specific gravity of which is well ascertained.

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The principle I have exemplified above may not be novel; but as I have never met with it, chemists, as well as manufacturers (especially of colors), will probably also find it of interest, and certainly highly practicable and easy of execution.

36 Beekman street, New York, Feb. 3d, 1860.

3. *New Chemical Journal.*—The Chemical News (with which is incorporated the Chemical Gazette), edited by WILLIAM CROOKES. London: Weekly. Price 3*d*., stamped 4*d*. 8vo. 12 p. each number. This new Journal commenced on the 10th of December last, and eight numbers have already reached us. The contents are divided under Scientific and Analytical Chemistry, Technical Chemistry, Pharmacy, Toxicology, &c., Proceedings of Societies, Notices of Patents, Correspondence, Scientific Notes and Queries, Laboratory Memoranda, Miscellanies, and Answers to Correspondents. Mr. Crookes is favorably known by several valuable researches, and thus far has shown good judgment and spirit as an editor. His verbatim reports of the late lectures of Dr. Faraday (the Holiday lectures) at the Royal Institution, attest his appreciation of the true sources of vitality for such a journal.

4. *American Druggists' Circular and Chemical Gazette;* N. Y. Feb. 1860. 4to.—Although chiefly special and wholly technical in its objects, this Journal (which has now reached its 4th volume, whole number 38) is conducted by Mr. Mayer and others, in a manner to entitle it to rank as a valuable coadjutor in technical chemistry.

II. GEOLOGY.

1. *On some of the Igneous Rocks of Canada;* by T. STERRY HUNT, F.R.S. (In a letter to one of the editors, dated Jan. 1860.)—There occurs in the district of Montreal a series of isolated hills running nearly east and west for a distance of ninety miles along the line of an undulation which has disturbed the lower Silurian strata. These hills, which often cover considerable areas, consist of igneous rocks which have apparently been solidified under a considerable pressure, and have subsequently been exposed by the denuding action which has removed from around them the soft and unaltered palæozoic strata. The names of these mountains counting from the west are Rigaud, Mount Royal, Montarville, Belœil, Rougemont, Yamaska, Shefford and Brome, to which we may add Monnoir a similar mass lying somewhat to the south of Belœil.

I am now engaged in the study of the various rocks composing these mountains, which offer great diversities in lithological character and composition. Prominent among them we may mention the trachytes, which in their various types of compact, granular, porphyritic and granitoid are abundant. The mountains of Brome and Shefford appear to be made up entirely of a granitoid trachyte, which consists of crystalline orthoclase, without quartz, and with small portions of hornblende or mica, sphene and magnetite. The orthoclase in a great number of these rocks which I have analyzed contains like sanidin a large proportion of soda. The other varieties of trachyte which occur in veins and dykes often contain a portion of carbonates amounting to from 6·0 to 18·0 p. c. and consisting chiefly of carbonate of lime with some magnesia and

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iron. Some of these rocks pass into phonolites through the admixture of a silicate which gelatinizes with acids and has the composition of natrolite. This mineral in one case amounted to more than 40·0 p. c. of the rock, the remainder being orthoclase with a small amount of carbonates.

A large part of the mountain of Yamaska consists of a coarsely crystalline diorite the feldspar of which approaches anorthite in composition, while an apparently similar diorite, which makes up the mass of Monnoir, contains oligoclase in large crystals. Other diorites from this series contain labradorite; mica and sphene, in small quantities, are often present.

Dolerites are also abundant, and sometimes pass, owing to a scarcity of feldspar, into an augite rock, generally with ilmenite and magnetite. A fine-grained dolerite from Rougemont contains abundance of crystallized olivine, and a large part of Montarvilles consists of a remarkable granitoid rock, made up of a crystalline feldspar, in some parts at least labradorite, with sparsely disseminated crystals of black augite, a little brown mica, and a great abundance of crystals of honey-yellow olivine, which amount to more than 45·0 p. c. of the mass. The composition of this olivine I have found to be silica 37·17, magnesia 39·68, protoxyd of iron 22·54 = 99·39.

Many of these diorites and dolerites, except in their lithological structure, closely resemble the stratified rocks made up of anorthic feldspar, with hornblende and pyroxene, and containing magnetite and ilmenite, which are so abundant in the Laurentian system, suggesting the notion that the intrusive masses may be nothing more than these stratified rocks displaced and injected among the Palæozoic strata. Durocher has already pointed out a similar resemblance between the intrusive rocks of some parts of Scandinavia, and the subjacent gneiss.—(*Bul. Soc. Geol. France*, [2] vi, 33.)

The granitoid trachytes as well as the dolerites, diorites and peridotite (olivenite rock) make up mountain masses, while the earthy and porphyritic trachytes and the phonolites are generally found cutting the above, and the adjacent strata. The absence of quartz or of any excess of silica from all these rocks is a remarkable feature. Farther to the east, however, intrusive granites are very abundant; these penetrate the Devonian strata but are older than the carboniferous. Quartziferous plutonic rocks are also abundant in the county of Grenville, where they penetrate the Laurentian series. These plutonic rocks consist of dolerites, syenites and eurites, which are in their turn cut by dykes of very beautiful porphyries. The base of these is jasper-like, black, red or green in color, and encloses crystals of red orthoclase and occasional grains of quartz. The analysis of the base shows it to consist of the elements of orthoclase with an excess of silica and a little oxyd of iron. The syenites are cut by large veins of chert, and in the vicinity of these have been changed into a sort of kaolin from a decomposition of the feldspar, which may have been the source of the silicious accumulations. This group of igneous rocks, which is overlaid by the Potsdam sandstone is very unlike those which we find penetrating the palæozoic strata.

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The details of my investigations on these rocks, so far as completed, will be found in the Report of the Geological survey of Canada, for the last year, now in press; a first portion has already appeared in the Report for 1853–56, p. 485. It is by the systematic study of different series of igneous rocks that we may hope to arrive at just notions as to their origin, their mode of formation and their relations to metamorphic sedimentary rocks.

2. *Notes on the Dolomites of the Paris Basin, etc.;* by T. STERRY HUNT, F.R.S. (In a letter to one of the Editors, dated Montreal, Feb. 3, 1860.)—The gypsums of the Paris basin are evidently not of epigenic origin but regularly stratified and alternating with marls and limestones. In September, 1855, I visited with Elie de Beaumont and several members of the Geological Society of France, the gypsum quarries at the hill of Chaumont, and there insisted upon the views which I have since urged in this Journal (vol. xxviii, p. 365) upon the different origins of gypsum. For a report of my remarks on that occasion see the *Bulletin de la Société Geologique de France*, [2], xii, 1306.

I have subsequently shown in the memoir just cited in the last volume of this Journal that the formation of these stratified gypsums by the double decomposition of bicarbonate of lime and sulphate of magnesia involves the production of carbonate of magnesia, which unless carried away or decomposed by an irruption of sea water will be found to overlie the gypsums forming the dolomite which is their common associate. The presence of carbonate of magnesia in the gypsiferous series of the Paris basin has hitherto been unnoticed, and having at the time above mentioned collected specimens from the quarries at Chaumont I was recently induced to examine the so-called *white marls* which overlie the gypsiferous series, and find them to be magnesian. The analyses of two specimens, one penetrated by seams of gypsum, gave each about 60·0 per cent of dolomite mingled with clay. The Paris gypsums then offer no exception to the general rule.

Beneath the gypsiferous series and in the lacustrine group known as the lower travertine, or St. Owen limestone, occur beds of a whitish, very fissile, shaly matter, enclosing concretions of menilite (opal), and consisting of a hydrated silicate of magnesia, identical with meerschaum or quincite in composition, intermingled with small portions of earthy carbonates. I have examined a specimen of this mineral which I collected near Paris and find it to be the same with that described by Dufrénoy and Berthier as occurring in similar positions in various other localities. This appearance of beds of a silicate of magnesia approaching talc in composition, in the midst of unaltered deposits, is interesting inasmuch as it seems to show that such silicates may be formed in basins at the earth's surface by the reactions between magnesian solutions and dissolved silica. I have many years since described the existence of similar silicates among the deposits during the artificial evaporations of natural alkaline waters, and farther inquiries in this direction may show us to what extent certain rocks consisting of calcareous and magnesian silicates may be directly formed in the moist way.

I propose to send you very soon a supplement to my paper on Gypsums and Magnesian rocks, describing some recent experiments which

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confirm what I have already announced that no dolomite is formed in the experiment of Von Morlot and Haidinger, recently resuscitated by Charles Deville, and appealed to by Prof. Phillips in his last Annual Address to the Geological Society of London, as resolving the problem of the origin of dolomite. This double salt is however readily formed when a mixture of the moist amorphous carbonates (such as is obtained by precipitating in the cold by an excess of carbonate of soda a solution of the chlorids of calcium and magnesium in equivalent proportions), is gradually heated under pressure.

3. *New Palæozic Fossils;* by J. H. McChesney. Chicago, 1859. 8vo. pp. 64. In this publication the following species from the Carboniferous rocks of the Western States are noticed as newly described:

Crinoidea.—*Platycrinus ornogranulus, P. inornatus, Scaphiocrinus longidactylus, Zeacrinus bifurcatus, Z. mucrospinus, Actinocrinus asterius, A. tenuisculptus, A. subæqualis, A. Fosteri, A. subventricosus, A. urnæformis, A. Hurdianus, A. æquibrachiatus, A. Andrewsianus, A. Hageri, Forbesiocrinus Pratteni.*

Brachiopoda.—*Orthis, Kaskaskiensis, O. Lasallensis, O. Pratteni, O. Richmonda, Productus asperus, P. symmetricus, P. Wilberanus, P. tubulospinus, P. fasciculatus, P. inflatus, P. pileiformis, Ambocelia gemmula, Spirifer transversa, S. subelliptica, S. perplexea, S. subventricosa, Retzia subglobosa, Athyris spiriferoides, A. orbicularis, A. differentius, Terebratula inornatu, Rhynchonella Eatoniæformis, R. explanata, R. carbonaria, R. Algeri, Trematospira Mathewsoni, Discina caputiformia.*

Lamellibranchiata.—*Leda Oweni, L. gibbosa, L. polita, Nucula parva, N. cylindricus, N. rectangula, Astartella varica, Edmondia concentrica, Allorisma clavata, A. sinuata, Myalina Swallovi, Nuculites Vaseyana, Pinna Adamsi, Syringopora multattenuata, Cyathoxona prolifera.*

Gasteropoda.—*Bellerophon ellipticus, B. vittatus, B. Blanyana, B. Stevensiana, Pleurotomaria Beckwithana, P. nodomarginata, Natica Shumardi, Platyceras crytolites, Platyostoma Peoriensis, Bucania Chicagoensis.*

Cephalopoda.—*Nautilus Forbesianus, N. Illinoiensis, N. quadrangulus, N. nodocarinatus, Gonitites Hathawana, Cyrtoceras (Lituites?) giganteum, Trochoceras Desploniensis, Orthoceras Rushensis, O. Knoxensis.*

The typography of this brochure is very good, and the descriptions of the species show that the author has a wide acquaintance with the Carboniferous fauna of the West. We think, however, that the science would present to the student a much less formidable array of difficulties, if Palæontologists would on all occasions give accurate measurements of the species they describe. Throughout this book, for instance, the size of the individual is recognized as a specific character, and yet as no dimensions are given it must be impossible for even the most experienced practical naturalist to decide whether *O. Lasallensis, O. Richmonda* and *O. Pratteni* are small or large forms; whether two lines or two inches wide. The first mentioned of these species is also said to have the "*surface marked by sharp rugose radiating striæ increased by implantation,*" but so have a great many other *Orthides*, and until some standard for comparison is furnished, all the students of this book, except those who may have access to the original specimens, must remain in doubt upon the question as to whether there are ten or twenty striæ in the width

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of one line. We find it stated further that in *O. Richmonda* the striæ are "finer and less rugose" than they are in *O. Lasallensis* while in *O. Pratteni* they "are not so distant and not so rough," as in either of the other two, but as we have no means of ascertaining their size in the first named species, which is here made the standard, all that relates to the surface characters of the second and third might have been left out without detracting anything from the value of the description. No doubt all this appeared to be sufficiently clear to the author while he was engaged with the specimens before him, but we think upon a little reflection he will agree with us, that without some clue to the size and surface characters of these species, they cannot be identified. Where a shell is said to be smaller or larger than some other species which has been described in some other book, the difficulty would be somewhat less, provided that access could be had to that book, and even then, such questions as "how much smaller?" or "how much larger?" cannot be decided. By adding two or three lines to their descriptions, palæontologists may add a vast deal to the value of their labors, and save others engaged in the same pursuits much perplexing, and too often fruitless, intellectual toil. It is not always possible to give figures of new species, but it is easy to furnish measurements. The absence of these is the great defect in the work before us, and it is a defect that may be observed in books of much greater pretensions. We notice a new genus, (AMBOCELIA) among the Brachiopoda, which is said to have been "recently established by Prof. James Hall," in the "Regents' Report of the State of New York for the present year," (meaning 1859). Has this report been published? If not, then the genus is not established. No author can establish a genus before he has published his description, and, even then, he may not succeed in shewing that it is new.

The practice of antedating genera and species should be discountenanced, as it cannot be beneficial to science. Where specific names are derived from names of persons or places, the initial should be a capital letter, a rule which has been of late much disregarded.

4. *Explorations in Nebraska.*—Dr. F. V. HAYDEN, in a letter to Prof. DANA, dated Deer Creek, Nebraska, Dec. 1, 1859, states, after speaking of the interest of the region, that as soon as the grass is sufficiently high this coming Spring, and the swollen streams permit the passage of pack trains (which cannot be sooner than the first of June), Capt. Reynolds proposes to divide his party into two divisions—one party in charge of Lieutenant Maynadier, Asst., will pass up the Wind River Valley along the western side of the Big Horn Mountains. The other division under Capt. Reynolds will proceed up the Platte to the South Pass, exploring the Wind River Mountains—the two parties to spend the 4th of July at Clark's Pass. Here Lt. M.'s party will proceed down the Yellowstone to Fort Union, and Capt. R.'s party will reach the same point by crossing at the head waters of the Missouri and descending that stream, and thence to the States as soon as possible. Such is the programme of explorations; it may be slightly varied, but in any case will traverse a country very interesting for geology.

Dr. Hayden, in addition to his geological labors, has since 1852 interested himself in ethnographical studies, and will have by the end of this trip material enough for a large volume on this subject.

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5. *Geological Surveys of South Carolina and Kentucky.—The geological survey of South Carolina* has unfortunately been stopped by the unwise action of the legislature of that State at its last session. Dr. O. M. Lieber, late the State Geologist, is occupied in preparing his final report, which will embrace Anderson and Abbeville district, and a part of Edgerfield. Dorn's gold mine is described in it.

The geological survey of Kentucky is continued with unabated zeal.* We have received the Synoptical Report for the past year—a brochure of 50 pages—by Dr. D. D. Owen, principal, aided by S. S. Lyon and Jos. Lesley, Jr., Topographical Assistants, Leo Lesquereux, Palæontological Assistant, and Dr. Robert Peter, Chemist. Dr. Owen here expresses the opinion that "the report of Mr. Lesquereux of the last season's work (now completed), is by far the most practically useful geological report on this subject (coal), which has ever appeared, not only in the United States but in any part of Europe."

In his synopsis of this forthcoming report, Mr. Lesquereux says, "The third section of the report contains a short comparison of the distribution, geologically and geographically, of the coal strata in Kentucky, Ohio and Pennsylvania. This comparison is of high scientific interest, as it fixes the general distribution of the coal strata in the whole extent of the coal basins of the United States, and cannot but give to the geological reports of Kentucky a great value as containing the key of the general distribution of the coal. Henceforth all the reports treating of the distribution of coal strata will naturally take their guide and standard of comparison from the section in the Kentucky coal-fields."

6. *First report of Progress of the Geological and Agricultural Survey of Texas;* by B. F. Shumard, M.D., State Geologist. Austin, Texas, 1859. pp. 17.—We learn from this brochure that satisfactory progress has been made in the preliminary reconnaissance of the vast territory (237,500 square miles) included within the State of Texas. Dr. Shumard states that a more complete series of the geological formations exists in Texas than in any State of the Union, ranging from the Potsdam sandstone to the latest Tertiary. He has made no less than *seven* lines of section extending in various directions a collective distance of 1220 miles, determining his levels by the barometer. He has found time also for minute and final surveys of eleven counties and partial surveys of several others. The coal measures cover an area not less than four thousand or five thousand square miles, with a thickness of eight or nine feet of coal in about 300 feet of coal rocks. The coal is good in quality. Extensive beds of brown coal also occur in the Tertiary rocks in the eastern and middle portions of the State.

The fossil forms of the various strata in Texas are very abundant, and Dr. Shumard informs us that his collection is already very rich.

From the head waters of the Brazos river he has been fortunate enough to obtain a fine mass of meteoric iron weighing about 320 pounds, and a smaller mass of the same kind from Denton county, specimens of which have reached us.

* Notwithstanding the cramped appropriation of money for the two years past —two-thirds of the whole sum being consumed by expenses of publication, which is an absurdity unworthy of the spirited state of Kentucky.

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7. *Post-pleiocene Fossils of South Carolina;* by FRANCIS S. HOLMES, A.M., &c. Nos. 6 to 10 inclusive, containing plates 11 to 20 inclusive. Quarto. Charleston, S. C., 1859; Russell & Ivens.—This beautiful monograph, previously noticed,* is continued in the same excellent manner in the numbers now received. The description and figures of the mollusca end with the part containing Nos. 6 and 7, and with Nos. 8, 9, and 10 commences a "Description of Vertebrate Fossils, by Prof. Jos. LEIDY." The student will refer to this memoir with particular interest at the present moment, when so much attention is being given to the occurrence of human reliquiæ with the remains of animals heretofore judged to be extinct before the human epoch. The Eocene and Post-pleiocene beds on the Ashley River are exposed to the wash of the water, and "the fossils washed from them form part of the shingle on the shore, and here become mingled with the remains of recent indigenous and domestic animals, together with objects of human art." Of those vertebrate remains actually obtained in excavations of the Post-pleiocene and Eocene formations, more confidence is felt in determining the actual age to which they belong. Both the collections submitted to Dr. Leidy by Prof. Holmes and Capt. Bowman, contain remains of the horse, ox, sheep, hog and dog, which we feel strongly persuaded, with the exception of many of those of the first mentioned genus, are of recent date, and have become intermingled with the true fossils of the Post-pleiocene and Eocene periods on the Ashley River and its tributaries. In regard to the remains of the horse from the facts related (in this memoir) we think it must be conceded that several species of this animal inhabited the country of the United States during the Post-pleiocene period, contemporaneously with the mastodon, the giant sloth, and the great broad-footed bison."—pp. 99–100.

8. *Assiniboine and Saskatchewan Exploring Expedition;* by HENRY YOULE HIND, M.A., Prof. of Chemistry and Geology in Univ. of Trinity Coll., Toronto. Toronto, 1859. 4to, pp. 202, with many maps, sections and plates.—This valuable Report comes to hand at too late an hour to enable us to do more than give its title, deferring to our next an analysis of its contents.

9. *Geology for Teachers, Classes, and Private Students;* by SANBORN TENNEY, A.M., Lecturer on Physical Geography and Natural History in the Mass. Teachers' Institutes. 12mo, pp. 311. Philadelphia, 1860.—As the scope of this little work comprises the whole range of geological phenomena, it gives necessarily a very concise account of the various departments included. The matter however seems in most cases well selected, and shows the author to be acquainted with his subject. The book is illustrated by some 200 good wood-cuts, and has one excellent feature;—that the illustrations are taken, whenever possible, from *American* objects, which cannot be said of most geological text-books previously published in this country, the authors of which have generally gone to Europe for their examples. American geology should be the main subject of American text-books, and it is pleasant to see a step taken in the right direction. Our author however is not quite up to date in some instances, as in ignoring our Permian and western Jurassic beds; in including the Lias in the Oölitic system, etc. W. S.

* This Journal, xxvii. 156.

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III. ZOOLOGY.

1. *On Botanical and Zoological Nomenclature;* by Wm. Stimpson.—A more careful attention to the subject of nomenclature is urgently demanded of the followers of all branches of Natural History. It is a subject to which too little attention has been paid in an abstract or general sense, and too much perhaps in particular cases. A comprehensive code of rules, recognised by the authority of the greater lights of science, has been always needed. This was attempted during the last century by Linnæus and Illiger, and in 1842 "Rules of Nomenclature" were drawn up by the British Association, and ratified by the American Association in 1845. These are excellent as far as they go, but need much extension and many additions, as any one may observe who attempts to decide by them all questions which occur in his experience.

On the other hand, in particular cases of species and genera, the discussion of questions of nomenclature has reached such a pitch that it is no uncommon thing to see the greater part of a new zoological work devoted to synonymy. One author, after six pages of historical and synonymical matter, evincing great critical acumen and much bibliographical research, will arrive at what appears to him to be a certain and final conclusion that the true *Orthonymus aliquis* is such and such a species. The next writer who succeeds him in the same field will triumphantly prove in *ten* pages that it is not that species at all, but the *O. neminis*. And so on to the end of the chapter, if it ever will have an end, which is doubtful unless some decided action is soon taken by naturalists for the purging of their favorite science from this opprobrium. After all the pages which have been written upon some of these cases we seem no nearer to a settlement than at first. The difficulty increases rather than diminishes, —each succeeding author putting forth views differing from those of his predecessors. All this discussion, let us bear in mind, is merely preliminary, and for the purpose of indicating with certainty an object about which the author has perhaps not a dozen words to say.

Now it may appear at the first glance that the application of the law of priority is exceedingly simple. The name given by the first describer of a genus or species is to be respected, and applied to that genus or species throughout all time. But as soon as we come to apply this rule, we find cases without number in which complications occur, rendering limitations of the law necessary. Genera are to be subdivided, and are subdivided with different limits by different authors; the species of one are found by another to include two or three distinct forms, and so on. Some of the limitations of the law of priority have been laid down in the "Rules" of the British Association, but not enough to enable us to decide half the cases which may arise,—leaving the remainder subject to the whims or dependent upon the extent of the knowledge of the author who would follow them.

In applying the great law, the most difficult question of all immediately arises,—What constitutes a description? or, When has an author so designated his species that his name for it should hold? On this subject we have every variety of opinion, from that of the German ornithologists, who consider that a simple published name, referring to a specimen

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in a museum, is sufficient, to that of the lamented Edward Forbes, who once insisted that no name proposed should be accepted unless accompanied by a *Latin* description or an illustrative figure. The first opinion we believe to be scouted by nine-tenths of living naturalists;—the second appears to be too stringent, as an author can of course write better in his own language than in any other, though we doubt if a description appearing in Chinese would gain the least notice from modern naturalists.

The question, "What constitutes a description," can never be decidedly answered. No rule can be proposed which is universally applicable. With regard to its *length*;—we may say that two words are not sufficient, an hundred are; but where shall we draw the line? The two sentences of one author may be better than the two pages of another. One writer will describe an object well except in one point, in which from defective observation, a character is represented in exact opposition to the true state of the case. Some descriptions are sufficient to enable the naturalists of one country, from their collateral knowledge, to determine a species, while these of another country or continent would be left entirely in the dark. An author may publish descriptions in a work for private distribution, which will be inaccessible to the great body of naturalists. We might fill many pages with such cases as these, and yet, were rules made out applicable to each, there would still be cases constantly arising which could be decided by none of them. How then can the matter be settled in these latter instances? We will suggest a method further on.

It will be observed that it is among the more common and earliest-described species that the synonymic heap is greatest. This is exceedingly embarrassing to the student, who in general has occasion to use these very species, being those most easily accessible, in the course of his studies. He may find in a dozen different books the characters, anatomical or otherwise, of what appear to him a dozen different objects, since the names used may be different, and elementary works cannot be expected to go into synonymical details. At the present day, thanks to the advance of knowledge and precision, and the international exchange of scientific works, the name of an entirely new genus or species may escape the burden to which that of older species is subjected. It is with those published in the last century that the greatest trouble occurs. Investigators among antique and forgotten books are constantly finding some obscure work or paper, perhaps scarcely known out of its immediate vicinity even at the time it was published, in which names occur which must be adopted, in the opinion of some, to the exclusion of the familiar titles which have been used for half a century. The disinterment of Klein's name *Cyclas* is an instance of this. How strange it must seem to a conchologist of the present day to be obliged to designate the common marine *Lucina* by a name which has been in use seventy years for a freshwater bivalve, while this freshwater bivalve becomes *Sphærium*; and to use *Cyclostoma* for *Delphinula*, *Terebellum* for *Turritella*, etc. The restoration by G. R. Gray of Boddaert's names in ornithology is another instance. By the discovery of a meagre pamphlet of the eighteenth century, only two or three copies of which now exist, we find ourselves forced to change the generic names of common birds, familiar as they are by long and constant usage.

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In the discussion of these questions all personal considerations should be entirely rejected. The smallest interest or convenience to the science in general, followed as it is by a republic of thousands, is of more importance than any compliment to the feelings of a living, or the memory of a deceased naturalist. In fact our mere recognition of an author's names is not of such vast importance to his reputation. His fame must rest upon a securer foundation than this. For the custom of placing the name of an author after a species described by him is not (or should not be) done for that author's personal advantage, but simply to assist us in the recognition of that species. It is a short method of referring to the place where the description of the species may be found, or enables us to distinguish it from some other to which the same name has been by mistake applied; as, *Pleurotoma violacea*, Hinds, *non* Mighels. In this view, how ludicrous it appears, to hear, as we often do, naturalists complain that if the custom of placing after a species the name of that author who first placed it in its proper genus is adhered to, more than one-half of Linné's species will be wrested from him. Does the fame of the great Linnæus depend upon the number of species he described?

We will now mention a few points concerning which great difference of opinion exists in the minds of naturalists, and which for the good of science should be immediately settled in one way or the other. The first is: shall the same generic name be allowed to occur in different departments of zoology or botany, or even in both these, or, we may add, in other sciences. Many are of the opinion that they may be used, and should not be changed, if so occurring;—in view of the great difficulty now experienced in selecting a name which is not preoccupied, and shall be at the same time descriptive or suggestive of the object intended. But what is the object of a name? Surely, the *main* object is to enable us to distinguish one thing from another, and from all others, that when it is used we may know what is intended, and not be forced to decide by other aids. Is it not of vastly more importance that a name should serve this purpose, than that it should remotely indicate (which is the most generally possible) some character of the object, which it may after all hold in common with an hundred others? Greek compounds are by no means exhausted yet, and if they were, we might fall back upon euphonic names, which serve the purpose however barbarous they may appear in the eyes of some. The custom of using the same name for many diverse objects is productive of serious inconveniences. If we have stars, countries, minerals, plants, vertebrates, articulates, mollusks and radiates, all named alike, some singular anomalies might occur, since we can of course reduplicate *specific* appellations as often as we please in different genera. For instance, suppose a travelling naturalist "making his researches in Arizona, observed specimens of the *Arizona pœtula* (hermit-crab) inhabiting the shell of *Arizona pœtula* (univalve), creeping among the roots of *Arizona pœtula* (shrub); and upon examining it anatomically, found great numbers of the *Arizona pœtula* (infusorium) living in its gills. The *Arizona pœtula* (bird) was feeding upon these crabs with great voracity," etc.

Another point. A genus may contain a vast number of species, and yet from want of profound investigations no one may see the propriety

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of dividing it up. As occurs very commonly, in the course of time some new species belonging to it are described under names, which, being preoccupied in that genus, are very properly changed. The new designations become established and may be used for years. At last it becomes necessary to divide the genus, and the species whose names have been referred to are found to belong to different genera. Shall the old reduplicated specific name, or the substituted one be now adhered to? Naturalists are about equally divided in opinion upon this point.

The propriety of using small initial letters to proper specific names, nouns or adjectives, has been made the subject of discussion. Whatever method be followed here, it would seem that uniformity is desirable; if any of these proper names are to have small initials, why not all? Most zoologists and botanists seem in this matter to follow the usage of their own language rather than that of the Latin, or any uniform system. The Germans will have all nouns begin with a capital, and all adjectives with a small letter, as *Ocypode Cursor*, *Chiton emersonianus*, whereas the English write common nouns with a small initial, and all *proper* appellations, whether nouns or adjectives, with a capital, as *Ocypode cursor*, *Chiton Emersonianus*. The truly convenient system will be to write all specific names without exception, with a small initial letter, as is done by one of the most eminent zoologists of this country, and by many of those of Europe. We shall then have no difficulty in distinguishing specific from generic names, and may discuss the relations of species without the necessity of repeating the generic name or its initial every time they are mentioned. A proper name, modified for use as a specific appellation, becomes a part of a new title, and involves a different idea.

We will not detain the reader by discussing other mooted points, as whether ante-Linnæan names shall be accepted, if binomial; whether names of faulty etymology shall be corrected, etc. The above are only mentioned as instances of the necessity of establishing many rules to produce uniformity of usage among naturalists. In pointing out how this may be satisfactorily done, we proceed to our promised suggestion.

We have somewhere read, that when any orthographical or other difficulty occurs in the use of the French language, the case is immediately referred,—in accordance with the admirable *system* for which the nation are remarkable both in science and literature,—to the Academy, who decide upon it, arbitrarily it may be, but finally. The action of this tribunal is respected, and no farther uncertainty or diversity in the use or spelling of the word can occur to embarrass French authors. Now why may not a similar mode of action be of use in science, and enable us at last to settle all our difficulties. Science is cosmopolitan, not national. Let a convention meet at Paris or some other central point, composed of delegates from all the scientific societies of the earth, and representing at least all the departments of zoology and botany. Here they may hold sessions of the entire body, for a sufficient length of time to establish all the general rules of nomenclature which can be conveniently applied. But as we have before observed, there are some particular cases for which no rule will serve, and which must in some way be decided separately and *arbitrarily*. For the settlement of these cases let the convention divide itself into as many sections or committees as there are classes of

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plants and animals; a committee of ornithologists for the birds, of entomologists for the insects, etc. These committees being composed of experts in the various branches, will not find it difficult to discuss and pass judgment by vote upon the name of each contested genus or species. Let these decisions be respected, and let the names of those who will not abide by them be placed upon a new edition of that black-book which LINNÆUS kept of old,—the list of *Damnati!*

2. *Les genres Loriope et Peltogaster*, H. Rathke; par W. LILJEBORG, Professeur de Zoologie à Upsala en Svède. pp. 35, 4to, pl. 3. (Extr. des Nova Acta Reg. Soc. Sc. Upsal, Ser. 3, Vol. iii.)—M. Liljeborg has unraveled a singular history in relation to the curious sac-like parasites found on the abdomens of Decapod crustaceans. They were first observed by Cavolini, and in more recent times by Rathke, who considered them to be worms, probably Entozoa, and instituted the genus *Peltogaster* for their reception, describing two species, *P. paguri* and *P. carcini*. Diesing placed them among his Myzelmintha, and formed a new genus, *Pachybdella*, for *P. carcini*. These forms were made the subject of discussion by Kroyer, Steenstrup, O. Schmidt, and Lindstrom, who agreed with Cavolini in referring them to the Crustacea, but could only conjecture their more intimate relations, although suggesting those with the Entomostraca, the Lerneidæ, or the Bopyridæ. From a study of their larvæ M. Liljeborg now ascertains their true place to be among the Cirripedes, and describes two new species.

Within the body of *Peltogaster paguri*, Rathke found a minute Tetradecapod, scarcely a line in length, which he considered to have been swallowed as food by that animal, and described it as an Amphipod under the name of *Loriope*. It was afterward referred to the Tanaidæ by Dana, who described a new allied genus *Cryptothir*. Its history however remained very obscure, although it was demonstrated that, being always found alive, it was not the food of the *Peltogaster*, and some naturalists even suspected it to be the male of that parasite. By a fortunate discovery M. Liljeborg has now cleared up the difficulty. In examining a *Peltogaster* taken from the abdomen of *Pagurus pubescens*, he found attached to it another sac-like body filled with Loriopes, which might well have been taken for the egg-pouch of the *Peltogaster* itself, but which after careful study proved to be a distinct animal,—a parasite upon a parasite! It proved, in fact, to be the female of the *Loriope*, grown monstrous by a process of degradation similar to that observed in the female of the *Bopyridæ*, to which family indeed the *Loriope* must now be referred. The occurrence of the young Loriopes in the digestive cavity of *Peltogaster* is, then, simply adventitious.

3. *Neue Wirbellose Thiere, beobachtet und gesammelt auf einer Reise um die Erde*, 1853 bis 1857, von LUDWIG K. SCHMARDA; 1st Band, 1st Hälfte, 4to, pp. 66, and 15 colored plates. Leipzig, 1859. (New York, B. Westermann & Co.)—A quarto volume, handsomely bound and splendidly illustrated, containing descriptions of the Turbellaria and Rotatoria collected and observed by Dr. Schmarda during a voyage round the world. The descriptions of the animals belonging to the former order are made additionally clear by woodcuts showing the shape of the head and the distribution of the ocelli. A review is given of the genera of

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each order. The Dendrocœla are divided into two divisions, *Acarena* and *Carenota*, the former having no distinct head, which those of the latter group possess. We should judge this to be a character of much less importance than those derived from anatomical characters, which forbid such a division. Following it, the author separates *Planaria gonocephala* from the other freshwater Planariæ and places it with such forms as *Cephalolepta* and *Planaria oceanica*, Darwin! There is also a large number of genera hitherto considered sufficiently well established, which are entirely ignored by M. Schmarda, as *Prosthiostomum*, *Dendrocœlum*, *Procerodes*, *Fovia*, *Bdelloura*, *Geoplana*, and *Rhynchodemus*. The new genera of Dendrocœla are *Dicelis*, *Prosthecereus*, *Homalocereus*, *Goniocarena*, *Carenocereus*, and *Sphyrocephalus*. Of these several have been previously established under different names. *Prosthecereus* may be adopted, as Quatrefages' name *Proceros* is preoccupied. *Goniocarena* is *Dugesia* Girard, *Carenocereus* is *Nautiloplana* Stm., and *Sphyrocephalus* is *Bipalium*.

The Nemertinea are subdivided upon more certain grounds than the preceding order, but we are at a loss to understand why the proboscis should not be considered as the mouth, as it is certainly the aperture through which food is introduced into the body. Oken's name *Borlasia* is adopted (although it is exactly synonymous with *Lineus*, Sow.) and made to include *Amphiporus*, *Acrostomum* and *Baseodiscus* of Diesing. A new genus, *Loxorrhochma*, is established for the *Polia coronata* of Quatrefages. The figures are excellent, showing well the colors, which among these animals form the most reliable specific characters, and notwithstanding errors of nomenclature and arrangement, we have to thank M. Schmarda for an exceedingly interesting and beautiful work. W. S.

4. *A Supplement to the "Terrestrial Air-breathing Mollusks of the United States and the Adjacent Territories of North America;"* by W. G. Binney. Boston, 1859. 8vo, pp. 207, and 6 colored plates. (Extracted from the Journal of the Boston Society of Natural History.)—This much needed supplement to Dr. Binney's great work contains the additions and corrections which have accumulated in the rapid progress of the science during the seven years which have elapsed since the publication of that work. Several doubtful species which were omitted in that work have since been investigated, and are now included or referred to their proper place in the synonymy. Besides these, we have figures and descriptions of all the recently discovered species, of which the number is large, particularly among those of the Pacific coast. These are placed together in the first part of the volume. The writings of foreign authors upon our land-snails are properly discussed, and their descriptions reprinted in full wherever there was any doubt as to the species to which they should be referred. Thus the whole subject is thoroughly posted up to the date of Jan. 1st, 1859, and the work forms a most acceptable contribution to American conchology. W. S.

5. *Catalogue of the Recent Marine Shells found on the Coasts of North and South Carolina;* by J. D. Kurtz. 8vo, pp. 10. Portland, Me., 1860.—This catalogue shows the results of the author's researches in the conchological fauna of our southern coast made in the years 1848–52. The number of species given is 204, an increase of 78 over that given

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by Prof. Gibbes in his catalogue published in Tuomey's "Geology of South Carolina," published in 1848. Several species are mentioned as occurring on the marl-bottom off the N. C. coast which have not been hitherto observed north of Florida. Four new species are briefly described, viz., *Venus trapezoidalis*, (which is perhaps the same as *V. pygmœa*,) *Arca Holmesii*, *Scalaria rupicola* and *Chemnitzia textilis;* and several new ones are mentioned by name only. Capt. Kurtz has also contributed very largely to our knowledge of the marine animals of the same coasts, in other departments, as our zoological archives abundantly show. W. S.

Proceedings Philadelphia Acad. Nat. Sci., 1859.—p. 281, Resolutions on the death of Thomas Nuttall.—A new Unio from the Isthmus of Darien; *I. Lea.*—Additions to the Coleopterous Fauna of Northern California and Oregon; *J. L. LeConte.*—p. 294, Notes and Descriptions of Foreign Reptiles; *E. D. Cope.*—p. 297, A new Myalina and Posidonia from the Carboniferous of Texas; *W. M. Gabb.*—p. 297, New birds from Cape St. Lucas; *J. Xantus.*—p. 299, Notes on Birds collected at Cape St. Lucas by Mr. John Xantus; *S. F. Baird.*—p. 306, Mineralogical Notices; *W. J. Taylor.*—p. 310, New Histeridæ; *J. Leconte.*—p. 317, Contributions to American Lepidopterology. No. 2; *B. Clemens.*—p. 329, On the soft parts of certain Unionidæ; *I. Lea.*—p. 331, Descriptions of three new species of exotic Uniones; *I. Lea.*—Notice of Shells collected by Xantus at Cape St. Lucas: *P. P. Carpenter.*—p. 332, Catalogue of the Venomous Serpents in the Museum of the Academy, with notes on the families, genera, and species (new genus, Teleuraspis); *E. D. Cope.*—Index to vol. for 1859.

Transactions of the Am. Philos. Society (*Philadelphia*). Vol. XII (new series), Part II, contains:—Art. XI. Della Correlazione delle Forze Chimiche Colla Rifrangibitita delle Irradiazione; Di Zantedeschi.—Esperimenti Eeguiti Col Calorico Solave.—Art. XII. Geological sketch of the estuary and freshwater deposit of the Bad Lands of the Judith, with some remarks upon the surrounding formations; by F. V. Hayden, M.D.—Art. XIII. Extinct Vertebrata from the Judith River and Great Lignite Formations of Nebraska; by Joseph Leidy, M.D.—Art. XIV. A sketch of the Botany of the Basin of the great Salt Lake of Utah; by E. Durand.—Art. XV. Observations on the Magnetic Dip in the United States; by Prof. Elias Loomis.

Report of the Twenty-eighth Meeting of the British Association for the Advancement of Science, held at Leeds, Sept. 1858. London, 1859.—Contains the following zoological papers of more general interest:—On the Anatomy of the Araneidea, particularly of their Spinning organs (2 plates); *R. H. Meade.*—Various reports of Dredging committees.—On the reproductive organs of Sertularia tamarisca; *Allman.*—On the Migration of Birds; *Collingwood.*—Anatomy of the Brain in some small quadrupeds; *Garner.*—On the formation of the Cells of Bees; *Tegetmeier.*

Det Kongelige Danske Videnskabernes Selskabs Skrifter, 5te Raekke, Naturv. og Math. Afdeling, 4de Binds. Copenhagen, 1856-59.—Contains the following zoological papers:—Coroctoca and Spirachtha, Staphylines which bring forth living young and are domesticated with a Termite (with 2 plates); *J. C. Shiödte.*—On the Hectocotyle forms of the Octopod genera Argonauta and Tremoctopus (with 2 plates); *J. J. Sm. Steenstrup.*—Attempt at a monographic exposition of Sergestes, a genus of Shrimps, with remarks upon the organs of hearing in the Decapod Crustacea (5 plates); *H. Kroyer.*—On Mephitis Westermanni, a new "Stinkdyr" from Brazil; *J. Reinhardt.*—5te Binds, 1ste Hefte, 1859: Additamenta ad historiam Ophiuridarum,—descriptions of new or little known species of Serpent-stars; 1st and 2d parts, 7 plates, (new genera, Ophiocten, Ophionereis, Ophiactis, Ophiostigma, and Ophioblenna); *Chr. Fr. Lütken.* W. S.

6. *New Zoological Journal.*—Dr. H. F. Weinland, already well known to American zoologists, has commenced, Oct. 1, 1859, at Frankfort on the Maine, a monthly journal (8vo, 16 pp.) entitled *Der Zoologische Carten. Organ für die Zoologische Gesellschaft.*

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IV. ASTRONOMY AND METEOROLOGY.

1. *Supposed intra-Mercurial planet.*—The announcement of M. Le Verrier that the existence of one or more planets within the orbit of Mercury is rendered highly probable by his computations on the movement of the perihelion of Mercury, has called out former observations of the transits of bodies over the sun's disc, in addition to those mentioned in vol. xxviii, pp. 445 and 446 of this Journal.

(1.) Dr. Lescarbault at Orgères (Dept. Eure et-Loir), France, observed with a telescope, March 26th, 1859, a small black circular spot moving across the upper limb of the sun, at a rate which would occupy $4^h\ 26^m\ 48^s$ to traverse the entire disc. Its apparent diameter was less than a quarter that of Mercury in transit. M. LeVerrier thinks the observation worthy of credit, and computes that on the supposition of a circular orbit the time of the planet's revolution around the sun is $19^d{\cdot}7$, and the inclination of its orbit 12° 10′. Its greatest elongation from the sun would not exceed eight degrees, and its light be less than that of Mercury. This planet seems however insufficient to account for all the movement of the perihelion of Mercury.—*Comptes Rend. Acad. Sci., Jan.* 2, 1860.

(2.) Mr. Benj. Scott, of London, states that about midsummer in 1847, he chanced to turn a telescope towards the sun then near its setting, and saw on the sun's disc a well defined black spot, which was not to be seen there at sunrise the next morning. Its angular diameter appeared as large as that of Venus. Mr. Scott mentions that a similar body, or spot, was seen by Mr. Lloft, January 6, 1818.

2. *Mr. Alvan Clark's New Micrometer for measuring large Distances,* (Extracted from the Monthly Notices of the Royal Astronomical Society for July 1859).—At the monthly meeting of the Society in June, Mr. Alvan Clark, of Boston U. S., exhibited a micrometer invented by himself, which is capable of measuring with accuracy any distance up to about one degree. It is also furnished with a position-circle. Its character is essentially the same as that of the parallel-wire micrometer; but it has some peculiarities not, it is believed, previously introduced, and on which its wide range depends.

The most remarkable of these peculiarities consists in its being furnished with *two eye-pieces*, composed of small single lenses, mounted in separate frames, which slide in a grove, and can be separated to the required distance. A frame carrying two parallel spider-lines, each mounted separately with its own micrometer-screw, slides in a dove tailed grove in front of the eye-pieces; and, by a free motion in this frame, each web can be brought opposite to its own eye-lens.

In using this micrometer, the first step is, to set the position-vernier to the approximate position of the objects to be measured. Then the eye-lenses are separated till each is opposite to its own object. The frame containing the webs and their micrometer-screws is then slid into its place; and, the webs having been separated nearly to their proper distance by their free motion in the frame, they are placed precisely on the objects by their fine screws, the observer's eye being carried rapidly from one eye-lens to the other a few times, till he is satisfied of the bisection of each of the objects by its own web. The frame is then removed for

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reading off the measure by means of an achromatic microscope, on the stage of which it is placed. One of the webs is brought to the intersection cross-wires in the eye-piece of the microscope; and by turning a screw (the revolutions of which are counted), the frame travels before the microscope, and the other web is brought to the intersection of the cross-wires. The *parts* of a revolution are read off by a vernier from a large divided circle attached to the screw.

The advantages arising from the peculiar construction of this micrometer are the following:—

1. Distances can be observed with great accuracy up to about *one degree*, and the angles of position also.

2. The webs, being in the same plane, are free from parallax, and are both equally distinct, however high the magnifying power may be.

3. The webs are also free from distortion and from color.

4. A different magnifying power may be used on each of the objects; which may be advantageous in comparing a faint comet with a star.

3. *New Double Stars discovered by Mr. Alvan Clark;* communicated by the Rev. W. R. Dawes. (From Monthly Notices of the Royal Astronomical Society, xx., p. 55. Second series.)

Corr't No.	Designation.	R.A. 1860. h m s	N.P.D. 1860. ° ′	Mag.	Dist. ″	Date of discovery.
13	Lalande 1980	1 0 51	45 32·4	8, 8	0·45	1858, Nov. 29
14	Lalande 2634	1 20 7	47 56·6	8, 9	0·8	1858, Oct. 13
15	99 Herculis	18 1 43	59 27·4	5½, 10½	1·7	1859, July 10
16	* Vulpeculæ	19 52	63 7	7½, 8	0·4	1858, Aug. 30
17	Cygni 153 B / B.A.C. 6959	20 8 3·8	38 57·4	6, 11½	3·8	1859, July 19
18	44 Cygni	20 25 41	53 32·0	6½, 11½	2·8	1859, July 6
19	A.Z. xxiv. 11	21 10 50	26 10·2	7½, 7½	+0·9	1859, July 8
20	75 Cygni	21 34 41	47 21·6	5½, 11	2·9	1859, July 19

All the double stars in this series may be considered as good test-objects for telescopes of from 6 to 8 or 9 inches aperture. But it is not merely as such that they are interesting: they become especially so from the fact that they are all situated in the northern hemisphere, and all but one at more than 30° from the equator. They consequently attain a good altitude at Pulkova; notwithstanding which they have escaped the acute search of M. Otto Struve with the 15-inch refractor, in addition to the previous one by his father with the Dorpat telescope of 9·6 inches aperture. Either, therefore, they have recently undergone considerable change; or, if not, it appears that objects of great difficulty and delicacy may be detected with very perfect telescopes of smaller size, which have escaped the most diligent scrutiny with far larger instruments. The whole of these objects were discovered by Mr. Clark with his own object-glasses of 8 and 8½ inches aperture, and five of them in my own observatory, during his visit to me last summer; since which I have met with *seven others*, of similar character and situation; and it should be mentioned that none of the objects recorded are below Struve's eighth magnitude for the larger star; all smaller, of which many have been found, having been systematically rejected. It appears, therefore, that there is still much oc-

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cupation for telescopes of moderate dimensions, even in this department of astronomy, which might reasonably have been supposed to have been long since exhausted. The *distances* stated in the list are from my own recent measurements.

Haddenham, Thame, November, 1859.

4. *Notice of the Meteor of Nov.* 15, 1859; by Prof. E. Loomis.—In the last No. of this Journal, p. 137, I gave a brief notice of this meteor, but from want of space was compelled to limit myself to a brief summary of results. I have received a large amount of documents relating to this meteor, most of which however are too indefinite to be of much value. I now proceed to present a brief summary of what appear to me the most reliable observations.

At New Haven, Ct., Judge W. W. Boardman saw the meteor descend at an angle of 25° to 35° with the vertical, and it passed from his view at the edge of the dome of a steeple in azimuth S. 35° 34′ W. Continuing the meteor's path down to an altitude of 3° or 2°, we have S. 37° W. for the azimuth of the place where the meteor would have disappeared to him had his view been unobstructed.

At New York city Mr. Tatham was riding in the Bowery, and saw the meteor descend at an angle of 20° with the vertical, and in a range with the middle of the street opposite the Old Bowery theatre. According to the map of the city, this direction was S. 27½° W. The diameter of the meteor appeared to be about one-third that of the full moon.

A correspondent of the Evening Post, walking down Broadway, saw the meteor disappear in azimuth S. 25° W.

Mr. Gould, also in Broadway, saw the meteor disappear behind a building in azimuth S. 23½° W.

Mr. Pirsson, also in Broadway, saw the meteor disappear behind a high building in azimuth S. 21° W.

Mr. Bradley, also in Broadway, reports that the meteor disappeared in azimuth S. 16° W.

Several other observers agree as to the general direction of the meteor, but their statements are less precise than those of the preceding. As Mr. Bradley's observation differs materially from the others, I reject it, presuming that his memory must have been in fault, either in respect to his point of observation, or that of the meteor's disappearance. The mean of the other four estimates is S. 24½° W., or allowing for the effect of the high buildings which obstructed the view of three of the observers, the mean would be about S. 26° W. This result differs five degrees from my former estimate; a difference which is explained by my having obtained two new observations, and by my rejection of Mr. Bradley's observation.

At Washington, the apparent path of the meteor was vertical, and its point of disappearance was estimated at four degrees north of east.

A gentleman four miles west of Dover, Del., was riding towards Dover. His wife saw the meteor; he only saw the smoky trail which he describes as a nearly vertical column, with its base 20°, and its top 40° from the horizon; direction due east.

Mr. Parsons, at Salisbury, Somerset county, Md., saw the meteor descend in a slanting direction to the earth, when it exploded with a dull sound. Its direction was from the N.E.

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At Lewistown, Del., the meteor was seen to fall in the N.E. The report was heard five minutes later—loud but distant.

If we mark upon a map all the preceding directions, we find that the lines do not intersect at one point, but they indicate the most probable point of the meteor's disappearance to have been near lat. 39° 10′ and long. 75° 5′.

At New Haven, the path of the meteor was estimated to make an angle of from 25° to 35° with the vertical. Mr. Wilder Smith, near Waterbury, Ct., estimated the inclination to the vertical at about 30°.

At New York, Mr. Tatham estimated the angle with the vertical at 20°, Mr. Gould 10°, Mr. Pirsson 35°, and Mr. Bradley 45°. The mean of these four estimates is 27°.

At Washington, the path was pronounced exactly vertical. The actual path of the meteor was therefore such as, if continued, must undoubtedly have struck the earth. It must have passed vertically over the extreme southern part of New Jersey, and must have struck the earth in Delaware Bay, or near its shore.

That this conclusion is a near approximation to the truth, is confirmed by observations from the southern part of New Jersey.

Mr. Mills was surveying in the forest four miles west of Stephens Creek in Atlantic county, and heard a noise nearly overhead. He looked up and saw a cloud of a rounded form like a puff of smoke about 15 degrees south of the zenith.

At Millville, Cumberland county, a strange rumbling noise was heard somewhat resembling thunder, and one or more clouds of smoke were seen in a southeast direction at an elevation very roughly estimated at 45°.

At Newport, Cumberland county, a rumbling noise, which lasted two minutes, was heard in an east or southeast direction.

At Maurice River Cove, Cumberland county, the captains of the oyster boats saw a flash and smoke in an easterly direction.

At Dias Creek, Cape May county, Mr. Smith states that the noise was great and lasted two or three minutes. The flash was brilliant, and the smoke was seen in a northeast direction at an elevation of 75° or 80° above the horizon.

At Goshen, Cape May county, a noise was heard in a northeast direction, and a cloud of a rounded form was seen in the northeast.

At Dennisville, Cape May county, the noise appeared directly overhead. There was a small cloud or belt of white smoke left in the train of the meteor, about five degrees northwest of the zenith, the atmosphere being perfectly clear at the time. The detonations lasted somewhat over a minute.

The directions indicated in the preceding notices have a decided convergence towards a point near lat. 39° 13′, and long. 74° 52′. This result accords so nearly with that derived from observations made at a distance of a hundred miles and upwards, as to show that the observations are in the main reliable, but subject to that uncertainty which attends all estimates made without instruments, and not reduced immediately to writing. We must then conclude that this meteor passed vertically over the southern part of New Jersey, nearly on the parallel of 39° 13′, and that it struck the earth near the eastern shore of Delaware Bay, probably between Dennis Creek and Maurice River.

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I assume that this meteor was a solid body. We are acquainted with two classes of meteors quite distinct from each other and differing greatly in density. Ordinary shooting stars have never been known to reach the earth's surface, or to produce any audible noise. Another class of meteors like that of Agram in 1751 is composed chiefly of iron, and another like that of Weston, Ct. (1807), partly of iron and partly of silicates. They frequently strike the earth, or at least let fall fragments to the earth, and are attended not only by a brilliant flash of light, but by a tremendous noise. The New Jersey meteor bore a striking resemblance to the Weston meteor, not only in the brilliancy of its light, but in the noise which attended it. We cannot doubt that it was a body of considerable density; and the direction of its motion was such that the entire mass must have struck the earth. It may have sunk into Delaware Bay and not a single fragment have fallen upon dry land; but there is reason to hope that at least some fragments of it may yet be discovered. Such fragments, if they exist, are probably scattered along an east and west line coinciding nearly with the parallel of 39° 13′, and the entire mass probably lies near the meridian of 75°.

5. *Sandwich Island Meteor of Nov.* 14, 1859.—A meteor of remarkable size and brilliancy was seen from the slope of Mauna Kea north of the great volcanoe of Kilauea, S. I., soon after dark of the 14th November last, shaped like a cross, having the light of the moon at full, moving vertically south from a point a little below the zenith and disappearing near the crater.—*Pacific Com. Advertiser, Dec.* 15.

6. *Der Meteoreisenfall von Hraschina bei Agram am* 26 *Mai* 1751; von W. Haidinger. Wien, 1859.—Prof. Haidinger has here revised all the contemporary evidence respecting the fall of this remarkable meteoric iron mass, the details of which are fortunately well authenticated. This history is of great interest at the present moment when the late meteors of August and November last have called up anew the discussion of this subject.

V. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *Monthly varying level of Lake Ontario, measured, in inches, from a fixed point above the surface downwards, for fourteen years, at Charlotte, mouth of Genesee River, N. Y.:*

Year.	Jan.	Feb	Mar	Apr	May	Jun.	July	Aug	Sep	Oct.	Nov	Dec	Mean	Range.	
1846	51	54	48	45	42	39	39	42	45	45	48	45	45·2	15	Low.
1847	48	42	36	36	29	25	25	25	36	39	43	46	35·8	23	Higher.
1848	29	34	43	38	38	37	38	39	44	49	54	53	41·3	25	Lower.
1849	50	50	52	46	36	33	44	39	45	38	38	41	42·7	19	do.
1850	45	40	40	40	32	29	34	46	47	52	55	43	41·9	26	do.
1851	44	54	48	47	44	38	35	38	42	47	53	51	45·0	19	Low.
1852	50	51	48	44	26	26	22	24	30	23	38	34	34·7	29	Higher.
1853	35	32	32	25	20	14	27	20	24	28	38	39	27·8	25	Mean.
1854	39	39	38	38	27	24	25	27	36	44	48	50	36·2	26	Lower.
1855	52	53	36	40	40	36	34	36	36	34	33	33	38·6	20	do.
1856	35	35	33	31	23	18	23	30	37	46	53	53	34·7	35	Higher.
1857	54	56	46	44	35	24	19	12	14	9	24	22	30·0	47	Mean.
1858	19	13	13	18	4	6	4	2	8	12	14	16	10·9	17	Very high.
1859	20	24	16	10	6	2	8	11	17	22	28	25	15·7	26	Lower.
Mean.	41	41	38	36	29	25	27	28	33	35	40	39	34·3	25	

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1. As more water falls usually in the warmer months, the Lake is higher in those months generally than in the colder months.

2. The range has been only 54 inches, the lowest being in February, 1857, and the highest in August, 1858, and in June, 1359; the mean of the two is 27 inches.

3. In 1846 and 1857 the mean level *lowest*, and in 1858 and 1859 *highest*.

4. In 1853 the Lake was near the *mean level*, and in 1857 only a little less, though the first half of the year gave low water and the last half high.

5. The Lake was near the highest, or within four inches of it, in May, June, July and August, 1858, and in May and June, 1859, and of course the average of both years was high.

6. The Lake down to 50 inches or more in January and February, 1846; in November and December, 1848; in January, February and March, 1849; in October and November, 1850; in February, November and December, 1851; in January and February, 1852; in December, 1854; in January and February, 1855; in November and December, 1856; and in January and February, 1857.

These statements show that the changes of the level must be owing to the ordinary causes of supply or diminution of water over this great watershed, and disprove any notion of *periodic* rise and fall under any but meteoric laws. As the water was high in the Lake through 1858, it was suggested that the average fall of water must continue it high in 1859, as the measures now prove. In November, 1859, the water fell to the mean, and rose afterwards from the great autumnal rains at the west which had flowed into Lake Ontario. C. D.

2. *Eruption of Mauna Loa, Sandwich Islands*, (in a letter to Prof. DANA from Prof. R. C. HASKELL, Oahu College, dated Honolulu, Nov. 5, 1859).—Since my last dates (June 22d)* the lava continues to flow from the place of the recent eruption. With scarcely any cessation since the middle of June it has been flowing into the sea. Hawaii has been increased in area by many acres at least, by several hundred acres it is said.

After writing you from Kona in June, I visited Kilauea, which I found *very quiet*. There has, however, been considerable action since you were there in 1840, for the crater is now filled up even with the "black ledge" of which Wilkes speaks.

From Kilauea, passing through Hilo, I went to Waimeu, intending to ascend Mauna Kea, but the weather proved so rainy and foggy that I was unable to do so. From Waimeu I went direct to Kona, crossing the lava stream without difficulty on a mule, between the three mountains. The stream was fully three miles wide where I crossed, and at some points above appeared to be five or six miles wide. At this time the lava was flowing into the sea, and of course running under me as I crossed, yet the lava on the surface was in no place so hot as to burn the hoofs of the mule, or even to be noticed by myself, unless I touched my hand to it.

After arriving at Kona I went by canoe to visit the place where the lava was then and is still flowing into the sea. Without attempting to give an adequate description of the sight presented as I passed, by night, a few rods in front of the stream, which was more than a mile wide, I will only mention one fact.

* Vol. xxviii, [2], 284.

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The lava was at a light-red heat, and flowed into the sea with a velocity of two or three miles per hour. And yet this point is forty miles from the source of the stream, and at least twenty-five miles from the lowest point to which the "fissure" in Mauna Loa can possibly extend. Therefore the lava flows twenty-five miles at least, without receiving any heat from the interior of the earth, and yet is still of a light-red heat. It will be remembered, of course, that the stream is covered over with solid lava all the way from the source to within a few feet of the sea, with the exception of a small opening here and there, once in a mile perhaps.

Rev. T. Coan adds, under date of Hilo, Hawaii, Nov. 25, 1859.—"The old lake of fusion in Kilauea is slowly enlarging, and the area around it is subsiding. Probably it may in time resume its old size of half a mile in diameter. Recent visitors have found it active. On one occasion it was thought to throw up jets to the height of 70 feet.

The present eruption has now been in progress ten months, and our last advices report it still active. Several streams have fallen into the ocean along the coast of Kona. These are of different widths, and some of them are separated miles from each other. A small village, Kibele, has been covered of late with the lava, and a large and valuable fish-pond filled up. The people in Kibele pulled down their houses, and also the church, on the approach of the lava stream, and carried off the materials. Just above the church the fiery stream parted, flowed along on each side of the ground where the church had stood, reunited below it, and continued in one stream to the sea. This fact struck the Hawaiians as marvellous, and they regretted having removed their house of worship.

During the early stages of this eruption there were many splendid exhibitions along the line of flow. Canals, cataracts, lakes, fountains and jets of fusion were seen along the slope of the mountain. Forests were consumed, rocks were rent, loud and startling detonations were heard, and the heavens were shrouded with a pall of darkness. Now, and for a long season past, little or no fire is seen, except where the red lava pours into the sea. Here a broken line of fusion is seen coming out from under its self-made counterpane of hardened lava, and pouring down the face of a low and cragged precipice into the ocean, keeping up a constant boiling and sending up clouds of vapor into the air.

The central parts of Kilauea are more quiet than any other part of the crater. We have occasional earthquakes. Two shocks occurred in February, one in July, and two in November of the current year."

Book Notices.—

1. *Trübner's Bibliographical Guide to American Literature:* a classed list of books, published in the United States of America during the last forty years. With Bibliographical Introduction, Notes and Alphabetical Index. Compiled and edited by Nicholas Trübner. London, 1859. 8vo, 554 pp.—This work is beyond all question the best guide which we have to recent American literature and science. Not only is it better than all other bibliographical works of a similar scope but it is excellent in itself. Our limits permit us to mention only one of its most valuable features. Special attention has been bestowed on works in natural science, not only those which appear with an author's name, and are accordingly easy to trace, but more particularly on serial works, such as scientific journals, the transactions of learned societies, and reports of the

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state and national legislatures, which are often very difficult to discover by the ordinary apparatus of the trade. Mr. Trübner not only mentions what constitute complete sets of such works; but he enumerates the contents of the several volumes,—so that by means of his excellent index a multitude of articles and essays, often overlooked, are brought to the knowledge of every student. This book should be owned by every book-buyer. D. C. G.

2. *Manual of Public Libraries, Institutions and Societies in the United States and British Provinces of North America;* by WILLIAM J. RHEES, chief clerk of the Smithsonian Institution. 8vo, pp. 687. Lippincott. Philadelphia, 1859.—This volume contains a great amount of useful information on Public Libraries, and gives evidence of much labor in its compilation. The list of libraries in the various States extends to over three thousand titles. In a second edition the Author will be able to supply some obvious deficiencies which are inseparable from the first cast of such a work.

3. *The New American Cyclopedia: a popular Dictionary of general Knowledge;* edited by GEO. RIPLEY and CHAS. A. DANA. Vol. I—VIII. 8vo. New York and London: D. Appleton & Co.—Since our former notice of this Cyclopedia it has advanced rapidly, until now we have before us eight volumes of eight hundred pages each, the last article being on the too famous HAYNAU. Such promptness in issuing so large a mass of elaborately prepared matter speaks well not only for the energy of the publishers and the industry of its editors, but also of the public appreciation of the work. Like its predecessor, the "*Encyclopedia Americana,*" 1829–47, by Dr. LIEBER and others, it gives a satisfactory response to almost all questions coming within the range of its plan. The *New Cyclopedia*, however, besides its greater range of topics, has the advantage derived from a vast progress in many departments of knowledge, developed by a numerous corps of contributors skilled each in his own speciality.

In looking over its articles with a peculiar reference to our own departments, we are often tempted to linger among its miscellaneous topics, so rich in various and interesting information. The fine arts, religion, law, politics and war, share our attention with history and biography, ancient and modern, foreign and American, including persons still living, with sketches of events within their respective eras; geography, with the physical and picturesque features and the mineral treasures of particular countries; common and useful arts, agriculture, mechanics, and their various productions; gas lighting, gun-powder, its history, manufacture and uses; caoutchouc, gutta percha, and kindred topics of technical chemistry, with their diversified applications, and a multitude of other subjects, more or less practical and interesting to society at large.

In the American Cyclopedia the articles on science are numerous and valuable, and elevate the work to the character of a compendium of modern science. These articles are in most cases written with decided ability, and evidently by persons who are familiar with the topics which they discuss. While many of the less important subjects are presented with luminous brevity, others are more fully expanded. Among these are many topics of natural history; Chemistry is presented with its equivalents and laws of combination illustrated by many of its modern discoveries and practical applications; of the latter an example is found in the full account of the manufacture of gelatine, of beer and bricks, and

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in the ample history of gas lighting. Geology, voltaic electricity, magnetism, and other departments of pure or applied science, are treated with reasonable fulness. As a literary work the Cyclopedia is written in a pure and chaste style, and exhibits the candor and fairness which should ever adorn a record of universal knowledge. B. S.

An Introduction to Practical Pharmacy: designed as a text-book for the Student, &c.; by EDWARD PARRISH. Philadelphia. 2d edition. 246 illustrations. 8vo, pp. 720. Blanchard & Lea, 1859.—A well arranged and carefully prepared treatise, adapted to the state of this art in the United States.

Elements of Inorganic Chemistry, including the applications of the Science in the Arts; by THOS. GRAHAM, F.R.S., L. and E. Edited by HENRY WATTS, B.A., and ROBT. BRIDGES, M.D. 2d American edition in one volume. 233 woodcuts, 8vo, pp. 852. Philadelphia: Blanchard & Lea. 1858.—The American publishers issued the 480 first pages of this volume in 1852 under the editorship of Dr. Bridges. The remainder is reproduced without alteration from the English edition.

Nautical Monographs, No. I. (Washington Observatory) Oct. 1859. The winds at sea, their mean direction, and annual average duration from each of the four quarters. With four plates of diagrams of winds and calms. 4to, pp. 8. By Lt. M. F. MAURY.

Caloric: its Mechanical, Chemical and Vital Agencies in the Phenomena of Nature; by SAMUEL J. METCALF, M.D. 2 vols. 8vo, pp. 630 and 481. Philadelphia: J. B. Lippincott & Co.

Announced.

A Dictionary of English Surnames; by JOHN HENRY ALEXANDER, Esq., of Baltimore, Maryland.—The work will be comprehended in about one thousand pages, and it will be sent to press directly after the necessary commercial and technical arrangements can be made.

Little, Brown & Company, of Boston, propose to publish by subscription a series of photo-lithographic plates of the Fossil Footprints found on the Connecticut River Sandstone, prepared by the late Dr. JAMES DEANE, of Greenfield, in one volume, 4to. Price $5.00.—The work will be issued under the superintendence of T. T. BOUVE, Esq., A. A. GOULD, M.D., and HENRY I. BOWDITCH, M.D., and for the benefit of the family of Dr. Deane, and will be published in the best style, similar to Prof. Agassiz's "Contributions to the Natural History of the United States." Two hundred subscribers are required.

OBITUARY.—

Mr. GUSTAVUS WURDEMANN died on the 29th of Sept. 1859 at Swedesboro, N. J., aged 41 years. Mr. Wurdemann was employed in the U. S. Coast Survey since 1837, in the last twelve years of his life principally as a tidal and meteorological observer in Florida and the Gulf of Mexico. The observations made by him are of great value for their completeness and faithfulness. The short intervals of time left to him by the confining nature of his duties he employed with much success in collecting objects of natural history, and as he was mostly stationed on parts of our coast seldom visited by naturalists, he succeeded in obtaining several species new to science and still more which were new to the fauna of the United States. His collections are in the museums of the Smithsonian Institution and of Prof. Agassiz in Cambridge. Most of his zoological acquisitions have been already published to the world. The largest of our North American herons, *Ardea Wurdemanni*, was discovered by him.

JAMES P. ESPY, one of the most successful meteorologists of our time, died in Cincinnati, on the 24th of January, after a short illness, in the 75th year of his age. We expect to present a notice of his life in our May number.

JEAN-FRÉD.-LUDW. HAUSMANN, the eminent mineralogist, died at Göttingen, Dec. 26, 1859, aged 77 years 10 months.

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FOSSIL SHELLS FROM NEBRASKA.

Fig. 1



Fig. 2

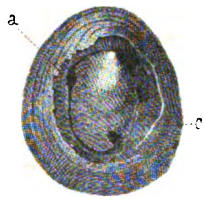


Fig. 3

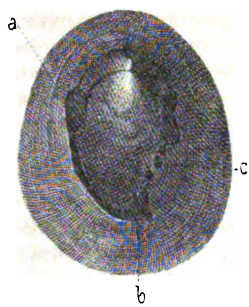


Fig 1 Anisomyon patelliformis side view.

Fig 2 Dorsal view of same, with a portion of the shell removed so as to show the muscular scar on the internal cast.

Fig 3 Internal cast of another specimen of same species, showing muscular scar

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THE

AMERICAN

JOURNAL OF SCIENCE AND ARTS.

[SECOND SERIES.]

ART. XXVI.—*On the Origination and Distribution of Vegetable Species:—Introductory Essay to the Flora of Tasmania;* by Dr. JOSEPH D. HOOKER.

(Continued from p. 25.)

§ 4. *On the General Phenomena of the Distribution of Plants in Time.*

A THIRD class of facts relates to the antiquity of vegetable forms and types on the globe, as evidenced by fossil plants. The chief facts relating to these are the following:—

31. The earliest Flora of which we know much scientifically, is that of the Carboniferous formation. We have indeed plants that belonged to an earlier vegetation, but they do not differ in any important respects from those of the carboniferous formation.

Now the ascertained features of the coal vegetation may be summed up very briefly. There existed at that time,—

Filices, in the main entirely resembling their modern representatives, and some of which may even be generically, though not specifically, identical with them.

Lycopodiaceæ; the same in the main characters as those now existing, and, though of higher specialization of stem, of greater stature, of different species, and perhaps also genera, from modern *Lycopodiaceæ*, yet identical with these in the structure of their reproductive organs and their contents, and in the minute anatomy of their tissues.

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Coniferæ. The evidence of this order is derived chiefly from the anatomical characters of the Dicotyledonous wood so abundantly found in the coal, and which seems to be identical in all important respects with the wood of modern genera of that order, to which must be added the probability of *Trigonocarpon* and *Nœggerathia* being Gymnospermous, and allied to *Salisburia*.* On the other hand, it must not be overlooked that no Coniferous strobili have been hitherto detected in the Carboniferous formation.

Cycadeæ. Some fragments of wood, presenting a striking similarity in anatomical characters to that of *Cycadeæ*, have been found in the carboniferous series.

In the absence of the fructification of *Calamites*, *Calamodendron*, *Halonia*, *Anabathra*, etc., there are no materials for any safe conclusions as to their immediate affinities, beyond that they all seem to be allied to Ferns or *Lycopodiaceæ*. But the same can hardly be said of the affinities of *Volkmannia*,† *Antholites*, and others, which have been referred, with more or less probability, to Angiospermous Dicotyledons.

The Permian Flora is for the most part specifically distinct from the Carboniferous, but many of its genera are the same. The prevalent types are Gymnospermous Dicotyledons, especially *Cycadeæ*, and a great abundance of Tree-ferns.

The New Red Sandstone, or Trias group, presents plants more analogous to those of the Oolite than to those of the Carboniferous epoch, but they have also much in common with the latter. *Voltzia*, a remarkable genus of Conifers, appears to be peculiar to this period.

In the Lias numerous species of *Cycadeæ* have been found, with various Conifers and many Ferns. No other Dicotyledonous or any Monocotyledonous plants have as yet been discovered, but it is difficult to believe that none such should have existed at a period when wood-boring and herb-devouring insects, belonging to modern genera, were extremely abundant, as has been proved by the researches of Mr. Brodie and Mr. Westwood.‡

The Oolite contains numerous *Cycadeæ*, *Coniferæ*, and Ferns, and more herbivorous genera of insects; and here Monocotyledonous vegetables are recognizable in *Podocarya* and other Pandaneous plants. A cone of *Pinus* has been discovered in the Purbeck, and one of *Araucaria* in the inferior Oolite of Somersetshire.

* Phil. Trans., 1855, p. 149.

† See Quarterly Journal of the Geological Society, May, 1854.

‡ These insects include species of the existing common European genera, *Elater*, *Gryllus*, *Hemerobius*, *Ephemera*, *Libellula*, *Panorpa*, and *Carabus*. Of all conspicuous tribes of plants the *Cycadeæ*, *Filices*, *Coniferæ*, and *Lycopodiaceæ* perhaps support the fewest insects, and the association of the above-named insects with a vegetation consisting solely or mainly of plants of these orders is quite inconceivable.

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In the Cretaceous group, Dicotyledons of a very high type appear. A good many species are enumerated* by Dr. Debey, of Aix-la-Chapelle, including a species of *Juglans*, a genus belonging to an order of highly-developed floral structure and complex affinities.†

Characeæ appear for the first time at this epoch, and are apparently wholly similar in structure to those of the present day.

The Tertiary strata present large assemblages of plants of so many existing genera and orders, that it can hardly be doubted but that even the earliest Flora of that period was almost as complex and varied as that of our own. In the lowest Eocene beds are found *Anonaceæ*, *Nipa*, *Acacia*, and *Cucurbitaceæ*.‡ In the Bagshot sands some silicified wood has been found, which may confidently be referred to *Banksia*, and which is, in fact, scarcely distinguishable from recent and fossil Australian Banksia wood.§

In the brown coal of the Eocene and Miocene periods, Fan-palms, Conifers, and various existing genera of *Myriceæ*, *Laurineæ*, and *Plataneæ* are believed to have been identified. Wesel and Weber describe from the brown coal of the Rhine a rich and varied Flora, representing numerous families never now seen associated, and including some of the peculiar and characteristic genera of the Australian, South African, American, Indian, and European floras.‖

* Quart. Journ. Geol. Soc., vii, pt. 1, misc., p. 110.

† Professor Oswald Heer, of Zurich, in an interesting little paper (Quelques Mots sur les Noyers), in Bibl. Univ. Genev., Sep. 1858, argues from the fact of the early appearance of *Juglans* in the geological series, that this genus must be a low type of the Dicotyledonous class to which it belongs. The position of *Juglans* is unsettled in the present state of our classification of Dicotyledonous orders, as it has equal claims to be ranked with *Terebinthaceæ*, which are very high in the series, and with *Cupuliferæ*, which are placed very low; and were the grounds for our thus ranking these orders based on characters of ascertained relative value, such an argument might be admissible; but the system which sunders these orders is a purely artificial one, and *Juglans* with its allies would prove it so, if other proofs were wanting; for it absolutely combines *Terebinthaceæ* and *Cupuliferæ* into one natural group, in which (as in so many others) there is a gradual passage from great complexity of floral organs to great simplicity.

‡ I am far from considering the identification of these and the other genera which I have enumerated in various strata as satisfactory, but I conclude that they may be taken as evidence of as highly developed and varied plants having then existed as are now represented by these genera.

§ I am indebted to the late Robert Brown for this fact, and for the means of comparing the specimens, which are beautifully opalized. I ascertained that he was satisfied with the evidence of this wood having really been dug up near Staines, though it is so perfectly similar in every respect to the opalized Banksia-wood of Tasmania as to suggest to his mind and my own the most serious doubts as to its English origin.

‖ See Quart. Journ. Geol. Soc., xv, misc. 3, where an abstract is given, with some excellent cautions, by C. J. F. Bunbury, Esq. The Australian genera include *Eucalyptus*, *Casuarina*, *Leptomeria*, *Templetonia*, *Banksia*, *Dryandra*, and *Hakea*. I am not prepared to assert that these identifications, or the Australian ones of the Mollasse, are all so unsatisfactory that the evidence of Australian types in the brown coal and Mollasse should be altogether set aside; but I do consider that not one of the above-named genera is identified at all satisfactorily, and that many of them are not even problematically decided.

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In the Mollasse and certain Miocene formations at Œningen and elsewhere in Germany, Switzerland, and Tuscany,* 900 species of Dicotyledons† have been observed, all apparently different from existing ones. They have been referred, with more or less probability, to Fan-palms, Poplars (three species), evergreen *Laurineæ*, *Ceratonia*, *Acacia*, *Tamarindus*, *Banksia*, *Embothrium*, *Grevillea*, *Cupressus*, several species of *Juglans* (one near the North American *J. acuminata*, another near the common walnut of Europe and Asia, *J. regia*, and a third near the North American *J. cinerea*); also a Hickory, near the *Carya alba* (a genus now wholly American), and a *Pterocarya* closely allied to *P. Caucasica*.

The rise of the Alps was subsequent to this period; and in the European deposits immediately succeeding that event, in Switzerland (at Durnten and Utznach) are found evidences of the following existing species,—Spruce, Larch, Scotch Fir, Birch, a Hazel (different from that now existing), *Scirpus lacustris*, *Phragmites communis*, and *Menyanthes trifoliata*.

The glacial epoch followed, during and since which there has probably been little generic change in the vegetation of the globe.

32. So much for the main facts hitherto regarded as established in Vegetable Palæontology. They are of little value as compared with those afforded by the Animal Kingdom, even granting that they are all well made out, which is by no means the case. In applying them theoretically to the solution of the question of creation and distribution, the first point which strikes us is the impossibility of establishing a parallel between the successive appearances of vegetable forms in time, and their complexity of structure or specialization of organs, as represented by the successively higher groups in the natural method of classification. Secondly, that the earliest recognizable Cryptogams

* During the printing of this sheet I have received from my friend M. DeCandolle a very interesting memoir on the tertiary fossil plants of Tuscany, by M. C. Gaudin and the Marquis C. Strozzi, in which some of the genera here alluded to are described. The age of these Tuscan beds is referred by Prof. O. Heer to a period intermediate between those of Utznach and Œningen. The most important plants described are, Coniferæ 6 sp., Salix 2, Liquidambar 1, Alnus 1, Carpinus 1, Populus 2, Fagus 1, Quercus 5, Ulmus 2, Planera 1, Ficus 1, Platanus 1, Oreodaphne 1, Laurus 2, Persea 1, Acer 2, Vitis 1, Juglans 4, Carya 1, Pterocarya 1. There are 49 extinct species in all, of which 46 are referred, without even a mark of doubt or caution, to existing genera, and this in almost all cases from imperfect leaves alone! Without questioning the good faith or ability of the authors of this really valuable and interesting memoir, I cannot withhold my protest against this practice of making what are at best little better than surmises, appear under the guise of scientifically established identifications. What confidence can be placed in the positive reference of supposed fossil Fungi to *Sphæria*, or of pinnated leaves to *Sapindus*, and other fragments of foliage to existing genera of *Laurineæ*, *Ficus*, and *Vitis*?

† O. Heer, Sur les Charbons feuilletés de Durnten et Utznach, in Mem. Soc. Helvet. Sc. Nat. 1857; Bibl. Univers. Genev., August, 1858.

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should not only be the highest now existing, but have more highly differentiated vegetative organs than any subsequently appearing; and that the dicotyledonous embryo and perfect exogenous wood with the highest specialized tissue known (the coniferous, with glandular tissue*), should have preceded the monocotyledonous embryo and endogenous wood in date of appearance on the globe, are facts wholly opposed to the doctrine of progression, and they can only be set aside on the supposition that they are fragmentary evidence of a time further removed from that of the origin of vegetation than from the present day; to which must be added the supposition that types of *Lycopodiaceæ*, and a number of other orders and genera, as low as those now living, existed at that time also.

33. Another point is the evidence,† said to be established, of genera now respectively considered peculiar to the five continents having existed cotemporaneously at a comparatively recent geological epoch in Europe, and the very close affinity, if not identity, of some of these with existing species. The changes in the level and contour of the different parts of the earth's surface which have occurred since the period of the chalk, or even since that preceding the rise of the Alps, imply a very great amount of difference between the past and present relations of sea and land and climate; and it is no doubt owing to these changes that the *Araucariæ*, which once inhabited England, are no longer found in the northern hemisphere, and that the Australian genera which inhabited Europe at a period preceding the rise of the Alps have since been expelled.

34. Such facts, standing at the threshold of our knowledge of vegetable palæontology, should lead us to expect that the problem of distribution is an infinitely complicated one, and suggest the idea that the mutations of the surface of our planet, which replace continents by oceans, and plains by mountains, may be insignificant measures of time when compared with the duration of some existing genera and perhaps species of plants, for some of these appear to have outlived the slow submersion of continents.

* The vexed question of the true position of Gymnospermous plants in the Natural System assumes a somewhat different aspect under the view of species being created by progressive evolution. In the haste to press the recent important discoveries in vegetable impregnation and embryogeny into the service of classification, the long-established facts regarding the development of the stem, flower, and reproductive organs themselves of Gymnospermous plants have been relatively underrated or wholly lost sight of; and if an examination of the doctrines of progression and variation lead to a better general estimation of the comparative value of the characters presented by these organs, the acceptance or rejection of the doctrines themselves is, in the present state of science, a matter of secondary importance.

† See fifth foot-note of p. 307 (‖): what I have there said of the supposed identifications of the Australian genera applies to many of those of the other enumerated quarters of the globe.

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35. From the sum then of our theories, as arranged in accordance with ascertained facts, we may make the following assumptions:—That the principal recognized families of plants which inhabited the globe at and since the Palæozoic period still exist, and therefore have as families survived all intervening geological changes. That of these types some have been transferred, or have migrated, from one hemisphere to another. That it is not unreasonable to suppose that further evidence may be forthcoming which will show that all existing species may have descended genealogically from fewer pre-existing ones; that we owe their different forms to the variation of individuals, and the power of limiting them into genera and species to the destruction of some of these varieties, etc., and the increase of individuals of others. Lastly, that the fact of species being with so much uniformity the ultimate and most definable group (the leaves as it were of the family tree), may possibly be owing to the tendency to vary being checked, partly by the ample opportunities each brood of a variety possesses of being fertilized by the pollen of its nearest counterpart, partly by the temporary stability of its surrounding physical conditions, and partly by the superabundance of seeds shed by each individual, those only vegetating which are well suited to existing conditions. An appearance of stability is also, in the case of many perennials, due to the fact that the individuals normally attain a great age,* and thus survive many generations of other species, of which generations some present characters foreign to their parents.

36. In the above line of argument I have not alluded to the question of the origin of those families of plants which appear in the earliest geological formations, nor to that of vegetable life in the abstract, conceiving these to be subjects upon which, in the present state of science, botany throws no light whatever. Regarded from the classificatory point of view, the geological history of plants is not altogether favorable to the theory of progressive development, both because the earliest ascertained types are of such high and complex organization,† and because there are no known fossil plants which we can certainly assume to be-

* In considering the relative amount and rate at which different plants vary, it should be remembered that we habitually estimate them not only loosely but falsely. We assume annuals to be more variable than perennials, but we probably greatly overrate the amount to which they really are so, because a brief personal experience enables us to study many generations of an annual under many combinations of physical conditions: whereas the same experience embraces but a fractional period of the duration of (comparatively) very few perennials. It has also been well shown by Bentham (in his paper on the British Flora, read [1858] before the Linnean Society) that an appearance of stability is given to many varieties of perennials, through their habitual increase by buds, offsets, etc., which propagate the individual; and in the case of *Rubi*, which comparatively seldom propagate by seed, a large tract of ground may be peopled by parts of a single individual.

† I have elsewhere stated that I consider the evidence of *Algæ* having existed at a period preceding vascular Cryptogams to be of very little value. (Lond. Journ. Bot., viii, p. 254.)

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long to a non-existing class or even family, or that are ascertained to be intermediate in affinity between recent classes or families.*

The progress of investigation may ultimately reveal the true history of the unrecognized vegetable remains with which our collections abound, and may discover to us amongst them new and unexpected organisms, suggesting or proving a progressive development; but in the meantime the fact remains that the prominent phenomena of vegetable palæontology do not advance us one step towards a satisfactory conception of the first origin of existing natural orders of plants.

Taking the Conifers for an example, whatever rank is given to them by the systematist, that they should have preceded Monocotyledons and many Dicotyledons in date of appearance on the globe, is a fact quite incompatible with progressive development in the scientific acceptation of the term, whilst to argue from their apparently early appearance that they are low in a classificatory system is begging the question.

Another fact to be borne in mind is, that we have no accurate idea of what systematic progression is in botany. We know little of high and low in the Vegetable Kingdom further than is expressed by the sequence of the three classes, Dicotyledons, Monocotyledons, and Acotyledons; and amongst Acotyledons, of Thallogens being lower than Acrogens, and of these that the Mosses, etc., are lower than Filices and their allies. It is true that we technically consider multiplication and complexity of floral whorls in phænogamic plants as indications of superior organization; but very many of the genera and orders most deficient in these respects are so manifestly reduced members of others, which are indisputably the most complex in organization in the whole Vegetable Kingdom, that no good classification even has been founded on these considerations alone.†

* It must not be supposed that in saying this I am even expressing a doubt as to there having been plants intermediate in affinity between existing orders and classes. Analogy with the animal kingdom suggests that some at any rate of the plants of the coal epoch do hold such a relationship; but should they not do so, I consider this fact to be of little value in the present inquiry, for I incline to believe that the ascertained geological history of plants embraces a mere fraction of their whole history.

† The subject of the retrogression of types has never yet been investigated in botany, nor its importance estimated in inquiries of this nature. To whatever order we may grant the dignity of great superiority or complexity, we find that order containing groups of species of very simple organization; these are moreover often of great size and importance, and of wide geographical distribution. Such groups, if regarded *per se*, appear to be far lower in organization than other groups which are many degrees below them in the classified series; and our only clue to their real position is their evident affinity with their complex co-ordinates;—destroy the latter by a geological or other event, and all clue to the real position of the former may be lost. Are such groups of simply-constructed species created by retrogressive variation of the higher, or did the higher proceed from them by progressive variation? If the latter, did the simpler forms precede in origin the highest forms of all other groups which rank below them in the classified series?

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37. Again, it is argued by both Mr. Darwin and Mr. Wallace that the general effects of variation by selection must be to establish a general progressive development of the whole animal kingdom. But here again in botany we are checked by the question, What is the standard of progression? Is it physiological or morphological? Is it evidenced by the power of overcoming physical obstacles to dispersion or propagation, or by a nice adaptation of structure or constitution to very restricted or complex conditions? Are cosmopolites to be regarded as superior to plants of restricted range, hermaphrodite plants to unisexual, parasites to self-sustainers, albuminous-seeded to exalbuminous, gymnosperms to angiosperms, water plants to land, trees to herbs, perennials to annuals, insular plants to continental? and, in fine, what is the significance of the multitudinous differences in point of structure and complexity, and powers of endurance, presented by the members of the Vegetable Kingdom, and which have no recognized physiological end and interpretation, nor importance in a classificatory point of view? It is extremely easy to answer any of these questions, and to support the opinion by a host of arguments, morphological, physiological, and teleological; but any one gifted with a quick perception of relations, and whose mind is stored with a sufficiency of facts, will turn every argument to equal advantage for both sides of the question.

To my mind, however, the doctrine of progression, if considered in connection with the hypothesis of the origin of species being by variation, is by far the most profound of all that have ever agitated the schools of Natural History, and I do not think that it has yet been treated in the unprejudiced spirit it demands. The elements for its study are the vastest and most complicated which the naturalist can contemplate, and reside in the comprehension of the reciprocal action of the so-called inorganic on the organic world. Granting that multiplication and specialization of organs is the evidence and measure of progression, that variation explains the *rationale* of the operation which results in this progression, the question arises, What are the limits to the combinations of physical causes which determine this progression, and how can the specializing power of Nature stop short of causing every race or variety ultimately to represent a species? While the psychological philosophers persuade us that we see the tendency to specialize pervading every attribute of organic life, mental and physical; and the physicists teach that there are limits to the amount and duration of heat, light, and every other manifestation of physical force which our senses present or our intellects perceive, and which are all in process of consumption; the reflecting botanist, knowing that his ultimate results must accord with these facts, is perplexed at feeling

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that he has failed to establish on independent evidence the doctrines of variation and progressive specialization, or to co-ordinate his attempts to do so with the successive discoveries in physical science.

38. Before dismissing this subject, I may revert once more to the opposite doctrine, which regards species as immutable creations, and this principally to observe that the arguments in its favor have neither gained nor lost by increased facilities for investigation, or by additional means for observation. The facts are unassailable that we have no direct knowledge of the origin of any wild species; that many are separated by numerous structural peculiarities from all other plants; that some of them invariably propagate their like; and that a few have retained their characters unchanged under very different conditions and through geological epochs. Recent discoveries have not weakened the force of these facts, nor have successive thinkers derived new arguments from them; and if we hence conclude from them that species are really independent creations and immutable, though so often illimitable, then is all further inquiry a waste of time, and the question of their origin; and that of their classification in Genera and orders, can, in the present state of science, never be answered; and the only known avenues to all means of investigation must be considered as closed till the origin of life itself is brought to light.

39. Of these facts the most important, and indeed the only one that affords a tangible argument, is that of genetic resemblance. To the tyro in Natural History all similar plants may have had one parent, but all dissimilar plants must have had dissimilar parents. Daily experience demonstrates the first position, but it takes years of observation to prove that the second is not always true. There are, further, certain circumstances connected with the pursuit of the sciences of observation which tend to narrow the observer's views of the attributes of species; he begins by examining a few individuals of many extremely different kinds or species, which are to him fixed ideas, and the relationships of which he only discovers by patient investigation; he then distributes them into genera, orders, and classes, the process usually being that of reducing a great number of dissimilar ideas under a few successively higher general conceptions; whilst with the history of the ideas themselves, that is, of species, he seldom concerns himself. In a study so vast as botany, it takes a long time for a naturalist to arrive at an accurate knowledge of the relations of genera and orders if he aim at being a good systematist, or to acquire an intimate knowledge of species if he aim at a proficiency in local floras, and in both these pursuits the abstract consideration of the species itself is generally lost sight of; the systematist seldom returns to it, and

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the local botanist, who finds the minutest differences to be hereditary in a limited area, applies the argument derived from genetic resemblance to every hereditarily distinct form.

40. It has been urged against the theory that existing species have arisen through the variation of pre-existing ones and the destruction of intermediate varieties, that it is a hasty inference from a few facts in the life of a few variable plants, and is therefore unworthy of confidence, if not of consideration; but it appears to me that the opposite theory, which demands an independent creative act for each species, is an equally hasty inference from a few negative facts in the life of certain species,* of which some generations have proved invariable within our extremely limited experience. These theories must not, however, be judged of solely by the force of the very few absolute facts on which they are based. There are other considerations to be taken into account, and especially the conclusions to which they lead, and their bearing upon collateral biological phenomena, under which points of view the theory of independent creations appears to me to be greatly at a disadvantage. For according to it every fact and every phenomenon regarding the origin and continuance of species, but that of their occasional variation, and their extinction by natural causes, and regarding the *rationale* of classification, is swallowed up in the gigantic conception of a power intermittently exercised in the development, out of inorganic elements, of organisms the most bulky and complex as well as the most minute and simple; and the consanguinity of each new being to its pre-existent nearest ally, is a barren fact, of no scientific significance or further importance to the naturalist than that it enables him to classify. The realization of this conception is of course impossible; the boldest speculator cannot realize the idea of a highly organized plant or animal starting into life within an area that has been the field of his own exact observation† and research; whilst the more cautious advocate hesitates about admitting the origin of the simplest organism under such circumstances, because it compels his subscribing to the doctrine of the "spontaneous generation" of living beings of every degree of complexity in structure and refinement of organization.

* See paragraph 4, where I have stated that the grand total of unstable species probably exceeds that of the stable.

† It is a curious fact (illustrative of a well-known tendency of the mind), that the few writers who have in imagination endeavored to push the doctrine of special creations to a logical issue, either place the scene of the creative effort in some unknown, distant, or isolated corner of the globe, removed far beyond the ken of scientific observation, or suppose it to have been enacted at a period when the physical conditions of the globe differed both in degree and kind from what now obtain; thus in both cases arguing *ad ignotum ab ignoto*.

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On the other hand, the advocate of creation by variation may have to stretch his imagination to account for such gaps in a homogeneous system as will resolve its members into genera, classes, and orders; but in doing so he is only expanding the principle which both theorists allow to have operated in the resolution of some groups of individuals into varieties. And if, as I have endeavored to show, all those attributes of organic life which are involved in the study of classification, representation, and distribution, and which are barren facts under the theory of special creations, may receive a rational explanation under another theory, it is to this latter that the naturalist should look for the means of penetrating the mystery which envelops the history of species,—holding himself ready to lay it down when it shall prove as useless for the further advance of science, as the long serviceable theory of special creations, founded on genetic resemblance, now appears to be.

The arguments deduced from genetic resemblance being (in the present state of science), as far as I can discover, exhausted, I have felt it my duty to re-examine the phenomena of variation in reference to the origin of existing species. These phenomena I have long studied independently of this question; and when treating either of whole floras or of species, I have made it my constant aim to demonstrate how much more important and prevalent this element of variability is than is usually admitted, as also how deep it lies beneath the foundations of all our facts and reasonings concerning classification and distribution. I have hitherto endeavored to keep my ideas upon variation in subjection to the hypothesis of species being immutable; both because a due regard to that theory checks any tendency to careless observation of minute facts, and because the opposite one is apt to lead to a precipitate conclusion that slight differences have no significance; whereas, though not of specific importance, they may be of high structural and physiological value, and hence reveal affinities that might otherwise escape us. I have already stated how greatly I am indebted to Mr. Darwin's* *rationale* of the phenomena of variation and natural selection in the production of species; and though it does not positively establish the doctrine of creation by variation, I expect that every additional fact and observation relating to species will gain great additional value from being viewed in reference to it, and that it will materially assist in developing the principles of classification and distribution.

* In this Essay I refer to the brief abstract only (Linn. Journ.) of my friend's views, not to his work now in the press, a deliberate study of which may modify my opinion on some points whereon we differ. Matured conclusions on these subjects are very slowly developed.

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[This extensive Essay proceeds to consider the Flora of Australia under § 1. General Remarks. 2. Estimate of the numbers, distribution, and affinity of the Classes, Orders, &c. 3. The Australian distribution of Natural Orders. 4. The Genera of the Australian Flora. 5. The Tropical Australian Flora. 6. The Flora of Extra-tropical Australia. 7. The Flora of Countries around Spencer's Gulf. 8. The Tasmanian Flora, an analysis of its elements and the geographical distribution of the species and their allies. 9. The New Zealand and Polynesian features of the Australian vegetation. 10. The Antarctic plants of Australia. 11. The South African features of Australian vegetation. 12. The European features of the Australian Flora. 13. On the Fossil Flora of Australia and its Geology in relation to the existing Flora. 14. On some of the naturalized Plants of Australia. 15. A List of some of the Esculent Plants of Australia. 16. Outlines of the progress of Botanical Discovery in Australia, etc.—From the large amount of matter suitable to our pages, we have space for only two of the shorter of the sections.]

General Remarks on the Flora of Australia.

The Flora of Australia has been justly regarded as the most remarkable that is known, owing to the number of peculiar forms of vegetation which that continent presents. So numerous indeed are the peculiarities of this Flora, that it has been considered as differing fundamentally, or in almost all its attributes, from those of other lands; and speculations have been entertained that its origin is either referable to another period of the world's history from that in which the existing plants of other continents have been produced, or to a separate creative effort from that which contemporaneously peopled the rest of the globe with its existing vegetation; whilst others again have supposed that the climate or some other attribute of Australia has exerted an influence on its vegetation, differing both in kind and degree from that of other climates. One of my objects in undertaking a general survey of the Australian Flora, has been to test the value of the facts which have given rise to these speculations, and to determine the extent and comparative value of a different and larger class of facts which are opposed to them, and which might also give some clue to the origin of the flora, and thus account for its peculiarities. This I pursued under the impression that it is the same with the study of whole floras as of single species or their organs, viz., that it is much easier to see peculiarities than to appreciate resemblances; and that important general characters which pervade all the members of a family or flora, are too often overlooked or undervalued, when associated with more conspicuous differences which enable us to dismember them. The result has proved, as I anticipated, that, the great difficulty being surmounted of collecting all the materials and so classifying them as to allow of their being generalized upon, the peculiarities of the flora, great

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though they be, are found to be more apparent than real, and to be due to a multitude of specialities affecting the species, and to a certain extent the genera, but not extending to the more important characteristics of the vegetation, which is not fundamentally different from that of other parts of the globe.

Before proceeding to the discussion of the elements of the Australian Flora, I shall shortly describe its general character, viewed in the double light of a peculiar vegetation and as a part of the existing flora of the globe. Its chief peculiarities are:—

That it contains more genera and species peculiar to its own area, and fewer plants belonging to other parts of the world, than any other country of equal extent. About two-fifths of its genera, and upwards of seven-eighths of its species are entirely confined to Australia.

Many of the plants have a very peculiar habit or physiognomy, giving in some cases a character to the forest scenery (as *Eucalypti*, *Acaciæ*, *Proteaceæ*, *Casuarinæ*, *Coniferæ*), or are themselves of anomalous or grotesque appearance (as *Xanthorrhœa*, *Kingia*, *Delabechea*, *Casuarina*, *Banksia*, *Dryandra*, etc.).

A great many of the species have anomalous organs, as the pitchers of *Cephalotus*, the deciduous bark and remarkable vertical leaves of the *Eucalypti*, the phyllodia of *Acacia*, the fleshy peduncle of *Exocarpus*, the inflorescence and ragged foliage of many *Proteaceæ*.

Many genera and species display singular structural peculiarities, as the ovules of *Banksia*, calyptra of *Eucalyptus*, stigma of *Goodeniaceæ*, staminal column of *Stylidium*, irritable labellum of various *Orchideæ*, flowers sunk in the wood of some *Leptospermeæ*, pericarp of *Casuarina*, receptacle and inner staminodia of *Exspomatia*, stomata of *Proteaceæ*.

On the other hand, if, disregarding the peculiarities of the flora, I compare its elements with those of the floras of similarly situated large areas of land, or with that of the whole globe, I find that there is so great an agreement between these, that it is impossible to regard Australian vegetation in any other light than as forming a peculiar, but not an aberrant or anomalous, botanical province of the existing Vegetable Kingdom. I find:—

That the relative proportions of the great classes of Monocotyledons to Dicotyledons, of genera to orders, and of species to genera, are the same as those which prevail in other floras of equal extent.

That the subclasses distinguished by a greater or less complexity of the floral envelops, or their absence, as *Thalamifloræ*, *Calyciflorœ*, *Corolliflorœ*, etc., are also in the same relative proportions as prevail in other floras.

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That the proportion of Gymnospermous plants to other Dicotyledons is not increased.

That all the Australian natural orders, with only two small exceptions, are also found in other countries; that most of those most widely diffused in Australia are such as are also the most widely distributed over the globe; and that Australia wants no known order of general distribution.

That the only two absolutely peculiar natural orders contain together only three genera, and very few species; they are, further comparatively local in Australia, and are rather aberrant forms of existing natural families than well-marked isolated groups: *Brunoniaceæ* being intermediate between *Goodeniaceæ* and *Compositæ*, and *Tremandreæ* between *Polygaleæ* and *Buettneriaceæ*.

That the large natural orders and genera, which, though not absolutely restricted to Australia, are there very abundant in species and rare elsewhere, and for which I shall hence adopt the term Australian, stand in very close relationship to groups of plants which are widely spread over the globe (as *Epacrideæ* to *Ericeæ*, *Goodeniaceæ* to *Campanulaceæ*, *Stylideæ* to *Lobeliaceæ*, *Casuarineæ* to *Myriceæ*).

That these Australian orders are exceedingly unequally distributed in Australia; that there is a greater specific difference between two quarters of Australia (southeastern and southwestern) than between Australia and the rest of the globe; and that the most marked characteristics of the flora are concentrated at that point which is geographically most remote from any other region of the globe.

That most of those Australian orders and genera which are found in other countries around Australia, have their maximum development in Australia at points approximating in geographical position towards those neighboring countries. Thus the peculiarly Indian features of the flora are most developed in northwestern Australia, the Polynesian and Malayan in northeastern, the New Zealand and South American in southeastern, and the South African in southwestern Australia.

That of the nine largest natural orders, which together include a moiety of the Australian species of flowering plants, no fewer than six belong to the nine largest natural orders of the whole world, and five belong to the largest in India also.

That in Australia itself, in advancing from the tropics to the coldest latitudes, or from the driest to the most humid districts, or from the interior to the seashore, or in ascending the mountains, the changes in vegetation are in every aspect analogous to what occur in other parts of the globe.

That the relations between the epochs of the flowering and the fruiting of plants, and the seasons of the year, are the same

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in Australia as elsewhere, and most remarkably so; the *Orchidæ* being spring flowers, the *Leguminosæ* summer, the *Compositæ* autumn, and the *Cryptogamia* winter.

That the peculiarities of the Australian flora in no way disturb the principles of natural arrangement derived from the study of the flora of the globe apart from that of Australia. For, after having attempted to consider the Australian vegetation in a classificatory point of view, shutting out of my view, as far as I could, that of other countries, I have been led to the conclusion that the authors of the Natural System—Ray, Linnæus,* and the Jussieus—might have developed the same Natural System had they worked upon Australian plants instead of upon European.

I find further, that the classes, orders, genera, and species, may be about as well (or as ill) fixed or limited by a study of their Australian members as by those of any other country similarly circumstanced; and that there is the same vagueness as to the exact limits of natural groups, a similar inequality amongst them in numerical value and botanical characters, and an analogous difficulty in forming subclasses intermediate between classes and orders, as other floras present. The Australian flora, in short, neither breaks down nor improves the Natural System of plants as a whole, though it throws great light on its parts; the Australian genera fall into their places in that system well enough, though that system was developed before Australia was known botanically, and was chiefly founded upon a study of the vegetation of its antipodes.

Thus, whether the Australian flora is viewed under the aspect of its morphology and structure, as exhibited by its natural classification, or its numerical proportions or geographical distribution, it presents essentially the same primary features as do those of the other great continents: and it hence appears to me rash to assume that its origin belongs to another epoch of the earth's

* The real merits of Linnæus as a founder of the Natural System have never been appreciated. In the well deserved admiration of the genius and labors of the Jussieus, it is forgotten that the powers displayed by Linnæus in constructing the Genera Plantarum was not less (perhaps greater) than that exercised in grouping these into those genera of a higher value, which are now called Jussieuan Orders. The history of our Natural System presents but four salient points:—I. Ray's division of all plants into Phænogams and Cryptogams, and of the former into Monocotyledons and Dicotyledons. II. Linnæus's forming natural groups called genera, and rendering a knowledge of them accessible to scientific minds by means of a binomial nomenclature and a mixed natural and artificial system of classes and orders. III. The Jussieus' combining most of the genera of Linnæus into truly natural orders, under Ray's classes, which classes they divided into subclasses as artificial as many of Linnæus's classes were. IV. The separation of Gymnosperms, by Brown, which is the first step towards a natural classification of the Jussieuan orders of Dicotyledons. (See Lond. Journ. of Bot. and Kew Gard. Misc., ix, 314, *note*.)

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history than that of other floras, when the proportions of its classes, etc., are identically the same with these; or that it should be attributed to a distinct creative effort, if this is manifested only in effecting morphological differences requisite to constitute species and genera in our classification, without disturbing the proportions of these; or that the local influence of the Australian climate should be essentially different from that of other countries, and yet effect no physiological change in the periods of flowering and fruiting, or produce any other functional disturbances of the vegetable organisms, or affect the agency of humidity, temperature, soil, and elevation, on plants.

On the Fossil Flora of Australia, and its Geology in relation to the Existing Flora.

The fossiliferous rocks of Australia do not throw much light upon the antiquity of its existing flora, because of the hiatus which geologists seem to consider exists between the palæozoic and tertiary strata of that country. Mr. Jukes* has called attention to the curious fact that this deficient series in Australia is largely developed in Europe, and there presents such Australian forms of life as marsupiate quadrupeds, *Trigonia* and other fossil shells, together with Cycadeous plants. To the latter no importance can be attached, as this order is far more characteristic of tropical America, of India, and even of southeast Africa, than of Australia; but on the other hand the *Araucaria* of the English oolite, and other fossils alluded to at p. 308, would seem to tend to confirm Mr. Jukes's observation.

The so-called Palæozoic rocks of Australia contain fossil plants of which so little, botanically, is known, that it would be rash to speculate on their affinities, even if we knew the age of the beds they are found in, as compared with the European, which we do not. Their fossils comprise ferns of several genera, including the genus *Glossopteris*, which is found in the oolite beds of England, and in India;† *Phyllotheca*, a plant somewhat similar to *Casuarina*, but of extremely doubtful affinity; *Vertebraria*, also an Indian fossil, as to the affinities of which no plausible guess has been made; *Sphenopteris* and *Zygophyllites*, of which little more can be said. To these the Rev. W. B. Clarke‡ adds the following well-known British coal fossils,—*Lepidodendron*, *Halonia*, *Sigillaria*, *Ulodendron*, *Calamites*, and *Stigmaria*.

Many of the tertiary fossil plants of Australia would seem to be very closely allied to existing ones; these include the *Casuarina* cones of Flinders Island, the *Banksia* and *Araucaria* wood

* J. B. Jukes, 'Physical Structure of Australia,' p. 89, etc.
† M'Coy in Ann. Nat. Hist., vol. xx, p. 152.
‡ Journ. Geolog. Soc. Lond., vol. iv, p. 60.

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of Tasmania, the *Banksia* cones of Victoria (which seem identical with those of *B. ericifolia*, though buried under many feet of trap). The leaves of the calcareous tuffs on the banks of the Derwent,* etc., appear however to belong to a different and warmer period.

From the above it would appear that the extinct flora of Australia was not entirely different from that now existing, and, following Mr. Jukes's line of argument, that Australia continued as dry land during the European Oolitic and Cretaceous periods. At this epoch Mr. Jukes assumes that the peculiar flora of Australia was introduced, and that the continent was again submerged during the Tertiary epoch, when it presented the appearance of two long islands, or chains of islands, one, the larger, representing the elevated land of eastern Australia and Tasmania, the other that of southeastern Australia, together with subsidiary groups in the western and northern parts of the continent.

These are the speculations of an able geologist and voyager, which I introduce without comment, and chiefly to observe that such a partition of the continent may be supposed to be favorable to the multiplication of forms of vegetable life out of fewer pre-existing ones, by the segregation of varieties. These groups of islands would present a precise analogy with the Galapagos and Sandwich groups, where we have the small islands of one Archipelago peopled by different species, and even genera. The subsequent elevation of these islets, and consequent union of them into larger ones, would further, according to Darwin's hypothesis (of the struggle of very different kinds of species and families for occupation of the soil resulting in a further separation of varieties into species), tend to enlarge the genera numerically within comparatively small geographical limits, and thus effect such a geographical distribution of plants as Australia now presents.

In our complete ignorance as to the condition of all the continents during the Palæozoic epoch, it is impossible to speculate on the earlier condition of the Australian flora. That previous to some Tertiary submersion of a great part of the continent, it was not altogether specifically different from what it now is, would appear from a fact insisted on by Mr. Jukes, that it was during such a submersion that those volcanos were active, the lavas of which now cover large tracts of southern Australia, and which we know to have buried a plant apparently identical with *Banksia ericifolia*, which is still one of the commonest trees in that part of the country: but the question of where the *Banksias* and their allies were created, and, if in other lands than

* Darwin's Journal, p. 535, and Volcanic Islands, p. 140; Strzelecki, p. 254; Milligan in Tasman. Journ., i, 131.

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Australia, how they migrated thither, we have no means of answering. If the identifications of *Banksia* and other Proteaceous leaves in the Cretaceous and Miocene formations of Europe are worthy of confidence, it is possible that the Australian types may have migrated from the northern to the southern hemisphere, as, according to Darwin's speculations, the existing European plants in Australia have.

Some arguments in favor of the antiquity of the Australian flora as compared with the European may be derived from a consideration of its generic and ordinal peculiarities. If, as I have expressed it, a genus or order is rendered peculiar, that is, unlike its allies, by the extinction of the intermediate species, it follows that the greater the peculiarity the greater the number of lapsed forms. Applying this argument to the Australian flora, we must assume an extraordinary destruction of species that once linked it with the general flora of the globe, to account for its many peculiar genera, and these being represented by so many species. But as this destruction of species is primarily due to geological causes that influence climates, and so directly and indirectly lead to the extinction of species, and as geological events are of slow progress, it follows that we must regard the Australian flora as a very ancient one. Again, Darwin argues that a rich flora or fauna, marked by a preponderance of highly developed types, must have required a large area for its development: this is because, according to his view, the principle of natural selection favors the high forms, and is unfavorable to the low. Now it could easily be shown that the Australian flora is of as high a type as any in the globe, but under existing conditions has a very small area for its development, and presents fewer representatives of other floras to contend with than most; and we must hence, under these hypotheses, assume not only the antiquity of the flora, but that it was developed in a much larger area than it now occupies.

The only other geological speculation, founded upon anything like plausible grounds, that bears upon the origin of any of the plants now inhabiting Australia, is that of Mr. Darwin in reference to the European species, to which I have alluded at p. 23. It implies of course that the existing European types were introduced into the continent long subsequently to the peculiar Australian, and are plants of a later creation. I have already pointed out the difficulties attending its adoption, the chief of which is the admission of such a cold climate in the intertropical latitudes as that not merely a temperate, but a decidedly northern flora should have migrated across them; and that this migration, if conceded, must have been extensive and have introduced very many genera and species into the tropics appears likely, when we consider the fragmentary character of the assem-

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blage of northern forms still left in Australia; for even when reduced to its most typical examples, it consists of nearly as many natural orders as species. The little colony of south Australian genera found under the equator, on Kini Balou, in Borneo, presents another difficulty, except indeed it be regarded as evidence of that previous southern migration of Australian forms from Europe to Australia, which I have just mentioned as conceivable.

There are then the Antarctic types to account for; were they of more recent introduction than the European or Australian? Darwin has alluded to the possibility of these having been transported by icebergs from higher southern latitudes, during a period of greater cold than now obtains in the southern hemisphere (as the Scandinavian and Arctic plants are supposed by Forbes to have been transported to Britain, etc., during the glacial period), and, with the north European plants already in Australia, to have ascended the mountains during the subsequent rise of temperature. This would imply that Australia was, during a cold Tertiary period, simultaneously peopled by all those Antarctic, European, and Australian types which now inhabit it, but that the latter flora was much less developed in number of species and genera than now; for I cannot but regard the Antarctic flora in the same light as the European, and as a mere fragment of a much more extensive one, whose other members perished in the battle for place waged with the European and Australian during those changes of climate and level that succeeded their first introduction. The ultimate numerical ascendancy of the Australian botanical element may have been gained during the subsequent partition of the continent into archipelagos of islands, which became so many colonies of Australian types of vegetation, prepared on the final rise of the land to descend and occupy the intermediate ground. The paucity of alpine plants of Australian genera is a fact which lends itself well to this idea; it implies that, during either the rise of land or increase of temperature, the tendency of the species of Australian type was to seek warmer regions, and that the boreal and antarctic types being better suited to a colder climate prevented to a great extent the establishment of such varieties of Australian type as might otherwise have been adapted to inhabit the same climate as themselves.

When I take a comprehensive view of the vegetation of the Old World, I am struck with the appearance it presents of there being a continuous current of vegetation (if I may so fancifully express myself) from Scandinavia to Tasmania; along, in short, the whole extent of that arc of the terrestrial sphere which presents the greatest continuity of land. In the first place, Scandinavian genera, and even species, reappear everywhere from Lap-

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land and Iceland to the tops of the Tasmanian alps, in rapidly diminishing numbers it is true, but in vigorous development throughout. They abound on the Alps and Pyrenees, pass on to the Caucasus and Himalaya, thence they extend along the Khasia mountains, and those of the peninulas of India to those of Ceylon and the Malayan archipelago (Java and Borneo), and after a hiatus of 30°, they appear on the alps of New South Wales, Victoria and Tasmania, and beyond these again on those of New Zealand and the Antarctic Islands, many of the species remaining unchanged throughout! It matters not what the vegetation of the bases and flanks of these mountains may be; the northern species may be associated with alpine forms of Germanic, Siberian, Oriental, Chinese, American, Malayan, and finally Australian and Antarctic types; but whereas these are all, more or less, local assemblages, the Scandinavian asserts his prerogative of ubiquity from Britain to beyond its antipodes.

Next in importance and appearance along the arc indicated is that flora which may be called Himalayan,* and which consists of the endemic plants of that range, with a mixture of Siberian, Caucasian, and Chinese genera; this, gathering strength in its progress southeastward along the ranges of northern and eastern India, occupies the flanks of all the mountain-chains I have enumerated between the Caucasus and Malay Islands; but there the Himalayan flora disappears, and does not reappear in Australia or New Zealand, and scarcely a trace of it is found in Polynesia.

The Malayan flora† is in many respects closely allied to the Himalayan, but is wholly tropical in character. This also very gradually appears in the valleys of the western and central Himalaya, and multiplying in genera and species in the eastern Himalaya and Khasia ranges, it sweeps down the Malayan peninsula, occupies all the Malayan Islands, and then it too stops short without entering Australia, being, however, continued eastward in tropical Polynesia..

Lastly, there is the flora of the plains and lower hills of India,‡ which is of a drier character than the Malayan, and is equally characteristic of Africa. This commences gradually in northwest India, or even in eastern Persia, and occupies all central India, the Gangetic plain, the whole of the Madras peninsula, except the western coast and mountains, the valley of the Irrawaddi, and the lower flat districts of the Malay Islands, whence it is continued in great force over the whole of tropical Australia.

* Characterized by *Cupuliferæ*, *Magnoliaceæ*, *Ternstræmiaceæ*, *Laurineæ*, *Balsamineæ*, *Ericeæ*, *Fumariaceæ*, etc.

† *Vaccineæ*, *Rhododendron*, *Begoniaceæ*, *Quercus:* and equally typified by *Cyrtandraceæ*, *Dipterocarpeæ*, *Myristiceæ*, *Anonaceæ*, *Menispermeæ*.

‡ It consists of *Acanthaceæ*, *Sterculiaceæ*, and other orders, enumerated at p. xliii, *et seq.* [in the original Essay].

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Reversing the position, and beginning at the southern extreme of this arc of vegetation, there is first the Antarctic flora (the complement of the Scandinavian), with its decided Australian representatives in *Centrolepideæ* and *Stylidieæ*, commencing in Fuegia, the Falklands, and Lord Auckland's and Campbell's group, reappearing in the alps of New Zealand, Tasmania, and Australia, and disappearing under the equator, on the alps of Borneo, being thus strictly confined to the southern hemisphere. Next there is the Australian flora proper, a large and highly developed one, diminishing rapidly after crossing the southern tropic, and as it advances towards the northwestern shore of the continent, reappearing in very small numbers in the Malay Islands, and terminated by a *Casuarina* on the east coast of the Bay of Bengal, and a *Stylidium* on the west. Not one representative of this vegetation advances further northwest.

Analogous appearances are presented by Africa and America. In Africa Indian forms prevail throughout the tropics, and, passing southwards, occupy the northern boundary of the south temperate zone; but there a very copious and widely different vegetation succeeds, of which but few representatives advance north to the tropic, and none to India, but with which are mingled Scandinavian genera and even species. In the New World, Arctic, Scandinavian, and North American genera and species are continuously extended from the north to the south temperate and even Antarctic zones; but scarcely one Antarctic species, or even* genus (*Forstera, Calceolaria, Colobanthus, Gunnera*, etc. etc.) advances north beyond the Gulf of Mexico.

These considerations quite preclude my entertaining the idea that the southern and northern floras have had common origin within comparatively modern geological epochs. On the contrary, the European and Australian floras seem to me to be essentially distinct, and not united by those in intervening countries, though fragments of the former are associated with the latter in the southern hemisphere. For instance, I regard the Indian plants in Australia to be as foreign to it, botanically, as the Scandinavian, and more so than the Antarctic; and that to whatever lengths the theory of variation may be carried, we cannot by it speculate on the southern flora being directly a derivative one from the existing northern. On the contrary, the many bonds of affinity between the three southern floras, the Antarctic, Australian, and South African, indicate that these may all have been members of one great vegetation, which may once have covered as large a southern area as the European now does a northern. It is true that at some anterior time these two

* *Acæna* is a remarkable exception.

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floras may have had a common origin, but the period of their divergence antedates the creation of the principal existing generic forms of each. To what portion of the globe the maximum development of this southern flora is to be assigned, it is vain at present to speculate; but the geographical changes that have resulted in its dismemberment into isolated groups scattered over the Southern Ocean, must have been great indeed. Circumscribed as these floras are, and encroached upon everywhere by northern forms, their ultimate destiny must depend on that power of appropriation in the strife for place which we see in the force with which an intrusive foreign weed establishes itself in our already fully peopled fields and meadows, and of the real nature of which power no conception has been formed by naturalists, and which has not even a name in the language of biology. Everywhere, however, we see the more widely distributed, and therefore least peculiar forms of plants, spreading, and the most peculiar dying out in small areas, and the progress of civilization has introduced in man a new enemy to the scarce old forms, and a strong ally of those already common. Nor can it be doubted but that many of the small local genera of Australia, New Zealand, and South Africa, will ultimately disappear, owing to the usurping tendencies of the emigrant plants of the northern hemisphere, energetically supported as they are by the artificial aids that the northern races of man afford them.

ART. XXVII.—*On the Coloring Matter of the Privet and its application in the Analysis of Potable Waters;* by Mr. JEROME NICKLÈS.

THE berries of the privet (*Ligustrum vulgare*), which are often employed in Europe to color wines, contain, besides water and ligneous matter, a portion of glucose, a waxy substance and a beautiful crimson coloring matter, which is the principal element. This matter is soluble in water, alcohol and ether; it contains no nitrogen, and is much more stable than many allied substances. When exposed to a sufficient heat it gives a black porous charcoal, but the uncharred portions remain unchanged. It was not altered by boiling for forty-eight hours with distilled water, nor by digestion during six weeks with sulphurous acid. The fixed alkalies and their neutral carbonates turn its color to green, but the red is restored by acids so that it may be employed as a delicate test in place of litmus or the coloring matter of the dahlia. With a solution of acetate of alumina it gives a violet blue liquid, from which by boiling a fine blue lake is precipitated, which is insoluble in acetic acid, but dissolves in tartaric, citric and mineral acids to a red liquid, from which alkalies

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throw down again the blue lake. The basic, and even the neutral acetate of lead, yield with the red coloring matter of the privet a blue precipitate, which is soluble in acetic acid. Ammonia readily alters this coloring matter, giving rise to a yellow substance not well defined in its character. From these observations it would appear that the red coloring principle of the berries of the privet is a substance *sui-generis* and distinct from any hitherto known. I therefore propose to designate it by the name of *liguline*.

In order to obtain liguline in a state of purity, the filtered juice of the berries was precipitated by neutral acetate of lead, and the well washed lake suspended in a small quantity of water was decomposed by sulphuretted hydrogen. The residue was then thoroughly washed by ether, in which the liguline is insoluble.* Being taken up by alcohol, and again treated by acetate of lead, sulphuretted hydrogen and ether, it might be supposed to be pure. I was, however, unable to obtain concordant results in a series of elementary analyses, the carbon of the direct lead compound varying between 21·56 and 23·00 per cent, and the hydrogen from 1·89 to 2·58.

It is probable that the process described by Mr. Glénard for the preparation of *œnoline*, the red coloring matter of wines (*An. de Chim. et de Phys.*, Dec. 1858, p. 368), would be preferable for the extraction of liguline. I accordingly applied it, but the berries having been gathered too late in the season, the coloring matter had become so far altered that my trial was unsuccessful, so that the question of the elementary composition of liguline remains unsettled.

The following further observations on this coloring matter are not without interest. It is not precipitated by gelatine, which throws down the red coloring matter of wines. With hypochlorite of lime it gives a yellow color and a yellow precipitate. With chlorid of gold, a yellow color and reduction of the metal. With chlorid of platinum, no change in the cold, but a brown color by heat. With chromate of potash a green; with bichromate brown, and with sesquichlorid, and ferroso-ferric sulphate of iron the same color. Chlorine destroys the color of liguline. The chlorids of sodium, barium and mercury, the nitrates of baryta, lead, mercury and bismuth, as also the sulphates of starch, soda, lime, zinc, manganese and cadmium are without action on the coloring matter of the privet.

The bicarbonates of lime and of the alkalies (unlike the neutral alkaline carbonates which turn it to green) give a blue color with liguline, and the same is true of the chlorids and nitrates of zinc and calcium. The colors thus obtained offer however some

* The author has previously stated that the coloring matter is soluble in ether—there is apparently some error of the copyist.—Note of the Translator.

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peculiar differences when seen by transmitted light; in this way the blue produced by a chlorid of zinc and bicarbonate of lime appears red, while it is green with the chlorid of calcium or the nitrate of lime or zinc. The blue color produced by a solution of bicarbonate of potash, on the contrary, offers no variation when thus viewed by transmitted light.

The recent juice of the berries of privet alters readily even when mixed with alcohol; its fine crimson color turns to red, and the liquid then mingled with a solution of bicarbonate of lime gives a gray instead of a blue color, and gives a dirty blue with acetate of lead. This change appears to depend upon the development of ammonia from the transformation of the azotized matter of the juice; when separated from these matters and isolated, on the contrary, liguline may be preserved without change, either in aqueous or alcoholic solution. Its color is then an intense crimson.

Even the strong mineral acids in the cold do not alter liguline, but in the presence of alkalies on the contrary, it is rapidly altered, although the red color can be, to a certain extent, restored by an acid. This alteration is dependent upon the absorption of oxygen, as may be shown by introducing a mixture of liguline and potash ley in a glass tube over mercury, when rapid absorption takes place.

The property of liguline to produce blue with solutions of bicarbonate of lime renders it a delicate reagent for the detection of this salt in potable waters. For this purpose it suffices to let fall a drop of an aqueous or alcoholic solution of liguline into the water, the crimson tint which this communicates to distilled water is replaced by a beautiful blue. In place of the solution we may employ a test paper impregnated with the coloring matter, which is best as prepared from the lead precipitate. We may, however, employ the recent juice of the berries, taking care to redden the paper slightly by exposing it to the vapor of acetic acid before drying.

As a reagent for the detection of bicarbonate of lime in waters, liguline is greatly to be preferred to a tincture of logwood, and the paper prepared with it becomes a valuable reagent for the laboratory as well as for the naturalist in the field. I have found by this reagent that while bicarbonate of lime is indicated in the springs which flow from the jurassic strata, and especially those that supply the city of Nancy, no change of color is produced by a solution of liguline with the waters of other streams which have their source in rocks destitute of calcareous matter.

The observations which I have given above were made for the most part with the fruit of the privet gathered in the autumn of 1856, and I have in fact indicated in a note in the Bulletin of the local Society of Acclimation for the North-West district

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(Nancy, 1857, p. 121). I have delayed publication in the hope to render my research more complete by a good elementary analysis, but I am now induced to publish the results already obtained that I may claim the right to continue and complete the investigations, having learned that Mr. Glenard proposes to undertake a similar research.

In conclusion, we may remark that the coloring matter of the privet offers a great analogy with that of the wines of Villefranche isolated and examined by Mr. Glenard; this analogy is shown by their composition and their properties. Its reaction with bicarbonate of lime may render it a valuable reagent in chemical analysis. The fact that it is not precipitated by gelatine, which, as is well known, throws down the red coloring matter of wines, will serve to distinguish the two when associated. It still remains to be decided whether the coloring principle of all red wines is the same, but this is a question foreign to our present subject.

ART. XXVIII.—*On the Method of Measurements, as a diagnostic means of distinguishing Human Races, adopted by Drs. Scherzer and Schwarz, in the Austrian circumnavigatory Expedition of the "Novara";* by JOSEPH BARNARD DAVIS.

WEIGHT and measure have been very frequently applied as means to determine the physical proportions of different human races, and to ascertain their essential diversities. But it may well be doubted whether they have ever been employed in that systematic and comprehensive manner, which will afford the results they are capable of yielding. Travellers have generally contented themselves by speaking in indefinite comparative terms of the people with whom they have come into contact. But few have submitted any considerable number of these people to the test of measurement, and thus ascertained their dimensions. Anthropology stands in need of many more accurate and extended observations, to derive the full results from these sources of knowledge.

The subject itself is a large one, and some have confined themselves to one branch of it, some to others. Where actual measurements have been carried out, many have contented themselves with taking the *stature* of a few, or a number, of the people; others have, besides, ascertained the length of the *limbs;* and a few have subjected the *head* to a series of superficial measurements. As we are fully assured that this latter division of

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the body is the seat of those faculties which lie at the base of all the peculiarities of human races; bearing essentially and intimately upon their manners and customs, all their institutions, their religious impulses; their capacity for civilization, and the development to which it has attained, it is not surprising that it should have attracted the chiefest attention. Besides the superficial measurements of the head, a more extensive series of observations has been made upon the bony skull itself, with a view of determining its relative proportions, for comparison in the same race, or among different races. Many observers, advancing a step nearer, have endeavored to ascertain, by measure and by weight, the *internal capacity* of this marble palace. And, lastly, some have laboriously devoted their inquiries to the great central mass of the nervous system, and availed themselves of the opportunities that have occurred to them, to determine the size and the weight of the brain, and its different parts. As this last investigation comes nearest of all to the specialties of human beings—who are so finely discriminated by Professor Owen, as *archencephala*—it is to be regretted that the occasions for research among distinct races are so few, and have been so little availed of, and the investigation itself is so elaborate and nice, that hitherto this most interesting part of anthropological anatomy is, as it were, a *tabula rasa*, to use the language of one of the most laborious inquirers in this branch of science—Prof. Huschke, of the University of Jena. It is, however, fortunate that gauging the internal capacity of the skull should afford the means of so accurate an approximation to the volume and the weight of the brain; and thus, for the comparison of these important points among the different families and tribes of men. Hence, the labors of Tiedemann, the distinguished physiologist, who, with a very amiable design, undertook to show that the brain of a negro was not smaller than that of the European—an attempt similar to that of the late Sir William Hamilton. Tiedemann might have succeeded in impressing us with his own conclusion, had he not published the tables on which this conclusion was based, and which themselves refute such an erroneous opinion. To Tiedemann succeeded Professor Morton, of Philadelphia, Professor Van der Hoeven, of Leyden, and others. Among the most recent, is Prof. Huschke, of Jena, one of whose results of whose own estimation of the capacity of the skull, and of the size of the brain, is, that the Germanic races, among whom through our Anglo-Saxon forefathers we rank, as one great branch, have the largest brains of any people. They distinctly exceed the French in this respect.

That great diversities, capable of metrical appreciation, prevail among human races is very well known. Some of the tribes of North American Indians are remarkable for their great stat-

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ure. Catlin assures us that the men among the *Crows*, whose hair will frequently reach the calves of their legs, are most of them six feet or more. Other tribes are of a decidedly lower stature. Of the gigantic Patagonians of South America, the most extravagant accounts have been given by travellers. But Capt. King affirms them, upon measurement, to be from 5 feet 10 inches to 6 feet high, which is supported by the statement of M. D'Orbigny, that some are 6 ft. 3½ inches, and the medium stature is above 5 ft. 8 inches English. On the contrary, the average height of the Bushmen is only 4 feet 4 inches. This gives a range of very nearly two feet between the tallest and the shortest races of men we are acquainted with. The other races of mankind are comprised within these limits of difference. Some tribes of the Negritos average about 4 feet 8 inches; the so-called Malay races, ascending to a mean of 5 feet 3 inches. But among the Negrito tribes of the Pacific there is, as that eminent ethnologist, Mr. Crawford, has clearly shown, a great diversity of stature. They dwell in islands scattered over a large extent of ocean, and although some tribes do not reach 5 feet in height, others, as those of New Caledonia, attain to 6 feet, and individuals among them even more. In the recent expeditions to the Andaman Islands, for the purpose of selecting a spot for a penal settlement, the inhabitants are spoken of as "dwarf Negrillos," and as "men of middle size." An individual who was measured, gave a stature of 4 feet 9½ English inches. (Selections from the Records of the Government of India, No. xxv: the Andaman Islands.) Thus, in stature alone, a very great diversity prevails. And it is remarkable that tribes in close proximity to each other frequently exhibit startling contrasts. Dr. Livingstone, whose opportunities had he been an ethnologist were so extraordinary, observed in the plains of the interior of Southern Africa, scattered among the Kafirs, who are a tall, fine and robust race, the hordes of the diminutive Bushmen. He was deeply impressed with what he saw, so contrary to all his preconceptions; and expresses his great surprise that such dissimilar races should be everywhere scattered about the country without being mingled, where they have dwelt for unlimited ages, exposed to all the same influences of air, climate, food, &c. The tall Patagonians and some tribes of the Fuegians, distinguished for their dwarf stature, afford a similar example of contrast.

The brothers Schlagintweit, following in the train of Mr. Hodgson, carried on an extensive series of metrical observations on the tribes of the Himalaya and of India. Many curious results, chiefly pointing to the different proportions of parts of the bodies and limbs of these people from those of Europeans, have been attained, which will be published in the ethnological portion of their projected work. After ascertaining the weight of

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the individual and his strength, by means of the dynamometer, they made from 25 to 28 different measurements, chiefly of the head, and of other parts of the body and limbs. But Drs. Scherzer and Schwarz have striven, by a more complex and complete system of observation and measurement, to gain an image of the size and form of the individual, and of all his parts;—thus not merely to subserve the purposes of the anatomist, the physiologist and the ethnologist, but those of the artist also. Their more ambitious object of obtaining, in this way, to a natural classification of human races, is an evidence of laudable zeal; but we can hardly hope that their labors can do more than contribute towards the solution of this difficult problem. Although, it ought to be mentioned, that the late Baron Humboldt, a short time before his death, expressed his great satisfaction with the system of measurements of Drs. Scherzer and Schwarz; by which, he thought, we may at length arrive at a safer result in distinguishing and determining human races than by any other means.

After recording the age, weight, height, strength, color of the hair and eyes, and number of the pulsations of the radial artery, they divide their measurements into three sections, those of the *head*, the *trunk*, and the *extremities;* and of these they take no less than 70 different dimensions in all, by means of different instruments.

Their external measurements of the head are the most complete that have ever been employed. They embrace the face as well as the other parts of the head, and by means of a perpendicular line with plummet, and a small metre scale, they are able to ascertain pretty correctly the profile of the countenance. The number of their different measurements of and about the *head*, consisting of superficial distances, diameters, circumferences, &c. amounts to 31, those of the *trunk* to 18, and those of the *extremities* to 21.

When the frigate "Novara" reached Sydney, these gentlemen printed an account of their system of measurements, "for private circulation" among men of science, which is preceded by a number of ingenious observations. In these, they dwell upon the ease with which travellers intuitively discriminate the different nations and tribes of mankind; and yet the difficulty in some selected individuals and cases to carry out this diagnosis, especially when the eye is deceived by a substitution of dress; and express great confidence in a more minute examination by a systematic method of measurements. They insist with equal confidence that nature must recognize a definite plan by which man's different types are formed and distinguished; and conclude that we should dedicate the same amount of study and inquiry to the systematic arrangement of our own species, as has

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long been applied to thousands of species of the vegetable and animal kingdoms.

In the course of these introductory remarks they mention their examination of the Chinese inmates of the prison at Hong Kong. Among these they found persons belonging to the *Hakka Tribe*, with stout and vigorous constitutions, fine, well-shaped, aquiline or long and straight noses, and a form of the eyes not resembling the specific obliquity of other Chinese. As criminals, they had been deprived of their tails, and Drs. Scherzer and Schwarz affirm that they had such a resemblance to the figures of some Europeans of the lower class, that, by a change of dress, they might pass amongst us without being recognized. They also mention how successfully Gützlaff, Medhurst, Huc and others have travelled the Empire in a Chinese dress without detection. And, no doubt, there are individuals so capable of assuming, and, as it were, substituting, the manners and expressions of others that the ordinary and slight attention which is paid to persons on a journey and among numbers, does not suffice to discriminate them.

Still, *the rule* must run counter to such a confusion; or the statement of the Austrian voyagers could not be true—that an anthropologist on the Island of Java is able, at first view, to classify most of the Malay tribes inhabiting the larger and smaller Islands on the Indian Archipelago, without ever mistaking. And the very remarkable account of the Abbé Huc proves that if there are differences among the races of men too subtle to be detected by the eye, yet they are not the less certainly appreciable. He informs us that he and his companion successfully eluded the detection of the unsuspecting or inattentive Chinese, but that to the Chinese *dogs* they always stood at once revealed as Europeans, by their peculiar smell. "The dogs barked continually at us, and appeared to know that we were foreigners." This is not the proper time to refer to the distinguishable odors of the different races of mankind, which travellers allude to. Huc said he could easily distinguish those of the Negro, the Malay, the Tartar, the Thibetan, the Hindoo, the Arab and the Chinese. Indeed, it is the same, with those having a delicate sense of smell, as to the French and other European races. And with respect to the fact of the penetrating and offensive scent attached to man, more especially to civilized man, Mr. Galton and others, who have traversed desert countries teeming with wild animals, give distinct and prominent testimony—which testimony is, in truth, not very complimentary to us.

We have been informed, on the authority of one who has seen much of the North American Indians, that they describe an odor to them peculiarly disgusting as being attached to the Jews. A fact, which, if correct, is little accordant with the ex-

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traordinary hypotheses which would derive the Indians themselves from the lost tribes.

Finally, it may be mentioned, that by a recent communication from Dr. Scherzer we are informed that during the cruise of the "Novara," about 200 individuals of different races, but of about the same age, males and females, were subjected to measurement. The whole number of measures taken amount to nearly 12,000. Dr. Scherzer adds, that he does not consider these observations sufficient, but merely as a commencement of a system of thorough metrical examination;—that the paper on measurements has been translated into different languages, and copies of it left in the hands of physicians, and other men of science, in the different places and islands visited by the expedition, who promised to complete the observations on the aborigines, and to forward the results to Europe;—that the measurements already effected embrace those made on Negroes, Malays, Mongols, Papuans and Indians;—and that the greatest number were taken on individuals in the Nicobar Islands, Batavia, where natives of almost all the islands of the Indian Archipelago were met with; Manilla, Hong Kong, Sydney (Austral negroes), New Zealand, Tahiti (where were aborigines of New Caledonia and Norfolk Island), Chili and Peru.

The results obtained by the extensive series of measurements thus procured, will shortly be published to the world, in the volumes now in preparation at Vienna. The History of the important Voyage of the "Novara," a popular illustrated work, from the Journals of the Commanders, Commodore Wüllustorf and Dr. Scherzer, may be expected to be issued from the Imperial printing office, in Vienna, to be followed by an English translation, in the early part of the present year. It is proposed that this shall be succeeded by a number of other volumes on distinct subjects. 1. Those on nautical, astronomical, meteorological, magnetical, and other observations relating to Physical Geography, by Commodore Wüllustorf. 2. Geology, by Dr. Hochstetter. 3. Zoology, by Messrs. Frauenfeld and Zelebor. 5. Ethnography, by Dr. Scherzer. 6. Statistics and Natural Economy, by the same. 7. Medicine, (Pathological and Pharmacognostical Researches,) by Dr. Schwarz. And, lastly, 8. an Album selected from nearly 2,500 sketches made by Mr. Sellery, the artist of the expedition. Whenever this grand programme, which will have the best wishes of men of science in all countries, shall have been completed, the rich results of the first Austrian Circumnavigatory Expedition, placed as it has been in able and well instructed hands, will, we have no doubt, vindicate the national character in a new and much nobler field of enterprise; and give to that country a far more lasting and more dignified fame than any she has hitherto required.

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Note.—Such is the inconvenience resulting from the use of a variety of metre scales, and such a number of methods of measurement, frequently taking quite different points for measures bearing the same name, as in the case of the skull especially, that the distinguished Professor Von Bær, of St. Petersburg, has just proposed a Congress of Anthropologists, to determine upon one uniform scale and to establish one system. By this means, all the results of measurements of the human body would be rendered of universal applicability.—*Nachrichten über die ethnog. craniol. Sammlung zu St. Petersburg. S.* 81.

ART. XXIX.—*Report of Assistant Charles A. Schott, on the latest results of the Discussion of the Secular Change of the Magnetic Declination, accompanied by tables showing the declination (variation of the needle) for every tenth year from the date of the earliest reliable observations, for twenty-six stations on the Atlantic, Gulf, and Pacific coasts of the United States.*

[Published in this Journal by permission of the Treasury Department, and communicated by Prof. A. D. BACHE, Superintendent U. S. Coast Survey.]

IN accordance with the Superintendent's letter of January 21, 1859, I have prepared a set of tables for practical use, giving the secular change of the magnetic declination and showing for every tenth year, from the date of the earliest reliable observations to the present time, the magnetic declination (commonly called the variation of the magnetic needle) for stations on or near the northeastern coast of the United States and also for some stations on our southern and western coasts—as derived from my several discussions of the secular change in which have been included the latest data in possession of the Coast Survey. For the eastern and southern coasts, the following papers may be referred to: Coast Survey report for 1855, Appendix No. 48, pp. 306–337; Coast Survey report for 1858, Appendix No. 25, pp. 192–195, and Appendix No. 26, pp. 195–197. For the western coast, Coast Survey report for 1856, Appendix No. 31, pp. 228–235 may be consulted.

In general the secular change of the declination appears to be of a periodic character, but in no instance has a whole cycle been completed on either coast. Its length therefore remains necessarily in a great measure uncertain, and the tentative analytical process so far followed has for its main object the proper representation of all reliable observations made at any one station, so as to furnish the means of interpolation and also to enable us to calculate the magnetic declination for any required place and date, within the limits of the discussion. In the in-

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vestigation of 1855 a linear function was used in the discussion which does not involve the duration of the period, and on this account the results were, in regard to time, of rather limited extent (see remark on p. 337 of Report for 1855). For the western coast stations, I still prefer to retain this form of the discussion. Subsequently, by means of the knowledge gained in that discussion, an attempt was made to substitute a circular function, directly involving a period or periods, the length of which, as well as all other numerical co-efficients in the formula for the secular change, has been determined by applying the method of least squares. The use of a circular function—commenced in 1858 with two stations, is now extended to eighteen, within the limits stated above, and it has been applied to some stations in Canada, the southern coast of the United States and Central America, in order to furnish material for the generalization of the law, so far as ascertained, in reference to epochs and rates of change. A secondary period within the first was traced at several stations, its length, however, being much more variable and uncertain, was found fluctuating between one-half and one-fifth of the primary period, while its amplitude was on the average fifteen times smaller than that of the primary wave for stations forming group 1, or within the geographical limits of Portland, Burlington and Williamsburg. This smaller amplitude was found nearly constant and equal to 0°·4.

To make the present paper more complete it contains also the record of all observations used in the discussion not heretofore published in the Coast Survey reports.

As long as the cause producing the secular change remains altogether unknown, it is not safe to trust too far to the continuation of the law thus empirically derived, and in the following tables no value, deduced by the formula, has been inserted antecedent to the first observation by more than ten years. The tabular values may therefore be regarded in the light of a strict interpolation between actual observations, and since the analytical treatment will equalize and remove, in a measure, accidental errors of observation, they may be considered as certainly more trustworthy than any single observation, particularly in cases where the number of observations available for the discussion exceeds half a dozen, properly distributed in relation to time. The probable error of any single representation will be found in the second table. For all ordinary use by the surveyor (or navigator) the tabular values are sufficiently precise, when greater accuracy is required the annual inequality of the declination and the diurnal variation for the time required must be taken into account; the former correction will probably not exceed, in any case, one minute, and the latter may amount in summer, in maximo, to minus or plus six minutes, and in winter to

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minus or plus three minutes, numbers which were derived from Prof. Bache's discussion of the Philadelphia observations. The tables will also answer for intermediate places, for which they furnish the necessary data of interpolation.

It is proper to state that the present formulæ should be considered as liable to future changes and improvements depending on the accumulation of additional observations, and it is hardly necessary to state that their number also may hereafter be considerably increased by the accession of new material. The utility of a publication of tables showing the declination for every tenth year was suggested by Mr. T. B. Brooks. In the numerical calculations I was assisted by Mr. G. Rumpf of the Computing Division.

Formulæ expressing the secular change of the magnetic declination (commonly called variation of the magnetic needle) used for calculating the tabular values.—Group 1. Stations between Portland, Me., and Williamsburg, Va.

A positive sign of D indicates west declination, a negative sign, east declination. n equals the number of years (and fraction of a year) from 1830; positive for years after and negative for years before this epoch. Longitudes are reckoned from Greenwich.

No.	Locality.	Lat.	Long.	
1	Burlington, Vt.	44 27	73 10	$D=+11'55-4'10\cos(1°30n+36°)+0'21\cos(7°2n+290°)$
2	Portland, Me.	43 39	70 16	$D=+10'70-2'63\cos(1°33n+87)$
3	Portsmouth, N. H.	43 05	70 43	$D=+10'20-2'45\cos(1°37n+72)$
4	Rutland, Vt.	43 36	72 55	$D=+9'89-3'66\cos(1°5n+45)$
5	Cambridge, Mass.	42 23	71 07	$D=+9'65-2'78\cos(1°30n+71)+0'22\cos(2°7n+220°)$
6	Newburyport, Ms.	42 48	70 49	$D=+9'55-2'56\cos(1°4n+78)$
7	Boston,	42 20	71 02	$D=+9'16-2'55\cos(1°39n+76)+0'22\cos(3°6n+222°)$
8	Providence, R. I.	41 50	71 24	$D=+9'11-2'99\cos(1°45n+58)+0'19\cos(7°2n+246°)$
9	Hartford, Conn.	41 46	72 40	$D=+8'60-3'59\cos(1°25n+45)$
10	New Haven, Conn.	41 17	72 55	$D=+8'13-3'49\cos(1°33n+39)$
11	Albany, N. Y.	42 39	73 43	$D=+7'65-2'74\cos(1°42n+62)$
12	Oxford, N. Y.	42 27	75 42	$D=+6'65-3'69\cos(1°3n+40)$
13	New York,	40 43	74 00	$D=+6'47-2'32\cos(1°6n+55)$
14	Philadelphia,	39 58	75 10	$D=+5'37-3'44\cos(1°6n+39)$
15	Hatboro', Pa.	40 07	75 08	$D=+5'23-3'28\cos(1°54n+47)+0'22\cos(4°1n+347°)$
16	Baltimore,	39 16	76 35	$D=+2'70-2'25\cos(1°5n+49)$
17	Washington, D. C.	38 53	77 00	$D=+2'42-2'0\cos(1°5n+49)$
18	Williamsburg, Va.	37 15	76 40	$D=+2'22-2'6\cos(1°5n+22)$

The following table contains the number (n) of observations (single or combined) upon which each formula is based; the probable error (E_o) of an observation expressed in minutes, as a measure of the degree of accuracy with which the observations are represented; the epoch of the last minimum of west declination (or of maximum east declination) together with the least west declination (greatest east), and lastly, the annual variation

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for the years 1840, 1850, and 1860, expressed in minutes. The positive sign expresses west declination increasing (east diminishing).

Locality.	n	E_0	Epoch of min. W. decl'n.	Least W. decl'n.	Annual change.		
					▿1840	▿1850	▿1860
		′		°	′	′	′
Burlington, Vt.	9	± 5	1813	+7·4	+4·1	+3·4	+4·5
Portland, Me.	5	14	1765	+8·1	+3·6	+3·4	+3·0
Portsmouth, N. H.	4	10	1777	+7·7	+3·5	+3 5	+3·2
Rutland, Vt.	4	18	1800	+6·2	+4·9	+5·5	+5·7
Cambridge, Mass.	22	12	1782	+6·9	+4·3	+4·3	....
Newburyport, Mass.	4	12	1774	+7·0	+3·7	+2·6	+3·3
Boston,	8	10	1782	+6·7	+4·5	+4·3	+3·7
Providence, R. I.	30	5	1779	+6·1	+5·3	+3·3	+3·0
Hartford, Conn.	6	14	1794	+5·0	+4·0	+4·4	+4·6
New Haven,	14	10	1801	+4·6	+3·8	+4·4	+4·7
Albany, N. Y.	10	3	1787	+4·9	+3·9	+4·0	+3·9
Oxford, N. Y.	10	11	1799	+3·0	+4·0	+4·6	+4·9
New York,	13	13	1795	+4·1	+3·7	+3·9	+3·8
Philadelphia,	11	16	1805	+1·9	+4·7	+5·3	+5·4
Hatboro, Pa.	18	5	1796	+1·8	+4·2	+4·3	+4·4
Baltimore,	3	13	1798	+0·5	+3·2	+3·4	+3·4
Washington, D. C.	6	8	1798	+0·4	+2·8	+3·1	+3·1
Williamsburg, Va.	3	15	1815	-0·4	+2·4	+3·2	+3·7

Table of magnetic declinations for eighteen stations forming group 1, *on or near the northeastern coast of the United States, between the years* 1680 *and* 1860. *West declination is indicated by a plus sign, east declination by a minus sign, and is expressed in degrees and fractions of a degree.*

Year.	Burlington, Vt.	Portland, Me.	Portsmouth, N. H.	Rutland, Vt.	Cambridge, Mass.	Newburyport, Mass.	Boston.	Providence, R. I.	Hartford, Conn.
	°	°	°	°	°	°	°	°	°
1680	.....	.....	.....	.....	.....	.....	.....	.....	.....
1690	.....	.....	.....	.....	.....	.....	.....	.....	.....
1700	.....	.....	.....	.....	+ 9·9	.....	+ 9·7	.....	.....
1710	.....	.....	.....	.....	9·4	.....	9·0	+10·4	.....
1720	.....	.....	.....	.....	8·8	.....	8·3	9·5	.....
1730	.....	.....	.....	.....	8·4	.....	7·8	8·9	.....
1740	.....	.....	.....	.....	7·9	.....	7·4	8·3	.....
1750	.....	.....	.....	.....	7·5	.....	7·2	7·7	.....
1760	.....	+ 8·1	.....	.....	7·2	.....	7·0	6·9	.....
1770	.....	8·1	+ 7·8	.....	7·0	+ 7·0	6·8	6·3	.....
1780	.....	8·3	7·7	.....	6·9	7·0	6·8	6·1	+5·2
1790	+ 7·8	8·5	7·9	+6·3	6·9	7·2	6·8	6·3	5·0
1800	7·5	8·9	8·1	6·2	7·1	7·5	7·0	6·4	5·0
1810	7·3	9·4	8·5	6·3	7·5	7·9	7·3	6·5	5·2
1820	7·6	10·0	8·9	6·7	8·0	8·4	7·8	6·8	5·6
1830	8·30	10·6	9·4	7·3	8·58	9·0	8·41	7·46	6·1
1840	9·07	11·2	10·0	8·1	9·28	9·6	9·13	8·38	6·7
1850	9·69	11·8	10·6	8·9	10·0	10·3	9·88	9·14	7·4
1860	+10·30	+12·3	+11·2	+9·9	+.....	+10·8	+10·56	+ 9·68	+8·1

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Year.	New Haven, Conn.	Albany, N.Y.	Oxford, N.Y.	New York.	Philadelphia.	Hatboro', Pa.	Baltimore.	Washington, D.C.	Williamsburg, Va.
	°	°	°	°	°	°	°	°	°
1680	.....	.....	.....	+8·8	.....	+8·5	.....	.....	.....
1690	.....	.....	.....	8·7	.....	8·3	.....	.....	+4·8
1700	.....	.....	.....	8·5	+8·8	7·9	.....	.....	+4·8
1710	.....	.....	.....	8·0	8·4	7·5	.....	.....	.....
1720	.....	.....	.....	7·6	7·9	7·0	.....	.....	.....
1730	.....	.....	.....	7·0	7·1	6·3	.....	.....	.....
1740	.....	.....	.....	6·4	6·3	5·6	.....	.....	.....
1750	.....	.....	.....	5·8	5·3	4·7	.....	.....	.....
1760	+6·1	.....	.....	5·2	4·4	3·8	.....	.....	.....
1770	5·5	.....	.....	4·7	3·5	2·9	.....	.....	+1·2
1780	5·0	.....	.....	4·4	2·8	2·2	.....	.....	+0·7
1790	4·8	.....	+3·0	4·2	2·2	1·8	.....	.....	+0·2
1800	4·6	.....	3·0	4·2	2·0	1·8	.....	+0·4	-0·2
1810	4·7	+5·4	3·1	4·3	1·9	2·1	+0·6	0·5	-0·4
1820	5·0	5·8	3·4	4·7	2·2	2·6	0·8	0·8	-0·4
1830	5·42	6·3	3·82	5·16	2·70	3·20	1·2	1·1	-0·2
1840	5·98	7·0	4·43	5·73	3·41	3·89	1·7	1·5	+0·1
1850	6·71	7·7	5·15	6·37	4·25	4·61	2·4	2·0	+0·6
1860	+7·46	+8·3	+5·95	+7·01	+5·19	+5·32	+2·9	+2·6	+1·2

Note.—At Cambridge, Mass., the observations after 1855 require farther examination. At Williamsburg, the values between 1700 and 1770 were not considered sufficiently reliable for insertion. The expression for Baltimore depends for length of period and time of minimum on the Washington formula.

The total number of observations upon which the tabular values and the formulæ are based is 180, the average number of any one station is 10, and the average probable error of any single representation is $\pm 11'$.

If we arrange the stations geographically, we find that at the eastern stations the minimum (west) declination occurred earlier than at the more western and southern stations; thus, from six stations between Portland and Providence it occurred about the year 1777; in the Connecticut and Hudson valleys and along the sea-coast as far south as Washington, the year of the minimum does not differ much from 1797; Williamsburg in Virginia gives 1815. The transition as we pass from the New England states is somewhat abrupt, but too well marked to be accidental. Extending the investigation farther north, I find for Quebec, Canada, the year of the minimum 1769; going farther west we find that at Toronto it must have occurred before the year 1842, and at York Fort, Hudson Bay, I find the year 1842 (as already ascertained by General Sabine, after the receipt of Capt. Blakiston's observations of 1857). This latter station is nearly halfway across the continent, and if we proceed to the western coast we find that the eastern declination there has not yet reached its maximum (equivalent to a western minimum), but it is highly probable that it will reach it before the close of the

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present century. The present reverse or western motion of the isogonic lines in our eastern states which commenced about the year 1777 will gradually be communicated to the more westerly stations, and will, it is highly probable, be participated in our western coast before or at the close of the present century, the direction of the motion in this latter locality being at present still to the eastward and southward, though with a diminishing rate (see p. 235 of C. S. report of 1856).

The following equations constructed for the two northernmost stations may be added here:

York Fort, Hudson Bay, $D=+5°{\cdot}1-14°{\cdot}2\cos(1°{\cdot}6n+340°)$
Quebec, Canada, $D=+12\cdot84-3\cdot7\cos(1\cdot6n+97)$

The second group comprises the stations on the southern portion of the Atlantic coast and Gulf coast; only three in number, to which have been added some stations located further south.

Group II. *Southern Stations.*

No.	Locality.	Lat.	Long.	Magnetic declination.
1	Charleston, S. C.	32° 45′	79° 51′	$D=-2°{\cdot}12-2°{\cdot}02\cos(1°{\cdot}55n+56°)$
2	Savannah, Ga.	32 05	81 05	$D=-2\cdot95-1\cdot24\cos(1\cdot5n+20)$
3	Mobile, Ala.	30 41	88 02	$D=-6\cdot5-0\cdot77\cos(1\cdot6n+16)$

Locality.	n	E_0	Epoch of max. east declination.	Max. east declination	Annual change.		
					1840	1850	1860
Charleston, S. C.	5	± 9′	1794	−4°·1	+3′·1	+3′·2	+3′·2
Savannah, Ga.	4	12	1817	−4·2	+1·1	+1·5	+1·8
Mobile, Ala.	6	12	1820	−7·3	+0·7	+0·9	+1·1

Proceeding in a southerly direction the next station discussed outside of the boundaries of the United States is Havana, Cuba, lat. 23° 09′, long. 82° 22′, for which place I found $D=-4°{\cdot}82-1{\cdot}45\cos(1{\cdot}3n+26°)$ with 1810 as the year of maximum east declination. The values collected for Jamaica were not discussed, but the nine values I was able to obtain, will be found in the appended record. For Panama, New Granada, lat. +8° 57′, long. 79° 29′, the southernmost station discussed, I find $D=-6°{\cdot}9-1°{\cdot}04\cos(1{\cdot}2n+74°)$, an equation satisfying the observations, but not considered as preferable to the following expression, $D=-5°{\cdot}57-2°{\cdot}21\cos(1{\cdot}2n+34°)$, which supposes the maximum to occur in 1802.

Going westward and northward I found for Vera Cruz, Mexico, lat. 19° 12′, long. 96° 09′, $D=-4°{\cdot}2-5{\cdot}04\cos(1{\cdot}1n+7°)$, with the maximum east declination in 1824.

The following table has been calculated from the preceding equations:

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Year.	Charleston, S. C.	Savannah, Ga.	Mobile, Ala.
	°	°	°
1770	−3·7	....	....
1780	−4·0	....	....
1790	−4·1	....	....
1800	−4·1	−4·1	−7·1
1810	−4·0	−4·2	−7·2
1820	−3·6	−4·2	−7·3
1830	−3·2	−4·1	−7·2
1840	−2·8	−4·0	−7·1
1850	−2·2	−3·7	−7·0
1860	−1·7	−3·5	−6·8

The following formulæ for stations of the western coast, between San Diego and Cape Disappointment, forming group 8, have been copied from p. 234 of the report for 1856.

No.	Locality.	Lat.	[Long.]	
		° ′	° ′	
1	San Diego,	32 42	117 18	$D = -12°17 - 0·019n + 0·00018n^2$
2	Monterey,	36 38	121 54	$D = -14·19 - 0·050n + 0·00047n^2$
3	San Francisco,	37 48	122 27	$D = -15·14 - 0·028n + 0·00025n^2$
4	Cape Mendocino,	40 25	124 22	$D = -16·29 - 0·029n$
5	Cape Disappointment,	46 17	124 02	$D = -19·65 - 0·019n$

The total number of observations used for the construction of the above formulæ is 21, the greatest number for any one station being 6, the least 3, the average probable error of any single representation is ±12′. The annual change (increasing east declination) may be taken the same for all stations, viz., in 1840, −1′·6, in 1850, −1′·2, in 1860, −0′·8.

Year.	San Diego.	Monterey.	San Francisco.	Cape Mendocino.	Cape Disappointment.
	°	°	°	°	°
1790	−11·1	−11·4	−13·6	−15·1	−18·9
1800	11·4	12·3	14·1	15·4	19·1
1810	11·7	13·0	14·5	15·7	19·3
1820	12·0	13·6	14·8	16·0	19·5
1830	12·2	14·2	15·1	16·3	19·7
1840	12·3	14·6	15·4	16·6	19·8
1850	12·5	15·0	15·6	16·9	20·0
1860	−12·6	−15·3	−15·8	−17·2	−20·2

The next station discussed, south of California, is San Blas, Mexico, lat. 21° 32′ N., long. 105° 16′ W. of Gr., which gave the following expression (see p. 234, C. S. report for 1856),

$$D = -8°·63 - 0·042n - 0·00031n^2,$$

which equation, when compared with those above, shows a reversal in the sign of the coefficient of n^2 or an opposite curvature. The annual easterly increase at San Blas in 1850 according to the above formula was 3′·3. This station, however, is already within the area of the peculiar form of the isogonic lines, which position may possibly render an immediate compar-

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ison impracticable. The station Sitka, in Russian America, is the next place discussed north of Washington Territory. I find for it the approximate formula,

$$D = -28°{\cdot}12 - 0{\cdot}0607n - 0{\cdot}00025n^2.$$

It depends for its latest declination (1858) on the tabular value assigned by Mr. Evans on his late map of the lines of equal magnetic variation reduced to 1858.

Record of all observed declinations made use of in the above paper, not heretofore published in the U. S. Coast Survey reports.

The following record, containing only additional observations, we have to consult the preceding reports of 1854,* 1855–'56, and '58, if we desire to collect all results which may have been used at any one station. The stations are arranged geographically, commencing with the northern and eastern stations and concluding with the stations on the western coast. D= observed declination.

York Fort, Hudson Bay.

From the Proceedings of the Royal Society of London for January 7, 1858, by Major Gen. Sabine.

1725,	D=19° 00′ W.	Capt. Middleton.
1787,	5 00 "	Hansteen's map.
1819, Sept.,	6 00 E.	Sir J. Franklin.
1843, July,	9 25 "	Capt. Lefroy.
1857, Aug.,	7 37 "	Capt. Blakiston.

Quebec, Canada.

1649,	D=16° 00′ W.	P. Bressau, Hansteen's Erdmag's, Barlow Cycl. Met.
1686,	15 30 "	DeHayes, " " "
1810,	11 00 "	Becquerel, traité du magnetisme.
1814,	11 50 "	Kent, Becquerel traité du magnetisme.
1831,	13 38 "	Bayfield, " ". " "
1842,	14 12 "	Capt. Lefroy.
1859, July,	16 17 "	Chas. A. Schott, Asst. U. S. Coast Survey.

Burlington, Vt.

See former observations in 1855 report, pp. 326–337.

1837,	D= 8° 45′ W.	Prof. Benedict.
1840,	9 42 "	J. Johnson, Thompson's Hist. of Vermont.
1845, June,	9 22 "	Dr. J. Locke, Smiths. Contrib. to Knowledge, vol. iii, 1852.

Portland, Me.

1763,	D= 7° 45′ W.	J. Winthrop, Sill.'s Journal, xxxiv, 1838, Prof. Loomis's collection.
1775,	8 30 "	J. F. DeBarre's Atlantic Neptune, London, 1781.

* The table of the declinations in that report is reprinted and enlarged in the report of 1855.

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1845, June,		11	28 "	Dr. J. Locke, Smiths. Contrib. to Knowledge, vol. iii, 1852.
1859, July,		12	20 "	Chas. A. Schott, Asst. U. S. Coast Survey.

See also C. S. report of 1856, p. 215.

Portsmouth, N. H.

1771,	*D*=	7°	46′ W.	Holland, Sill.'s Journal, xxxiv, 1838, Prof. Loomis's collection.
1771,		7	48 "	Holland.
1775,		7	45 "	J. F. DeBarre's Atlantic Neptune.
1859, July,		11	15 "	Chas. A. Schott, Asst. U. S. Coast Survey.

See also C. S. report of 1856, p. 215.

Rutland, Vt.

1789, April,	*D*=	7°	03′ W.	Dr. Williams, Sill.'s Journal, xvi, 1829.
1810, May,		6	04 "	" " " " " "
1811, Sept.,		6	01 "	" " " " " "
1859, July,		9	49 "	Chas. A. Schott, Asst. U. S. Coast Survey.

Cambridge, Mass.

See pp. 317–318 of C. S. report of 1855, also C. S. report, 1856, p. 222.

1845, June,	*D*=	9°	32′ W.	Dr. J. Locke, Smiths. Contrib. to Knowledge, vol. iii, 1852.
1855, May,		10	54·6 "	W. C. Bond (in a letter to Supt. of C. S.)
1856, May,		10	50·3 "	" " " "
1856, July,		10	06 "	Karl Friesach, Imp. Acad. of Sciences, Vienna, vol. xxix, 1858.

Note.—More recent observations still require examination.

Newburyport, Mass.

1775,	*D*=	6°	45′ W.	J. F. W. DeBarre's Atlantic Neptune.
1781,		7	18 "	Dr. Williams, Sill.'s Journal, xxxiv, 1838, Prof. Loomis's collection.
1859, July,		10	58 "	Chas. A. Schott, Asst. U. S. Coast Survey.

See also C. S. report, 1856, p. 215.

Boston, Mass.

See C. S. report, 1855, pp. 316, 317–337.

Providence, R. I.

See C. S. report, 1855, pp. 307, 308, 309, 337.

Hartford, Conn.

1786,	*D*=	5°	25′ W.	Dr. Williams,	Prof. Loomis's collection in Sill.'s Journal, vol. xxxiv, 1838.
1810,		4	46 "	Asher Miller,	
1824,		5	45 "	N. Goodwin,	
1828,		6	03 "	"	
1829,		6	03 "	"	
1859, July,		8	04 "	An interpolated value from observations at Springfield and New Haven in 1859 and 1855.	

New Haven, Conn.

See C. S. report, 1855, pp. 319, 320, 337.

Albany, N. Y.

1847, Nov.,	*D*=	7°	35′ W.	Regent's report (geological survey).

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1856, Sept.,		8 35	"	Karl Friesach, Imp. Acad. of Sciences, Vienna, vol. xxix, 1858.

See also C. S. report, 1855, pp. 328–337, and C. S. report, 1858, p. 191.

Oxford, N. Y.

The following observations marked E. B. W. C. are from a letter of Mr. E. B. W. Call to the Superintendent C. S. Dec. 22, 1858.

1792–95,	$D=$	3° 00′	W.	E. B. W. C.
1817,		3 00	"	"
1828, July,		4 30	"	"
1834, Oct.,		3 52	"	Regent's report, Sill.'s Jour., xxxiv, 1838.
1836, Oct.,		4 09	"	" " " " "
1838, July,		4 30	"	" " observed at Guilford.
1849, Nov.,		5 11	"	E. B. W. C.
1857, April,		5 44	"	"
1858, Feb.,		5 47	"	"
1858, Dec.,		5 50	"	"

New York.

See C. S. report of 1855, pp. 320, 321, 333, and 337, also C. S. report, 1856, p. 217.

Philadelphia.

See C. S. report of 1855, pp. 313, 314, and 337.

Hatboro', Pa.

See C. S. report of 1858, pp. 192, 193, 194, and 195.

Baltimore, Md.

1808,	$D=$	0° 10′ to 15′	W.	D. Byrnes, vol. xviii, 1830, Sill.'s Journal.

See also C. S. report, 1856, pp. 219, 227, also C. S. report, 1858, p. 191.

Washington, D. C.

See C. S. report, 1858, pp. 195, 196, 197.

Williamsburg, Va.

1694,	$D=$	5° 00′	W.	Sill.'s Journal, vol. xxxiv, 1838, Prof. Loomis's collection.
1780,		0 50	"	" " " " " "
1809,		0 33	E.	" " " " " "
1856, Aug.,		1 04	W.	Deduced from observations at Petersburg, Old Point Comfort, and Norfolk.

Charleston, S. C.

1857, April,	$D=$	1° 56′	E.	Derived from observations at Savannah in 1852 and 1857.

See C. S. report, 1855, pp. 322, 323.

Savannah, Ga.

1817,	$D=$	4° 00′	E.	Becquerel traité du magnetisme.
1838,		5 05	"	Silliman's Journal, xxxiv, 1840.
1839,		3 31	"	" " " " "

See also C. S. report, 1856, p. 220, and C. S. report, 1858, p. 192.

Mobile, Ala.

See C. S. report, 1855, p. 323, also C. S. report, 1858, p. 192.

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Havana, Cuba.

See C. S. report, 1855, p. 324.

1857, Jan.,	$D=$ 5° 15′ E.	Karl Friesach, Imp. Acad. of Sciences, Vienna, vol. xxix, 1858.

Jamaica, W. Indies.

1732,	$D=$ 6° to 6° 05′ E.	J. Harris at Black river, in March and April. Phil. Trans., 1733.
1789–93,	6 50 "	J. Leard, map of Port Royal.
1791–92,	6 45 "	" " "
1819,	4 50 "	DeMackau, Becquerel's traité du magnetisme, Paris, 1846.
1821,	4 50 "	DeMayne, " "
1822,	4 54 "	Owen, " "
1832,	5 13 "	Foster, " "
1833?	4 40 "	From a map.
1840?	4 00 "	Gen. Sabine's isogonic map of the Atlantic Ocean.
1857, March,	3 40 "	Karl Friesach, Imp. Acad. of Sciences, Vienna, vol. xxix, 1858.

Panama, New Granada.

1775, Nov.,	$D=$ 7° 49′ E.	Encycl. Brit.
1791, Dec.,	7 49 "	" "
1802,	8 00 "	" "
1822,	7 00 "	Hall, Becquerel's traité du magnetisme.
1837,	7 02 "	Sir E. Belcher.
1849,	6 55 "	Major Emory (Mexican Bound. Survey).

See also C. S. report, 1856, p. 223.

Vera Cruz, Mexico.

1726–27,	$D=$ 2° 15′ E.	J. Harris, Phil. Trans. R. S. anno 1728.
1769,	6 40 "	Encyc. Brit., 7th edition, 1842.
1769, March,	6 28 "	"
1776,	7 30 "	Don Ulloa, Encyc. Brit.
1815,	10 37 "	Malony.
1819, April,	9 16 "	Wise.

See also C. S. report, 1856, p. 214.

San Diego, Monterey, San Francisco and Cape Mendocino, California, and for Cape Disappointment, Washington Territory, see C. S. report, 1856, pp. 228 to 235.

Sitka, Russian America.

1804,	$D=$ 26° 45′ E.	Lissiansky,
1824,	27 30 "	Kotzebue, } Becquerel's traité du magnetisme.
1829,	28 19 "	Erman,
1858,	30 00 "	From Evans' map of isogonic lines for 1858.

Washington, D. C.

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Art. XXX.—*Caricography;* by Prof. C. Dewey.

(Continued from vol. xxviii, p. 282, Second Series.)

No. 260. *Carex argyrantha*, Tuckerman.

Spica composita; spiculis 4–8, ovato-rotundis vel obovatis sub-approximatis alternis albis, inferiore subremota, supernè staminiferis, squamosi-bracteatis, distigmaticis; fructibus ovatis compressis erectis vel sub-patulis nervosis margine membranaceo-alatis viridibus acuminato-rostratis brevi-bifidis, squamam membranaceam albam lanceolatam aequantibus.

Culm 1½–3 feet high, smooth, lax, reclining, and twice longer than the leaves; spikelets nearer above and white; fruit margined or winged widely for its length and width; light green.

Rocky Woods, Amherst and Sunderland, Mass.; *Prof. Tuckerman*, by whom the plant and his description have been kindly presented. It is related to *C. Deweyana*, and the white-glumed family, silver-flowered, according to its name, but appears to be new and distinct; discovered the last season.

Note. *C. Rugeliana*, Kunze, suppl. to Schkuhr, No. 56, p. 189, is *C. aestivalis*, Curtis, according to Boott, Illust., p. 54, No. 133. *C. miser*, Buckley, in this Journal, vol. xlv, p. 173, and vol. xlviii, p. 140, is considered by Dr. Boott to be *C. juncea*, Willd. System. Veg., 1826, No. 226. See Boott, Lin. Trans. vol. xx, p. 116, and Illust., p. 55. The descriptions of Willd. & Kunze sustain this conclusion of Dr. Boott.

261. *C. paludosa*, Goodenough. Schk. fig. 103.

Spicis pistilliferis, 2–4, sæpe 3, cylindraceis erectis oblongis, arctè-floriferis sub-approximatis, superioribus sessilibus, inferiore sæpe longo-pedunculata vix vaginata inferne attenuata et hinc sublaxiflora, alternatis et foliaceo-bracteatis; perigyniis (fructibus) ovatis in breve rostrum bidentatum attenuatis vel ovalibus acuminatis brevi-rostratis distinctè et multi-nervosis subcompressis subglabris tristigmaticis, squamam angustam lanceolatam aequantibus vel lanceolata cuspidata brevioribus.

Culm 1½–2 feet high, erect, triquetrous, scabrous above, longer than the rough-edged leaves, with leafy bracts equal to or surpassing the culm. Varies, like the European plant, in the length and thickness of its spikes, and in its glumes or scales.

Near Boston—*Wm. Boott, Esq.*, in 1859, probably introduced not many years since. Common in England, Germany, and Sweden.

262. *C. monile*, Tuckerman. Boott, Illust., No. 71.

Spicis staminiferis, 2–4, longis, cylindraceis, gracilibus cum squama longo-lanceolata; pistilliferis 2, rarò 1, oblongo-cylindraceis, subremotis, brevi-pedunculatis, sub-densifloris, infima interdum basin attenuata sub-nutante, foliaceo-bracteatis vix vaginatis; fructibus globosis vel ellipsoideis inflatis brevi-rostratis bidentatis glabris multi-nervatis stramineis cum ore rostri sub-obliquo, squama angusta oblonga lanceolata sub-duplo longi-oribus.

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Culm 15-30 inches high, erect, triquetrous, longer than the leaves; bracts surpassing the culm, bright yellowish green.

In marshes, not abundant, N. England—*Tuckerman;* Rhode Island—*Olney;* New Jersey, Ohio and westward.

Note 1. The Carex, No. 197, vol. xlix, p. 47 of this Journal, accidentally misnamed, is *C. Vaseyi*, Dew., of which No. 197 contains the accurate description. From the preceding, *C. monile*, Tuck., the fruit is very different, being ovate, long-conic, subtriquetrous-inflated, glabrous and scabro-rostrate. *C. Vaseyi* is the plant referred to by Dr. Boott, Illust., No. 71, as different from *C. vesicaria* and *C. monile*, but not named by him among those sent him from Penn Yan, N. Y., by Dr. Sartwell. The reference of No. 197 above to No. 71 in the Illust. is, of course, an oversight. The correction on 197 is *C. Vaseyi*, Dew.

Note 2. *C. bullata*, Schk., Fig. 166, described in this Journal, vol. ix p. 71, 1825, from living specimens, and named from its (ball-shaped) nearly globose fruit, seems to me, as it does to Carey in Gray's Manual, distinct from *C. cylindrica*, Schw., and from the following number.

263. *C. physema*, Dew. *C. bullata*, Boott, Illust., No. 71, and Carey (non Schk).

Spicis staminiferis, 2–3, cylindraceis, gracilibus, contiguis, infima bracteata; pistillifera 1, interdum 2, subrotunda vel oblongo-cylindracea perdensiflora et crassa remota et subfulva, infima pedunculata et subnutante vel erecta longi-foliacea bracteata; fructibus turgidi-ovatis longi-cylindraceis rostratis bifurcatisque, inflatis glabris et scabro-dentatis vel serratis, squama lanceolata acuta albi-marginata longioribus et latioribus.

Culm 1–2 foot high or more, firm, slender for the thickness of pistillate spike, stiff, triquetrous and often roughish, shorter than the narrow, flat and prim leaves; bright green.

Humid meadows, New England to Pennsylvania.

On *C. bullata*, Schk., Fig. 166, the pistillate spikes are shown as long and loose-flowered, and the fruit globose (ovate-globose), inflated abruptly, contracted into a slender, round, long, scabrous bifurcate beak, with a glume about half as long.

On the Carex, Boott, Illust., No. 39, the fruit is very large and inflated-ovate, with a conic, tapering, three-sided beak, scabrous-dentate, forming thick, dense and large spikes. If the figure by Dr. Boott shows *C. bullata*, Schk., then the figure in Schk. is a palpable caricature. Trusting to the correctness of the figure of Schk., I may yet say that Dr. Boott has presented a well known and distinct form. It is obvious that it should have another name, which designates the *inflation* of the fruit or perigynium, like the other.

264. *C. Olneyi*, Boott, Illust., No. 40.

Spicis staminiferis, 2–4, saepe 3, cylindraceis approximatis gracilibus; pistilliferis saepe 2, vel 1–3, cylindaceis sub-crassis densifloris stramineis approximatis plus minus pedunculatis, infima basin tereti laxiflora saepe subnutante, bracteatis; fructibus inflati-ovatis brevi-conico-rostratis scabro-bifurcatis nervosis divergentibus, squama lanceolata acuta vel cuspidata longioribus.

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Culm 15–22 inches high, strong, obtusely triquetrous, scabrous above, shorter than the long and stiff margined leaves.

Rhode Island—*Olney;* probably associated with this group over the country. Were it not for the very different spikes, the fruit would closely ally this species to *C. physema*, as being a smaller form of it.

Remark.—This Vesicaria group is of difficult determination, and there has been much confusion in consequence. Hoping that some light has shined upon it, I can only say with Dr. Boott, that "future observation must determine in America the value to be affixed to the species of this group." The following shows the group as here exhibited:

C. monile, Tuckerman. Boott, Illust., No. 39.
C. Vaseyi, Dew. *C. monile*, Sart., Exsic. No. 151, and Sill. Journ., [1] vol. 48, p. 47, putting *C. Vaseyi* for *C. monile.*
C. bullata, Schk. Dew., Sill. Journ. vol. 9, p. 71 (not Boott or Carey).
C. physema, Dew. *C. bullata*, Boott, and Carey in Manual.
C. Tuckermani, Boott. Dew., Sill. Journ., vol. 49, p. 47. *C. cylindrica*, Carey.
C. Olneyi, Boott, Illust., and as above.

As Dr. Boott has found *C. lenticularis*, Mx., among the northern Carices, it is now distinguished and identified. The great difficulty had been in the imperfection of the description of Michaux. This is removed by Dr. Boott, which the younger botanists will be glad to see, and also the more complete description.

C. lenticularis, Mx. Boott, Illust., No. 76, Fig. 77. *C. concolor*, R. Br. in this Journal.

Spicis cylindraceis obtusis approximatis et sessilibus; staminifera unica, interdum 2, infima brevi, vel terminali superne sæpe fructifera; pistilliferis 2–5, vel rarò pluribus, foliato-bracteatis, interdum inferne staminiferis, infima vix sessili; fructibus ellipticis convexo-lenticularibus, interdum subovatis, per-brevi-rostellatis nervosis stramineis glabris, squama oblonga *obtusa* pallida longioribus; planta matura concolore.

Culm 8–14 inches high, triquetrous, erect, leafy towards the base, leaves about the length of the culm, and sheaths longer than the culm; terminal spike staminate, often only staminate below, sometimes a shorter staminate sessile spike near it; pistillate spikes 3–4, or more or less, cylindrical, erect, the lowest sometimes vaginate-bracted; stigmas 2; fruit sub-ovate, longer than the oblong obtuse white-edged scale; whole plant light green, nearly of the same color.

Lake of Swans and Arctic America, *Michaux and Richardson.* Var. Albi-montana, has fruit less oval, or ovate and acutish, tapering above, resembling somewhat *C. torta*, but taller than the Arctic plants. White Mountains, N. H., about ponds—*Tuckerman.* In Harrison, Me., about ponds, in 1859—*Rev. J. Blake.*

Errata.—On p. 232, vol. xxviii, for *C. gynocratis* read *C. gynocrates.* On p. 231, same vol., for vol. xxiv, p. 48, read vol. xxvii, p. 81, Sec. Ser., and for the numbers 254, 255 and 256, read 257, 258 and 259.

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ART. XXXI.—*On Numerical Relations existing between the Equivalent Numbers of Elementary Bodies;* by M. CAREY LEA, Philadelphia. Part II.

(Concluded from p. 111.)

On Geometrical Ratios existing between Equivalent Numbers.

THE First Part of this paper was devoted to the examination of relations between the equivalent numbers of certain elementary bodies depending upon the number 44–45, and it was attempted to show:—

1st. That such relations extend to nearly all the elements:—

2d. That the particular groups collected together by this relation consist of bodies whose properties are analogous, and that the classification is in harmony with the distinguishing characteristics of the substances classified.

The first portion of this Second Part will present a species of relation wholly distinct, it is believed, from any that has hitherto been pointed out, and which may be not inappropriately termed Geometrical Ratios, to distinguish them from the more familiar arithmetical relations which have been heretofore exclusively studied by chemists.

The arithmetical relations are susceptible of at least an hypothetical explanation, on the supposition that the common difference in a series of elements may represent the equivalent number of a substance as yet undetermined, which, by its combinations in varying proportions, gives rise to the bodies constituting the successive terms of the series. The analogies which are now to be considered are more difficult of explanation, even by hypothesis. Their accuracy, sometimes absolute, renders improbable the supposition that they are mere casual coincidences. In science it is not permitted to neglect facts merely because we cannot satisfactorily account for them.

The nature of these relations consists in this, that if we take two substances and examine the ratio which subsists between the numbers representing their atomic weights, we may find in certain cases, that it is identical with the ratio subsisting between the atomic weights of two other substances, and so on through a considerable number of elements. The ratio between the atomic weights, for instance, of oxygen and nitrogen, is that of four to seven, so likewise is that between those of zirconium and potassium, potassium and barium, with absolute exactitude. What renders this the more remarkable is, that all three of these last substances are striking exceptions to Prout's law that the equivalents of the elements are exact multiples of that of hydrogen; they all have decimals, zirconium 22·4, potassium 39·2, barium 68·6. Now the ratio just mentioned gives these num-

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bers with their decimals with perfect exactness. The same species of relation exists between many other elements, as will be seen by the table below :—

(1.)

Oxygen-Nitrogen Ratio, or that of Four to Seven.

			Atomic wts. calculated.		Atomic wts. received.
Nitrogen	= 14,	$\frac{4}{7}$ of 14	= 8	Oxygen	= 8
Barium	= 68·6,	$\frac{4}{7}$ " 68·6	= 39·2	Potassium	=39·2
Potassium	= 39·2,	$\frac{4}{7}$ " 39·2	= 22·4	Zirconium	=22·4
Calcium	= 20,	$\frac{4}{7}$ " 20	= 11·43	Magnesium	=12
Magnesium	= 12,	$\frac{4}{7}$ " 12	= 6·86	Glucinum	=7*
Strontium	= 43·75,	$\frac{4}{7}$ " 43·75	= 25	Titanium	=25
Lead	=103·5,	$\frac{4}{7}$ " 103·5	= 59·16	Tin	=59
Antimony	=120·3,	$\frac{4}{7}$ " 120·3	= 68·76	Vanadium	=68·60
Bismuth	=208,	$\frac{4}{7}$ " 208	= 118·84	Antimony	=120·3
Mercury	=100,	$\frac{4}{7}$ " 100	= 57·16	Cadmium	=56
		$\frac{4}{7}$ " 57·16	= 32·66	Zinc	=32·60
Molybdenum	= 48,	$\frac{4}{7}$ " 48	= 27·42	Chromium	=26·70
Chlorine	= 35·5,	$\frac{4}{7}$ " 35·5	= 20·28	Silicon	=21
Fluorine	= 19,	$\frac{4}{7}$ " 19	= 10·86	Boron	=10·9†
Boron	= 10·9,	$\frac{4}{7}$ " 10·9	= 6·23	Carbon	=6

It will be remarked that mercury, cadmium and zinc are here again brought together. Although four-sevenths of the atomic weight of mercury is not that of cadmium exactly, nevertheless four-sevenths the resulting number so obtained, viz. 57·16, gives for the atomic weight of zinc 32·66, an exceedingly close approximation.

The last three equations in the table show us that the atomic weight of silicon stands nearly in the same numerical ratio to that of boron, as that of chlorine does to that of fluorine, and that both these ratios, especially the latter, approach nearly to that existing between the atomic weight of boron and carbon.

Now as the atomic weight of boron is by no means positively determined, it may be allowable to examine how far an hypothetical number will fulfill these several ratios. Let us assume:—

Fluorine : Chlorine : : Carbon : Boron

we find:—

$$\frac{\text{Chlorine } 35{\cdot}5 \times \text{Carbon } 6}{\text{Fluorine } 19} = 11{\cdot}2104,$$

and obtain for the hypothetical equivalent of boron the number 11·21 (neglecting the last decimal figures).

Let us now examine how far the number 11·21 will fulfill the second proposed ratio, viz.

Carbon : Boron : : Boron : Silicon

* Taking Glucina as G_2O_3. † See Jahresbericht, 1858.

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we have $\frac{(\text{Boron } 11{\cdot}21)^2}{\text{Carbon } 6} = 20{\cdot}944$:—

a close approximation to 21, the received number.

If, therefore, we assume the atomic weight of boron to be a mean proportional between those of carbon and silicon, which we may do by an alteration certainly within the bounds of possible error in determinations so far made, we find that the proportion between the weights of carbon and boron, of boron and silicon, is the same as that between those of fluorine and chlorine.

(2.)

Carbon-Nitrogen Ratio, or that of Three-Sevenths.

The atomic weight of Carbon stands to that of Nitrogen in the ratio of 3 to 7, a proportion which is found exactly or approximately to extend to certain other elements.

Nitrogen	= 14,	$\frac{3}{7}$ of 14	= 6	Carbon	= 6
Fluorine	= 19,	$\frac{3}{7}$ " 19	= 8·14	Oxygen	= 8
Iron	= 28,	$\frac{3}{7}$ " 28	= 12	Magnesium	= 12
Cerium	= 47,	$\frac{3}{7}$ " 47	= 20·14	Calcium	= 20
Phosphorus	= 31,	$\frac{3}{7}$ " 31	= 13·29	Nitrogen	= 14
Bromine	= 80,	$\frac{3}{7}$ " 80	= 34·29	Chlorine	= 35·5
Arsenic	= 75,	$\frac{3}{7}$ " 75	= 32·14	Zinc	= 32·6
Lead	= 103·5	$\frac{3}{7}$ " 103·5	= 44·4	Strontium	= 43·75
Cadmium	= 56,	$\frac{3}{7}$ " 56	= 24	2 Magnesium	= 24
Uranium	= 60,	$\frac{3}{7}$ " 60	= 25·71	Chromium	= 26·7

(3.)

The proportions expressed by the preceding tables may be differently presented, and perhaps rendered more striking. As the numbers which express the equivalent weights of the elements are altogether relative, it is of course a mere question of convenience which is selected as unity. If in place of adopting the equivalent of one substance, as that of hydrogen or oxygen, as a permanent unit, we successively make those of the substances contained in the right hand column of the table in section 1. our unit, and consider the effect of such a change upon the equivalents of the substances contained in the left-hand column, we shall obtain the following results:

Making the							
equivalent of	Oxygen	100,	the eq. of	Nitrogen	becomes	175
"	Potassium	100,	" "	" Barium	"	175
"	Zirconium	100,	" "	" Potassium	"	175
"	Titanium	100,	" "	" Strontium	"	175
"	Tin	100,	" "	" Lead	"	175·4
"	Vanadium	100,	" "	" Antimony	"	174·9
"	Boron	100,	" "	" Fluorine	"	174·3
	&c.			&c.		

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The same view may be extended to Section 2. Assuming the atomic weight of the substances in the left-hand column successively as unity, or one hundred, we obtain the following results:

Making the equivalent of	Nitrogen	100	that of	Carbon	becomes	42·8
	Fluorine	100	" "	Oxygen	"	42·1
	Iron	100	" "	Magnesium	"	42·8
	Cerium	100	" "	Calcium	"	42·6
	&c.			&c.		

It is therefore clear that the atomic weight of each substance in the left hand column of these several tables bears to that of the corresponding substance in the right-hand column a definite numerical ratio, which is identical, or nearly so with that borne by the atomic weight of any other substance in the same column of the same table to that of the corresponding substance in the other column.

(4.)

We have seen that many elements stand to other elements in the same relation as nitrogen to oxygen—many in the same relation as nitrogen to carbon. Apart from these more general ratios, many elements may be classed together in double or treble pairs, such that the two elements in one pair stand to each other in the same numerical ratio as the two elements of a second or third pair, the two elements constituting each pair being more or less closely allied to each other in properties, though the pairs are not necessarily analogous with those with which they are compared.

For example, arsenic stands to antimony in the same numerical ratio as selenium to tellurium, within an extremely small fraction, so that by multiplying and dividing we have:—

$$\text{Arsenic } 75 \times \frac{\text{Tellurium } 64}{\text{Selenium } 40} = 120, \text{ Antimony} = 120{\cdot}3,$$

So in like manner magnesium stands to zirconium in the same ratio as fluorine to chlorine:—

$$\text{Magnesium } 12 \times \frac{\text{Chlorine } 35{\cdot}5}{\text{Fluorine } 19} = 22{\cdot}42, \text{ Zirconium} = 22{\cdot}40,$$

So carbon stands to boron in the same ratio as silver to gold:—

$$\text{Carbon } 6 \times \frac{\text{Gold } 197}{\text{Silver } 108} = 10{\cdot}94, \text{ Boron} = 10{\cdot}90.$$

These ratios, as well as those that follow, are very close approximations, certainly within the limits of error which may easily exist in the determinations on which we depend. For antimony we find 120, the recent determinations vary from 119 to 122, that which appears most reliable is 120·3, as adopted by the Jahres-

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bericht. In the second case, the number found for zirconium varies but $\frac{1}{11}\frac{1}{17}$ from that generally received. In the third, the number found for boron is intermediate between the number adopted in the Jahresbericht 10·90 and that found by Deville, 11. Discrepancies such as these are trifling in the extreme, and have not the slightest real significance.

Phosphorus stands to nitrogen in nearly the same ratio as strontium to calcium:—

$$\text{Phosphorus } 31 \times \frac{\text{Calcium } 20}{\text{Strontium } 43{\cdot}77} = 14{\cdot}16, \text{ Nitrogen} = 14.$$

Tin stands to titanium in nearly the same ratio as iron to magnesium:—

$$\text{Tin } 59 \times \frac{\text{Magnesium } 12}{\text{Iron } 28} = 25{\cdot}28, \text{ Titanium} = 25.$$

Tin stands to zinc in almost exactly the same ratio as gold to silver, or as boron to carbon:—

$$\text{Tin } 59 \times \frac{\text{Silver } 108}{\text{Gold } 197} = 32{\cdot}35, \text{ Zinc} = 32{\cdot}60.$$

$$\text{Tin } 59 \times \frac{\text{Carbon } 6}{\text{Boron } 10{\cdot}9} = 32{\cdot}48, \text{ Zinc} = 32{\cdot}60.$$

These and many other analogies of the same character are brought together in the following table, by which the ratios of comparison between each pair are shown, up to the second place of decimals, beyond which it is pseudo-accuracy to go, in view of the data.

Ratio of tellurium to selenium, or	$\frac{\text{Te } 64}{\text{Se } 40} = 1{\cdot}60$
Ratio of antimony to arsenic, or	$\frac{\text{Sb } 120}{\text{As } 75} = 1{\cdot}60$
Ratio of zirconium to magnesium,	$\frac{\text{Zr } 22{\cdot}4}{\text{Mg } 12} = 1{\cdot}87$
Ratio of chlorine to fluorine,	$\frac{\text{Cl } 35{\cdot}5}{\text{F } 19} = 1{\cdot}87$
Ratio of boron to carbon,	$\frac{\text{B } 10{\cdot}9}{\text{C } 6} = 1{\cdot}82$
Ratio of gold to silver.	$\frac{\text{Au } 197}{\text{Ag } 108} = 1{\cdot}82$
Ratio of strontium to calcium,	$\frac{\text{Sr } 43{\cdot}77}{\text{Ca } 20} = 2{\cdot}19$
Ratio of phosphorus to nitrogen,	$\frac{\text{P } 31}{\text{N } 14} = 2{\cdot}21$

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Ratio of iron to magnesium,	$\frac{\text{Fe } 28}{\text{Mg } 12} = 2{\cdot}33$
Ratio of tin to titanium,	$\frac{\text{Sn } 59}{\text{Ti } 25} = 2{\cdot}36$
Ratio of tin to zinc,	$\frac{\text{Sn } 59}{\text{Zn } 32{\cdot}6} = 1{\cdot}81$
Ratio of gold to silver,	$\frac{\text{Au } 197}{\text{Ag } 108} = 1{\cdot}82$
Ratio of zirconium to aluminum,	$\frac{\text{Zr } 22{\cdot}4}{\text{Al } 13{\cdot}7} = 1{\cdot}64$
Ratio of calcium to magnesium,	$\frac{\text{Ca } 20}{\text{Mg } 12} = 1{\cdot}67$
Ratio of potassium to sodium,	$\frac{\text{K } 39{\cdot}2}{\text{Na } 23} = 1{\cdot}70$
Ratio of arsenic to phosphorus,	$\frac{\text{As } 75}{\text{P } 31} = 2{\cdot}42$
Ratio of cobalt to magnesium,	$\frac{\text{Co } 29{\cdot}5}{\text{Mg } 12} = 2{\cdot}46$
Ratio of nickel to magnesium,	$\frac{\text{Ni } 29{\cdot}6}{\text{Mg } 12} = 2{\cdot}47$
Ratio of lead to tin,	$\frac{\text{Pb } 103{\cdot}5}{\text{Sn } 59} = 1{\cdot}75$
Ratio of cadmium to zinc,	$\frac{\text{Cd } 56}{\text{Zn } 32{\cdot}6} = 1{\cdot}72$

(5.)

It has been the object of this paper to develope as far as possible those numerical relations existing between the atomic weights of the elements which have not been previously observed. It is, perhaps, impracticable in the present state of chemical science to explain why such relations should exist, nevertheless, nothing which tends to an exacter knowledge of the laws which govern the proportions in which the elements combine should be neglected. In this way, little by little, the materials are collected for future generalizations, with the reasonable hope of eventually arriving at an intimate knowledge of the true constitution of the materials which compose our globe.

The substances which compose the chlorine group form a well marked family to which fluorine seems also to belong. The analogies which unite chlorine, bromine and iodine, appear to multiply with each new investigation of their properties. By none have they been more strikingly illustrated than by the recent experiments of Plücker on the spectra obtained from the light of electric discharges through gases in a state of extreme

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rarefaction, demonstrating that those given by Cl, Br and I, resembled each other in exhibiting great numbers of black lines of extreme fineness, so fine indeed as to be almost mathematical lines, and that in this respect these elements differed wholly from all others. With so remarkable an experiment as this before us, supported by the very numerous other relations which exist between these substances, we might naturally look to find striking numerical correspondences between their atomic weights, but the analyses upon which we at present depend give us numbers which afford no relations positively exact. Of the four numbers 19, 35·5, 80 and 127, two are prime, one fractional, and one has many divisors. Such numbers are very unpromising for the development of numerical relation.

Thus far, however, at least relations do exist: that it is possible to express the value of the equivalent of any one of these substances in terms of any two of the remaining—each equivalent is a function of any other two, thus:—

$$I = 10\,Cl - 12\,F \qquad Cl = \frac{12\,Br - 7\,I}{2}$$

$$I = \frac{5\,Br - F}{3} \qquad Cl = \frac{I + 12\,F}{10}$$

$$I = \frac{12\,Br - 2\,Cl}{7} \qquad Cl = \frac{Br - 7\,F}{6}$$

$$Br = 6\,Cl - 7\,F \qquad F = \frac{10\,Cl - I}{12}$$

$$Br = \frac{2\,Cl + 7\,I}{12} \qquad F = \frac{6\,Cl - Br}{7}$$

$$Br = \frac{3\,I + F}{5} \qquad F = 5\,Br - 3\,I$$

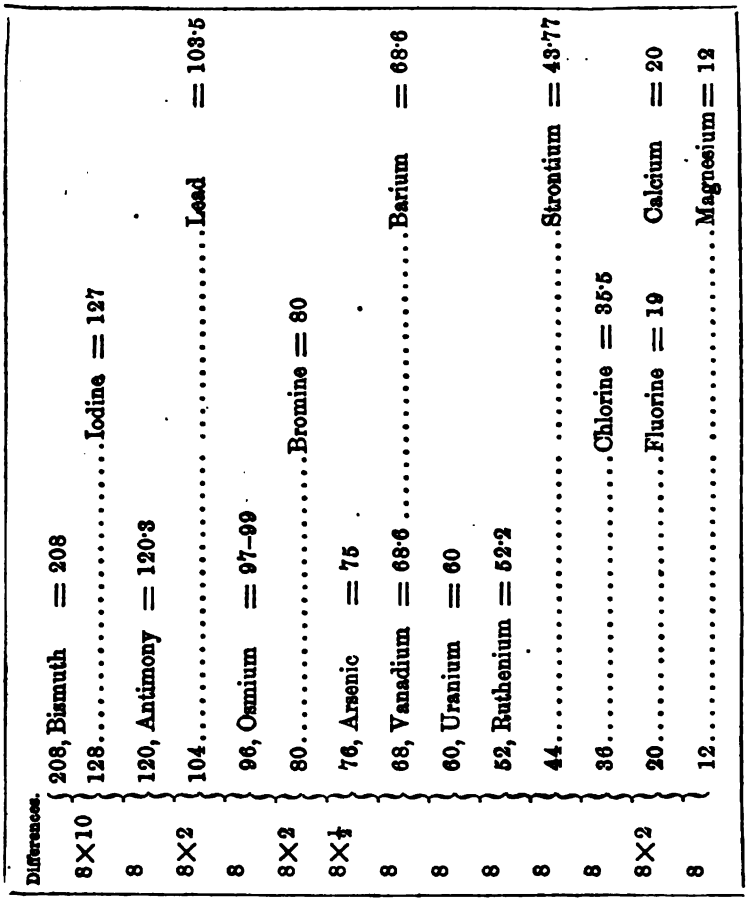
If these equations represented approximations, no matter how close, they would not be worthy of consideration. They are perfectly exact. The quasi-connection of oxygen with this series will be pointed out further on.

(6.)

It has been remarked that the atomic weights of the members of the oxygen series proper differ from each other by whole multiples of the atomic weight of oxygen, but the extent to which differences of this kind exist amongst the atomic weights of the elementary bodies has perhaps not been pointed out.

The elements having the highest atomic weights are bismuth and gold, commencing with the first of these substances, and diminishing its atomic weight successively by whole multiples (in one case by a sub-multiple) we obtain the following results:—

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It has not been usual to classify vanadium, osmium or ruthenium with the arsenic series, but Schafarik in a paper on the vanadium compounds,* and Hallwachs and Schafarik in a subsequent paper† on allied subjects (to neither of which the author had access while engaged on the first part of this paper except in the form of a brief notice) are of opinion that vanadium should be removed from the molybdenum group in which it has heretofore been classed, and be transferred to the arsenic series, an opinion which they support by arguments founded on the fact that the specific volume as well of vanadic acid as of the

* Sitzungsbericht of the Acad. of Sciences of Vienna, Bd. xxxiii, ext. in Chemisches Centralblatt, 1859, 97.

† Same vol. of Sitzungsbericht, ext. in Chemisches Centralblatt, 1859, p. 161.

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metal itself, corresponds with those of the metals of the arsenic series, viz.:

		Specif. Vol.	
Vanadic acid,	VO_3	26·5	
Arsenious acid,	AsO_3	26·6	
Antimonic oxyd,	SbO_3	25·9	
Bismuthic oxyd,	BiO_3	25·9*	28·4†

They also point out the fact that vanadinite (vanadinbleierz) is isomorphous with the analogous compounds of the arsenic series, mimetene (kampylit) and pyromorphite. Professor Mallet has shown that reasons can be given for classifying osmium also in the arsenic series.‡

Again, commencing with gold we obtain the following succession of numbers.

Differences.			
8×11	196 Gold	=197	
	108 Silver	=108	
8	100 Mercury	=100	
8	92 Tungsten	= 92	
8×3½	64 Tellurium	= 64	
8	56	Cadmium	=56
8	48 Molybdenum	= 48	
8	40 Selenium	= 40	
8	32	Zinc	=32·6
8×2	16 Sulphur	= 16	
8	8 Oxygen	= 8§	

In an interesting paper published in the Memoirs of the Am. Acad. of Boston, vol. v, Prof. J. P. Cooke, of Harvard University, attempted a classification of all the elementary bodies by making them all functions of a general series of the form $a+dx$. By giving fixed values to a and d particular expressions were obtained, giving rise to series by the changes in the value of the variable x. These particular series were characterized by the value of d, which in different series became successively 3, 4, 5,

* Boullay. † Karsten. ‡ Am. Jour. Science, xxix, 49.

§ If to the at. wt. of Li which may be taken at 7, we add 16=2×8, we have 23 the exact at. wt. of Na, and a further addition of 16 gives very nearly the at. wt. of K.

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6, 8 and 9. The values of a were likewise always small, never exceeding 8. The values of x were restricted to whole numbers.

This view, although new and ingenious, is evidently exposed to a serious objection: the series include too much. The form entitled by Prof. Cooke the "Three Series," and expressed by $1+3x$, includes one-third of all possible whole numbers, and so to a proportionally great extent with all the other forms. As the author did not make positive exactness essential, it is evident that a wide scope was given for the classification of elements under any particular series: therefore this theory could only find favor as far as it might be made to conform to a wholly unobjectionable classification of the elements. In this it only partially succeeded, as will appear from an examination of the substances included in the various groups.

In the first series, chromium, manganese, osmium and gold are classed as affiliated to the chlorine group, into which however the series only admits fluorine with an error of 2, or 12 per cent.

In the second, arsenic and manganese are made affiliated with the sulphur group, which includes also molybdenum, vanadium, &c.

In the third, oxygen is classed with the nitrogen-phosphorus group, into which antimony only enters with an error of 7·7.*

In the fifth we find tin and titanium, the platinum group, gold, mercury, and most of the magnesia group.

In the sixth we have part of the remainder of the magnesia group, the metals of the alkaline earths and lead, the alkaline metals, hydrogen and silver, with copper and manganese as affiliations. Rendering all justice to the author for the originality and ingenuity of his views, it must be admitted that the flexibility of his series has led to a classification not in all respects supported by analogies. Five out of his six series contain one or more members of the magnesia group either directly or as affiliated members.

Dumas has followed out these ideas of Prof. Cooke and extended them. In place of referring his series to the type of the formic acid series alone, he includes the types of substituted ammoniums and stannethyls, under the general form $na+xd+yd'$; thus adopting an expression even more general than that of Prof. Cooke, but restricting it more closely in its application.

Perhaps the most beautiful and important of the relations pointed out by Dumas, are the parallel series of numbers, which by the subtraction of each term of the one from the corresponding term of the other, exhibit a constant difference within a certain degree of approximation.

* This probably arises from the fact that the equivalent of antimony as generally received at the time when Prof. Cooke wrote was higher than it is now known to be, and by diminishing the value of the variable x corresponding to this term, by 1, a nearer approximation would be obtained.

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The analogies pointed out in the several parts of this paper evidently lead directly to the construction of series of the same kind some of which, together with others depending upon yet different relations are given below.

−Oxygen	=−8	Chlorine	=35·5	Bromine	=80	Iodine	=127
−Zinc	=−32·6	Magnesia	=12	Cadmium	=56	Lead	=103·5
	24·6		23·5		24		23·5

In connection with these two parallel series we may remark that oxygen appears to constitute a negative term in the chlorine series, precisely as nitrogen does in the phosphorus and zinc in the cadmium series, with considerable approximation to exactness. If the atomic weight of chlorine were taken at 36 instead of 35·5 it would constitute an exact numerical mean between the atomic weights of bromine and oxygen supposing the latter to be taken with a negative sign. We have before seen that nitrogen, the negative member of the phosphorus series, is less closely allied with the rest than they are with each other. Still greater is the step between chlorine and oxygen, so great indeed that it is very doubtful whether they can properly be classed in the same series, although certain cases of isomorphism can be urged in favor of such a classification.

We here see a new instance of the existence of the relation of 44—45 developed in the first part of this paper.

Differences.	Calculated equivalents.			Received equivalents.	
	− 8	-	-	Oxygen	= 8
44	36	-	-	Chlorine	= 35·5
44	80	-	-	Bromine	= 80
44	124	-	-	Iodine	=127

Gold	=197	M	=152·5	Silver	=108	Mercury	=100
Mercury	=100	Cadmium	= 56	Magnesium	= 12	Glucinum	= 4·7
	97		96·5		96		95·3

M here represents a possible metal as yet unknown. In the first part of this paper mention was made of the possibility of the existence of such a metal, having an atomic weight representing the arithmetical mean between those of gold and silver.

Bismuth	=208	M′	=164	Antimony	=120·3	Lead	=103·5
Antimony	=120·3	Arsenic	= 75	Phosphorus	= 31	Nitrogen	= 14
	87·7		89		89·3		89·5

In this double series we find nearly the same number (89 approximatively) obtained by subtraction, as by addition in the last of the double series, part first, section 8. If in that just presented we replace lead by arsenic and give a negative sign to

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the atomic weight of nitrogen, we shall still have a number conforming to the character of the series, viz., 89.

In this and the following series the symbol M′ represents a possible metal constituting an intermediate term between antimony and bismuth. That such a metal might exist was pointed out in the consideration of the phosphorus series.

Bismuth	=208	M′	=164	Antimony	=120·3	Arsenic	=75
Gold	=197	M	=152·5	Silver	=108	Copper	=63·4
	11		11·5		12·3		11·6

M as before represents an hypothetical metal having for its atomic weight a mean term between those of silver and gold.

Uranium	60	Vanadium	68·6	Tungsten	92
Magnesim	12	Calcium	20	Strontium	43·75
	48		48·6		48·25

Molybdenum	48	Vanadium	68·6	Tungsten	92	2 Tantalum	187·6
Magnesium	12	Zinc	32·6	Cadmium	56	Mercury	100
	36		36		36		87·6

The parallelism between the three first terms of these last two series commencing respectively with molybdenum and magnesium is positively exact, perhaps the only known case in which absolute exactitude obtains. It is probable that other cases of parallel series exist, and will be discovered.

It is not easy to fix the exact amount of importance which attaches to the numerical relations up to this time ascertained to exist between the atomic weights of the elements. Some are no doubt mere casual coincidences, and relations remarkably exact and symmetrical may exist between the atomic weights of bodies which have no analogies in their properties: for example we may take calcium 20, selenium 40, uranium 60, bromine 80, mercury 100. Here the differences are not only exact, but all the subsequent numbers are multiples of the first, and this between bodies remarkably dissimilar in their properties—a striking proof of the necessity of caution in inferring relations of properties as following from relations of numbers. But on the other hand, to reject the relations of number when accompanied by analogy of properties, as unmeaning and unimportant, would be to err quite as much on the other side. When the received equivalent of an element forming a term in a well marked series differs from that obtained by calculation, it naturally leads, as Prof. Mallet has remarked, to suspect an error and desire a redetermination. The fact that a group of elements allied in their chemical characters may be arranged in a series having a common difference or a definite ratio between its terms, confirms the propriety of grouping those elements together, and such analogies may in doubtful cases assist us in arriving at a correct classification.

Philadelphia, Feb. 27, 1860.

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ART. XXXII.—*Ornithichnites, or tracks resembling those of Birds;* by ROSWELL FIELD, of Greenfield, Mass.

WHEN fossil footprints were first discovered in the sandstones of the Connecticut Valley, it was indeed thought to be a great discovery, but that the tracks, thus found were made by birds was received by men of scientific attainments with great distrust and skepticism. That they were tracks made by once living animals there could be no doubt, but that they were ornithichnites was very much doubted.

It was not indeed until after my esteemed friend Dr. Hitchcock had spent much time in comparing, describing, and in distributing specimens, that the scientific public became satisfied that they were the tracks of once living birds.

The great and only proof that they were the tracks of birds, is found in the organization of the fossil foot, in the numbers of joints or lateral expansions in the toes; in this they are supposed to agree, and probably do, with living types; this with alternate steps of right and left feet is all the evidence we have that they were the tracks of birds.

Living in the immediate vicinity of Turner's Falls, the locality that has furnished the most numerous, and beyond all comparison the most beautiful specimens, my attention was drawn many years since to this particular subject. It was from my farm that the late Dexter Marsh obtained his choicest specimens. And it was in the vicinity of these Falls where my much lamented friend Dr. Deane found "new walks in an old field:" Where our barren and rocky wastes became to him a garden of delight.

It was here I witnessed their labors with pleasure, and in a more obscure way have followed in their footsteps. I think I may safely say that I have uncovered more footprints, and found more new species, and a greater variety of tracks than any other man, I think I might also say with propriety than all others that have preceded me, and if I have learnt anything on this subject I have learnt it at the quarries. It is there, and there only I have studied the history of Triassic days, and the more I have studied, the greater have been my doubts as to the ornithic character of any of the tracks which these tables of stone contain. I have seen thousands of tracks that others have not seen. With injudicious blasting, and the carelessness of workmen many choice specimens have been broken and lost; other slabs literally covered with footprints, have been spoilt by suncracks the shrinkage of the mud in drying; the stratum over which the animal moved being either too hard or too soft to receive or retain good impressions, all such are rejected and lost to the student, at the quarries.

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I have no new theory to advance, and none to build up, but if I can rightly decipher these fossil inscriptions impressed on the tombstones of a race of animals that have long since ceased to exist, they should all of them be classed in the animal kingdom as Reptilia. If I have not studied this subject in vain they were all quadrupedal. That they usually walked on two feet I admit, and that they could as readily walk on four when necessary is equally true. In proof of this we find tracks as perfect as if made in plaster or wax, which to all appearance as to the number of toes, and the phalangeal or lateral expansions in the toes agree perfectly with those of living birds, and still we know by the impressions made by their forward feet that these fossil tracks were made by quadrupeds. In other cases where the animals sunk deep in the muddy stratum over which they moved, it is plain that they dragged their tails in the mud, leaving a groove plowed up from one half inch to an inch in width. This groove is not always found on the surface where the foot rested, the weight of the animal causing the foot to sink through the yielding stratum, whilst the tail dragged on the one above. This we know was the case with animals that were surely quadrupeds, but they show the appendage of a tail only when their feet sunk deeply in the plastic clay. Thus the proof that we once relied upon to prove them birds can be relied upon no longer.

That there were quadrupeds in those sandstone days and that these had hind feet perfectly agreeing with the stony bird-tracks, throws great doubt and distrust on the question, whether there were any true birds in this age of reptiles.

If there were birds they were doubtless apterous and naked, for we should naturally suppose that where so large a number of birds congregated upon the muddy banks, that in dressing and pluming their feathers some of them must have been trodden under foot, but the impression of feathers has never been found, although we find the smallest leaves of vegetables, and the pathway and tracks of annelids, and insects, some of them so small that they can hardly be seen with the naked eye. Even the Otozoum whose giant-like track measures twenty inches in length, once supposed to have been a biped reptile, by later discoveries is proved to have been four-footed. Other new discoveries have reduced the number of so-called birds, transferring them to the class of quadrupeds, which I verily believe is the proper place for all of them. The smoothness of the bottom of the foot in our fossil tracks agrees better with some species of batrachians that now live in and about the water, than it does with such animals as live on the land. Had birds existed at this early geological period, when the sandstone of the Connecticut valley was being deposited, there has indeed been a woful gap in their history, from then up to near the historical period, while the die from

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which they were struck at their creation was not broken, but a new edition produced in these latter days. The work perhaps may have been revised but has not been enlarged as respects the size of the animal. I know that many eminent men and men of great scientific attainments, men who have spent much time and labor for many years in the investigation of this subject have come to different conclusions, and it may not become me to say that their conclusions are wrong. I would only add that when fossil tracks were first discovered there was so little known of the formation of the feet of fossil or of living animals, and particularly of their footprints that it is possible the first discoverers might have been mistaken as to the ornithic character of the footprints. The study of these fossils so very interesting to the geologist and naturalist, still merits their earnest attention. There is no known locality where they are found in such abundance, and in such perfection as at Turner's Falls, the northern terminus of the sandstone beds. Very few indeed have any conception of the marvelous perfection of this fossil inscription, or of the multitudes of once living creatures whose existence they commemorate.

March, 1860.

[Mr. Field is a plain farmer, who makes no claim to be an authority in science, but like Hugh Miller has hammered his geology out of the rocks on which he lives. He is well known as one of the most successful collectors of the foot-marks of the Connecticut sandstone, and his testimony as to the impression made on himself of their probable character and origin, has the merit of a conviction making head in an honest mind against all the weight and bias of opposing authorities.—Eds.]

Art. XXXIII.—*Eighth Supplement to Dana's Mineralogy;* by Geo. J. Brush, Professor of Metallurgy in Yale College.*

List of Works, etc.

F. X. M. Zippe: Lehrbuch der Mineralogie mit naturhistorischer Grundlage. pp. 450, 8vo, mit 334 Fig. Wien, 1860.

A. Descloizeaux: Sur l'emploi des propriétés optiques biréfringentes pour la détermination des espèces cristallisées. p. 88, 8vo. Extrait des Annales des Mines, tome xiv, 1858.

A. Delesse: Etudes sur le Métamorphisme. Roches granitiques, pp. 77, 8vo. Extrait des Annales des Mines, tome xii.

Carl F. Naumann: Elemente der Mineralogie, fünfte vermehrte und verbesserte Auflage. 8vo, pp. 460, mit 483 Fig. Leipzig, 1859.

N. von Kokscharow: Materialen zur Mineralogie Russlands, dritter Band, Lieferung 1—3, pp. 128, mit Atlas. St. Petersburg, 1859.—This continuation of Kokscharow's great work, contains monographs of the species garnet, magnetite, anal-

* In the absence of Professor Dana I have endeavored to give an abstract of the results of the mineralogical researches, published since the appearance of the seventh Supplement.—G. J. B.—New Haven, April 1, 1860.

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cime and euclase, together with important additional notes upon beryl, cancrinite, nepheline, phenacite, and apatite.

ADAM: Extrait du Tableau Minéralogique. 8vo, pp. 14. Paris.

J. F. L. HAUSMANN: Ueber die Krystallformen des Cordierites von Bodenmais in Bayern. 4to. pp. 16. Göttingen, 1859.

GUSTAV ROSE: Ueber die heteromorphen Zustande der kohlensauren Kalkerde, zweite Abhandlung, mit drei Kupfertafeln. 4to. pp. 48. Berlin, 1859.

VICTOR VON LANG: Versuch einer Monographie des Bleivitriols. 8vo, pp. 54, mit xxvii Tafeln. Wien, 1859.

D. D. OWEN: *First Report of a Geological Reconnaissance of the Northern Counties of Arkansas.* Little Rock, 1858.—In this report Dr. Elderhorst has contributed much that is of interest in regard to the ores of zinc, lead and manganese, and presented his facts in a manner that will make them of permanent practical value.

H. KOPP und H. WILL: Jahresbericht über die Fortschritte der Chemie und verwandter Theile anderer Wissenschaften für 1858. Giessen, 1859. pp. 859.—Pages 673 to 812 contain Dr. Kopp's excellent review of the progress of mineralogical science for the year 1858.

H. C. SORBY: On the Microscopical Structure of Crystals, indicating the Origin of Minerals and Rocks. 8vo, pp. 48, with plates. Quart. Jour. Geol. Soc. Lond., xiv, pp. 458–500.

H. DAUBER: Ermittelung krystallographischer Constanten und des Grades ihrer Zuverlässigkeit. Pogg. Ann., cvii, 267, 343, cviii, 439.

W. H. MILLER: On the employment of the Gnomonic Projection of the Sphere in Crystallography. L, E. and D. Phil. Mag. for July, 1859.

DAUBRÉE: Memoire sur la relation des sources thermales de Plombières avec les filous métalifères, et sur la formation contemporaine des zéolithes. Extrait du Bulletin de la Société Géologique de France, [2], xvi, p. 562–591.

A. KENNGOTT: Uebersicht der Resultate mineralogischer Forschungen im Jahre, 1858. 8vo, pp. 229. Leipzig, 1860.—This is a continuation of Dr. Kenngott's excellent reports, and contains a résumé of the results of mineralogical researches made in 1858. Its completeness, and the careful criticisms by Dr. Kenngott render it a most valuable auxiliary in the study of mineralogy.

F. HESSENBERG: Mineralogische Notizen, No. 3. 4to, pp. 32. Frankfurt, 1860.—This number of Hessenberg's mineralogical notices, contains contributions to the crystallography of the species lievrite, realgar, heavy-spar, calcite, sphene, anatase, crocoisite, and malachite.

*Descriptions of Species.**

ALBITE [p. 240, VI].—Analyses of albite, (1.) from Oberhalbstein in Graubunden by Descladissac (Zeitsch. d. deutschen geolog. Gesell., x, 207). (2.) from Calveras County, California, associated with auriferous pyrites and native gold by F. A. Genth (this Journal, [2], xxvii, 249):

	Si	Al	Fe	Ca	Mg	Na	K	ign.	
1.	68·50	18·11	——	0·56	0 66	12·17		.	=100·00
2.	68·39	19·65	0·41	0·47	——	10·97	*tr.*	0·21	=100·10

ALLANITE [p. 208, I—VI].—Analyses of *orthite* (1.) from Arendal by Zettel (Ann. d. Chem. u. Pharm., cxii, 85). (2.) mean of four analyses of orthite from Suontaka in Finland by Mendeljef (Kopp's Jahresbericht, 1858, 703):

	Si	Al	Fe	Mn	Ce	La, Di	Ca	Mg	K	Na	H	C	
1.	32·70	17·44	16·26	0·34	3·92	15·41	11·24	0·90	0·51	0·24	2·47	0·28	=101·71
2.	48·0	2·4	34·8	——	3·3	ÿ 1·5	9·3	——	——	——	0·7	——	=100·00

* The paging refers to Dana's Mineralogy, and the Roman numerals, in many places added, to the preceding Supplements.

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ANORTHITE [p. 234, II, VI].—Potyka (Pogg. Ann., cviii, 110) has found *anorthite* to be a constituent of the rock occurring associated with hornblende at the Konchekowskoi Kamen in the Urals. The sp. gr. in fragments was 2·731 (17·1° C.), in powder 2·7325 (16·8°). B.B. difficultly fusible. Only partially decomposed, and with gelatinization, by chlorhydric acid.

Si	Al	Fe	Ca	Mg	K	Na	
45·31	34·53	0·71	16·85	0·11	0·91	2·59	= 101·01

corresponding very closely with the formula $\dot{C}a^3\ddot{S}i+3\dot{A}l\ddot{S}i$, or considering silica as SiO_2=$\dot{C}a\ddot{S}i+\dot{A}l\ddot{S}i$.

Anorthite from Carlingford, Ireland, gave on analysis by Prof. Haughton, Si 45·87, Al 34·73, Ca 17·10, Mg 1·55=99·25 (Greg, L., E. and D. Phil. Mag., [4], xix, 13).

ANTIGORITE [p. 281, I, IV].—For description of optical properties of, see Haidinger's article in Pogg. Ann., lxxvii, 94, and Descloizeaux, Ann. des Mines, xiv, (1858.)

APATITE [p. 396, I—VII].—A description of crystallized *apatite* from Pfitsch-Thal in Tyrol is given by G. vom Rath.—Pogg. Ann., cviii, 353.

ATACAMITE [p. 138, I, III, IV, V].—Bibra gives for the composition of *atacamite* from Algodan Bay in Bolivia:

Cu	Cu	Cl	H	Si	
56·00	14·54	16·11	12·13	0·91	= 99·60

—Kopp's Jahresbericht, 1858, 740.

ARAGONITE [p. 448, II, III, IV, V, VII].—Luca has described a variety of aragonite, occurring in the Lias of Gerfalco in Tuscany, which he calls *mossottite*. The mineral is a prismatic fibrous radiated aggregate of a light green color. Sp. gr.= 2·884. On heating crumbles and loses its color. Composition:

Ca	Sr	C	Cu	Fe	Fl	H	
50·08	4·69	41·43	0·95	0·82	*tr.*	1·36	= 99·33

According to Marcel de Serres, the same variety of aragonite occurs in the province of Messina.—(Kopp's Jahresbericht, 1858, 732.)

A variety of aragonite from Nertschinsk has been named *cerasbite* by Breithaupt (B. and H. Zeit., xvii, 54).

AXINITE [p. 213].—This species has been observed at Cold Spring in New York.

BISMUTH [p. 20].—A specimen of *native bismuth*, associated with native gold, from the Peak of Sorato, analyzed by F. A. Genth (this Jour., [2], xxvii, 247) contained:

Bi	Te	Fe		
99·914	0·042	*tr.*	=	99·956

BORACITE [p. 393, II, III, IV].—The identity in chemical composition of *boracite* and *stassfurthite* was shown by Chandler (Suppl. IV), and is further confirmed by the analyses of Siewert and Drenkmann (Zeitschrift f. d. ges. Naturwissen, xi, 365, in Kopp's Jahresbericht, 1858, 735). The direct determinations made on the washed and ignited mineral in seven partial analyses gave as the mean:

Mg	Fe	B		
30·83	0·32	69·05	=	100·20

But H. Ludwig found (Arch. Pharm., [2], xcvi, 129, in Kopp's Jahresbericht, 1858, 735) that the unwashed air-dried stassfurthite contained

MgCl	Mg	B	H
11·75	23·80	58·45	6·00

(*a*) By the difference.

A direct determination of B gave 59·72 p. c., and Ludwig considered it a hydrous boracite containing a variable mixture of chlorid of magnesium. Heintz, however, has shown that chlorid of magnesium is an essential constituent of the mineral and that even after long washing with hot water it still contains a considerable amount of this chlorid. His results (calculated from five partial analyses) gave (Jour. f. prakt. Chem., lxxvi, 248):

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Cl	Mg	Ṁg	Fe	B̈	Ḣ	
8·14	2·84	25·74	0·43	61·22[a]	1·63	= 100

a By the difference.

from which he draws the formula $2(\dot{M}g^3\ddot{B}^4)+MgCl, \dot{H}$. In communicating these observations of Heintz to the Berlin Academy, H. Rose (loc. cit.) adds that the boracite from Lüneberg also contains chlorine as an essential constituent, and this has been substantiated by the recent analyses of Dr. Julius Potyka (Pogg. Ann., cvii, 433), and also by Heintz (Jour. f. prakt. Chem., lxxvii, 338).

Potyka analyzed both boracite and stassfurthite, and four analyses of boracite are given by Heintz, having been made under his direction by Siewert and Geist. The results are as follows:

	MgCl	Ṁg	Ḟe	B̈	Ḣ	
1. Clear boracite crystals,	10·90	25·24	1·59	62·91	0·55=101·19	Potyka.
2. Clouded " "	10·41	26·19	1·66	61·19	0·94=100·39	"
3. Stassfurthite,	10·73	26·15	0·40	60·77[a]	1·95=100·00	"
4. Boracite,	11·14	26·00	1·52	61·34[a]	——=100·00	Siewert.
5. "	11·71	24·86	1·13	62·30[a]	——=100·00	"
6. "	11·11	25·45	1·83	61·61[a]	——=100·00	Geist.
7. "	11·54	25·43	1·05	61·98[a]	——=100·00	"

a. By the difference.

Potyka's specimens, after pulverization, were washed with cold water until the wash water no longer gave any reaction with nitrate of silver or chlorid of barium; he found that both boracite and stassfurthite were slightly soluble in hot water. All the specimens of boracite were from Lüneberg. Heintz was unable to find a weighable quantity of water. Both Potyka and Heintz express the composition of boracite by the formula $2(\dot{M}g^3\ddot{B}^4)+MgCl=\ddot{B}$ 62·50, Ṁg 26·86, MgCl 10·64=100. Stassfurthite appears to be a boracite with one atom of water, $2(\dot{M}g^3\ddot{B}^4)+MgCl, \dot{H}=\ddot{B}$ 61·27, Ṁg 26·33, MgCl 10·42, Ḣ 1·98=100.

Bromyrite [p. 93].—F. Field has analyzed the *bromyrite* which occurs in octahedrons imbedded in carbonate of lime at Chañarcillo in Chile (Quar. Jour. Chem. Soc., x, 241). The crystals had the color and lustre of amber, and are much harder than the chloro-bromids or chlorid, and appear to be little affected by light. Composition,

Ag	Br	
57·43	42·57	= Ag Br

Cancrinite [p. 283, II].—P. v. Pusirewsky has analyzed the *cancrinite* from the Ilmen Mts. and from Mariinskaja in the Tunkinsk Mts. (Kokscharow, Mat. Min. Russlands, iii, 76):

	Si	Ä̇l	Fe	Ṅa	Ċa	Ö	Ḣ	S̈	
1. Ilmen Mts.,	35·71	29·58	——	18·78	5·56	5·56	3·76	0·32	=99·27
2. " "	36·21	29·56	0·19	18·27	5·81	5·54	3·64	——	=99·22
3. Tunkinsk Mts.,	37·72	27·75	——	21·60	3·11	5·61	4·07	——	=99·86

The variety from the Ilmen Mts. had a light rose-red color and sp. gr.=2·489. That from Mariinskaja was yellow, sp. gr.=2·454. Pusirewsky writes the formula $2(\dot{N}a^2\ddot{S}i+2\ddot{A}l\ddot{S}i+[\dot{N}a, \dot{C}a]\ddot{O})+3\dot{H}$, containing one atom more of water than the formula given by Whitney for the cancrinite of Litchfield, Maine.

Carnallite [III].—Heintz gives the composition of *carnallite*, as found by Siewert:

MgCl	KCl	NaCl	ĊaS̈	Ḣ	
36·03	27·41	0·23	1·14	36·33—38·01	

—Kopp's Jahresbericht, 1858, 739.

Cassiterite [p. 118, V, VI, VII].—In a recent letter (Boston, Jan. 8th, 1860) to Prof. B. Silliman, Jr., Dr. C. T. Jackson mentions having received from Los Angelos in California a so-called silver ore, which on examination proved to be oxyd of tin mixed with some peroxyd of iron. On assay it gave 60·5 per cent of metallic tin. From the size of the masses of ore, Dr. Jackson is led to suppose that a vein of workable magnitude exists, some specimens being eight inches in thickness.

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CERITE [p. 312].—Rammelsberg (Pogg., cvii, 632) has published several analyses of this species. The mean of the results gives—

Si	Ce	La, Di	Ca	Fe	H
19·18	64·55	7·28	1·35	1·54	5·71=99·61

from which he draws the formula $(Ce, La, Di, Ca, Fe)^3Si+Aq$, or R^3Si+Aq. An important fact observed by Rammelsberg is, that when cerite is treated with chlorhydric acid it is partially decomposed, leaving an insoluble residue of a different composition from that contained in the solution. He remarks that it appears as if cerite was composed of a mixture of silicates, differing in not being equally acted upon by acids.

CINNABAR [p. 48, II, IV, V].—Hugo Müller has analyzed and described tetrahedral crystals of cinnabar, from Asturia, Spain, which he supposes to be pseudomorphs of either tetrahedrite or chalcopyrite.—Quar. Jour. Chem. Soc., xi, 240.

CHLORITE [p. 294, IV, V].—For analyses of a chlorite-like substance from the melaphyr-porphyry of Ilfeld by Streng, see Zeitschrift d. deutschen geolog. Gesellschaft, x, 136.

CHRYSOCOLLA [p. 309, II].—P. Herter gives analyses of two varieties of chrysocolla from the crystalline slates of Ober and Nieder-Rochlitz in Transylvania, in Zeitsch. d. deutschen geolog. Gesellschaft, ix, 372. In the same paper Herter mentions a mineral which he considers a new species. It occurs in a geode of quartz. It is amorphous; brittle; color dark pistachio-green to liver-brown and dirty yellowish-green; has a strong pitchy lustre and an almost conchoidal fracture. Sp. gr.= 2·991. Contains Si 14·24, SbO_5 24·68, As 7·24, Cu 31·49, Pb 0·68, Ag 2·05, Fe 8·38, Ca 2·16, Mg 0·56, Al 0·21, H 8·03=99·71. Another specimen gave but 16 per cent copper, showing the composition to be variable. In matrass gives water. Fuses easily in the forceps coloring the flame emerald green, with soda on charcoal gives a metallic bead. The centre of several of the masses was found to contain tetrahedrite, of which the above substance is a product of decomposition.

CLAYITE, *W. J. Taylor* (Proc. Acad. Nat. Sci., Philad., Nov. 1859.).—This mineral is a sulphid of lead, with about twenty-five per cent of arsenic, copper and antimony, and appears to be intermediary between galena and cupro-plumbite. It is from Peru, and occurs in small monometric crystals, a combination of the tetrahedron with the dodecahedron. It is also found amorphous, forming a coating a thirty-second of an inch thick on a layer of quartz. Color and streak blackish-gray; sectile; hardness about 2·5. B.B. on charcoal fuses easily, giving reactions for lead, arsenic and antimony, and with soda a brilliant metallic globule which becomes lustreless on cooling. Carefully selected crystals gave—

	S	As	Sb	Pb	Cu	Ag
1.	8·22	9·78	6·54	68·51	7·67	*tr.*
2.	8·14	——	——	67·40	5·62	——

No. 2 was not entirely free from extraneous matters. Prof. Taylor gives the formula (Pb, Cu)(S, As, Sb). A confirmatory result was obtained on a specimen of the amorphous variety. The mineral was received from Joseph A. Clay, Esq., of Philadelphia, having been sent to him by his brother, Hon. J. Randolph Clay, United States Minister in Peru, and it is named in honor of these gentlemen. [The amount of sulphur is extremely small, and the presence of arsenic and antimony seems to indicate an analogy with *steinmannite*, which has recently been shown to be a galena, containing some twenty or more per cent of the sulphids of arsenic, antimony and zinc, although this composition varies exceedingly.—G. J. B.]

COAL [p. 26, II, IV, VI].—O. Matter has analyzed (Jour. f. prakt. Chem., lxxvii, 39) the so-called Bog-head coal from Torbane-Hill in Scotland, with the following result:

C	H	N	O	S	H	Si	Al	Fe	Ca
60·81	9·18	0·78	4·39	0·32	0·39	13·19	9·50	1·22	0·27=100·05

CONDURRITE [p. 36, V].—An examination of this mineral by C. Winkler (B. u. H. Zeit., xviii, 383) confirms the results obtained by v. Kobell and Rammels-

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berg, showing this mineral to be a mixture of arsenolite, cuprite, copper-glance, and arsenid of copper.

COPPERAS [p. 380].—A cupriferous variety of copperas from a mine of chalcopyrite in Turkey has been analyzed by Pisani (Comptes Rendus, April 18, 1859). Color like that of cyanosite, on exposure assumes an ochreous tint. Composition—

Ċu	Ḟe	S̄	Ḣ
15·56	10·98	29·90	43·56

giving the formula (Ḟe Ċu)S̄+7Ḣ, or copperas in which a portion of the iron is replaced by copper.—(L. E. and D. Phil. Mag., [4], xvii, 409.)

COPPER NICKEL [p. 52, VI].—According to G. Rose and Nöggerath the copper-nickel from Sangerhausen crystallizes in the hexagonal form.—(Zeitschrift d. deutschen geolog. Gesellsch., x, 91; Verhandl. d. naturhist. Ver d. Rheinlande, xv, xv.)

CYANOLITE, CENTRALLASSITE, CERINITE, *H. How*, (Ed. new Phil. Jour., x, 84).—Prof. How has described three new species of silicates occurring in a reniform nodule in the trap of the Bay of Fundy, one mile east of Black Rock. The nodule was about half the size of a fist. It was covered with a green chlorite-like coating, and on breaking it presented a curious internal structure; immediately beneath the coating was a narrow band of a yellowish-white mineral resembling wax (*cerinite*), then a portion having a stellated appearance and a highly pearly lustre (*centrallassite*), while the centre was principally made up of a bluish-gray opaque mineral in rounded spots (*cyanolite*). A careful separation of the constituents showed—1. *Cyanolite*, comprising the centre of the nodule, was amorphous; hardness=4·5; sp. gr.=2·495; fracture flat-conchoidal, even; streak white, lustre dull, color bluish-gray; sub-translucent in thin pieces, and the powder transparent under the microscope. Decomposed with chlorhydric acid, affording slimy silica, but does not gelatinize either before or after heating. In matrass becomes white and gives off water. B.B. in platinum forceps fuses only on the thin edges, with soda and borax gives transparent beads, with salt of phosphorus a translucent glass. Analysis gave—

	S̄i	Ā̇l	Ċa	Ṁg	K̇	Ḣ	
1.	74·15	0·84	17·52	*tr.*	0·53	7·39	=100·43
2.	72·52	1·24	18·19	*tr.*	0·61	6·91	= 99·47

Analysis No. 2 was made on a specimen not perfectly free from *centrallassite*. Disregarding the small amount of alumina and potash in No. 1, we have the oxygen ratio Ċa : S̄i : Ḣ as 1 : 7·85 : 1·31 or 4 : 31·40 : 5·2, from which Prof. How draws the formula $Ca^4Si_{10}+5H=Si$ 74·26, Ċa 18·36, Ḣ 7·37. Considering the water as basic the ratio of the oxygen in all the bases to that of the silica is as 1 : 3·2, approximating to that of Edelforsite, Ċa S̄i or 1 : 3. The name *cyanolite* is in allusion to the blue tint which distinguishes this mineral from its associates.

2. *Centrallassite* occurs in spherical concretions between the cyanolite and the rind. The concretions when broken have a lamellar structure and consist of plates diverging from a centre; the plates have a pearly lustre, but the mineral passes into an opaque white form. Centrallassite has a white, sometimes yellowish, color; translucent, transparent in thin plates; brittle; lustre sub-resinous; hardness=3·5; sp. gr.=2·45—2·46. In matrass yields water, becomes opaque and silvery-white. B.B. fuses readily, with spirting, to an opaque glass, with the fluxes gives a clear bead. Decomposed by chlorhydric acid without gelatinizing. The result of two analyses were:—

S̄i	Ā̇l	Ċa	Ṁg	K̇	Ḣ	
59·05	1·00	27·86	0·20	*undet.*	11·40	
58·67	1·28	27·97	0·13	0·59	11·43	= 100·07

The oxygen ratio of the mean of these analyses for the lime, silica and water is 1 : 3·91 : 1·27=4 : 15·64 : 5·08, from which Prof. How deduces the formula $Ca^4Si_5+5H=Si$ 59·06, Ċa 29·20, Ḣ 11·74. From two determinations of the water and an estimation of the silica, the opaque mineral was proved to have the same composition as the transparent variety.

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3. *Cerinite.* The narrow band enveloping the two preceding minerals (an eighth of an inch in thickness) was an opaque mineral, translucent in very thin fragments; amorphous; lustre sub-resinous, resembling white or yellowish-white wax; H.=3·5. B.B. fusible without intumescence. It was imperfectly decomposed by chlorhydric acid. Two analyses gave—

	Si	Al	Fe	Ca	Mg	K	H	
1.	58·13	12·21	1·01	9·49	1·83	0·37	15·96	=99·00
2.	57·02	13·11	1·27	10·15	1·91	*undet.*	15·42	=98·88

The iron and potash in No. 1 were dissolved out by chlorhydric acid—a fusion with carbonate of soda was made to complete the decomposition. The loss in the analyses is supposed by Prof. How to be due to alkali not determined. The ratio between R, R, Si, H is as 1 : 2 : 9 : 4, and gives the formula $3CaSi+2AlSi^2+12H$= Si 58·06, Al 14·60, Ca 11·96, H 15·38.

Datholite [p. 334, I—IV, VI].—J. D. Whitney has described a peculiar variety of this mineral, which occurs in nodules in the Minnesota mine, Lake Superior (this Jour. [2], xxviii, 13). The mineral is quite compact and breaks with a conchoidal fracture; it is perfectly white and opaque, resembling in physical character the purest and most close-grained marble. H.=4·5; sp. gr. 2·983. Analysis by C. F. Chandler gave—

Si	Fe, Al	Ca	B (loss)	H
37·41	0·35	25·11	21·40	5·73 = 100.

Q. Sella has in his collection, a crystal of datholite from Baveno, 4½ inches long by 3¾ inches broad, and 1½ inches in thickness.—(Wien Akad. Berichte, xxix, 239.)

Diasporе.—See under *Natrolite.*

Embolite [p. 93].—F. Field has given analyses of three varieties of chloro-bromid of silver from Chañarcillo, in the province of Atacama (Quar. Jour. Chem. Soc., x, 239):

	Ag	Br	Cl	
1.	68·22	16·84	14·92	= 99·98
2.	66·94	19·82	13·18	= 99·94
3.	61·07	33·82	5·00	= 99·89

No. 1 had a pale green color, and its formula is 2AgCl+AgBr. No. 2, of a darker color, and of more frequent occurrence, is identical with Breithaupt's *embolite*, as analyzed by Plattner, 3AgCl+2AgBr. No. 3 was of a very dark green color, sometimes having a purple tint, its formula is AgCl+3AgBr.

Under the names *megabromite* and *mikrobromite* Breithaupt has described two new chloro-bromids of silver (B. u. H. Zeit., xviii, 449). I. *Megabromite.* Lustre adamantine; color siskin to pistachio-green, changing on exposure to the light, to blackish-gray; streak pale green. Crystalline form cubic; cleavage cubic, though not always distinct; fracture conchoidal and uneven; slightly malleable and sectile. H.=2·75—3; sp. gr. 6·230—6·234. Occurs in compact limestone. Analysis by T. Richter:

Ag	Br	Cl	I
64·19	26·49	9·32	*tr.*

The relations of Ag, Br and Cl are as 2·26 : 1·26 : 1, or as 9 : 5 : 4=4AgCl+5AgBr. It bears a very strong resemblance to embolite in physical characters.

II. *Mikrobromite.* Lustre adamantine; color between asparagus and greenish-gray, on exposure becomes ash-gray and opaque; streak white, translucent; crystalline form, cubic; fracture irregular, and without any regular cleavage; very sectile and malleable. H.=2·5—3. Sp. gr. 5·75—5·76. Occurs with native silver in a yellowish-red compact limestone at Copiapo in Chile. Two analyses by Richard Müller gave—

	Ag	Br	Cl
1.	70·28	12·35	17·37
2.	69·81	12·44	17·75

The ratio of Ag, Br and Cl is as 4 : 1 : 3, giving the formula AgBr+3AgCl.

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[In this connection it may be interesting to notice the remarks of Domeyko upon the chloro-bromids of silver (Elementos de Mineralogia, 202). He says: "the chloro-bromids vary in color, from grayish-green or yellow, to asparagus and pistachio-green. In general the specimens that have a yellow color have more bromine, and consequently less silver, than those of a gray or pearly green color." Analyses of three specimens of the yellow variety from the mines of Chañarcillo gave Domeyko:

	AgCl	AgBr		Ag	Br	Cl
1.	51·00	49·00	=	66·53	20·85	12·62
2.	52·80	47·20	=	64·84	20·09	13·07
3.	51·00	49·00	=	66·53	20·85	12·62

The ratio of the atoms of Ag, Br and Cl in Nos. 1 and 3, are as $2\frac{1}{2} : 1 : 1\frac{1}{2}$ or 7 : 3 : 4, giving the formula 4AgCl+3AgBr. No. 2 has the ratio $2\frac{1}{2} : 1 : 1\frac{1}{2}$ or 5 : 2 : 3, giving the same formula as Breithaupt's embolite, 3AgCl+2AgBr. Domeyko also gives four analyses of the grayish green variety which occurs in masses an inch or more in thickness. Of these Nos. 4, 5 and 6, as analyzed below, were from Chañarcillo and 7, from Quillota. All the specimens examined were remarkably homogeneous and pure, some of them as translucent as wax.

	AgCl	AgBr		Ag	Br	Cl	Ratio.
4.	72·9	27·1	=	70·44	11·53	18·03	9 : 2 : 7
5.	65·6	34·4	=	69·14	14·63	16·23	7 : 2 : 5
6.	81·4	18·6	=	71·94	7·92	20·14	20 : 3 : 17
7.	66·4	33·6	=	69·28	14·30	16·42	15 : 4 : 11

If we now recapitulate these together with Field's analyses and the analyses of the three species of Breithaupt, in the order of the increase of bromine, we have:

	Ag	Br	Cl	
Pearly green, Chañarcillo, (6),	71·94	7·92	20·14	Domeyko.
" " " (4),	70·44	11·53	18·03	"
Mikrobromite, Copiapo,	69·84	12·39	17·77	Müller.
Pearly green, Quillota, (7),	69·28	14·30	16·42	Domeyko.
" " Chañarcillo, (5),	69·14	14·63	16·23	"
Light green, "	68·22	16·84	14·92	Field.
Embolite, "	66·94	19·82	13·18	"
" "	66·86	20·08	13·05	Plattner.
" " (2),	66·84	20·09	13·07	Domeyko.
Yellow, " (1 and 3),	66·53	20·85	12·62	"
Megabromite,	64·19	26·49	9·32	Richter.
Dark green, Chañarcillo,	61·07	33·82	5·00	Field.

Here then are ten distinct chemical compounds formed by the union of various proportions of AgCl and BrCl, and so far as known, they all crystallize in the monometric system. As both *bromyrite* (AgBr) and *kerargyrite* (AgCl) crystallize in the monometric system, and as Cl and Br are isomorphous and may replace each other in an infinite number of proportions, it is well to ask, where we shall stop in the making of new species. The five varieties which we have quoted from Domeyko, together with the two varieties analyzed by Field, deserve to rank as species quite as much as embolite, megabromite, and mikrobromite. The varieties of chloro-bromid of silver seem to shade insensibly into each other—the specific gravity increases, and the color deepens in proportion with the increase of bromine. We have already the name embolite admitted in the science, and if the native chloro-bromid should be found which has the ratio 2 : 1 : 1 or AgCl+AgBr, it would have the same right to be ranked a species as dolomite, but we protest against making ten or more species of Ag (Br, Cl), for the same reasons that we should protest against making distinct species of all of the varieties of dolomitic limestone.—G. J. B.]

FERGUSONITE [p. 350, III].—R. Weber has analyzed *fergusonite* from West Greenland (Pogg. Ann., cvii, 590):

Cb	Sn	Zr	Y	Ce	Fe	U	
48·84	0·35	6·93	38·61	3·05	1·48	0·35	= 99·61

This, although differing somewhat from the former analysis by Hartwall, proves the mineral to be distinct from the Norwegian *tyrite*.

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FRANKLINITE [p. 166, I, VII].—The chemical composition of this mineral has been carefully studied by Rammelsberg (Pogg. Ann., cvii, 312), and the causes of error, and the disagreement in former analyses pointed out. His analyses, partly from massive, and partly from crystallised specimens, gave:

G.=5·21		1.	2.	3.	4.	5.
	Fe	64·28	65·32	64·92	63·40	64·64
	Mn		13·08	13·87	13·28	13·31
	Zn			25·09	26·83	25·51
				103·88.	103·51	103·96

Analyses 4 and 5 were made by Schulz in Rammelsberg's Laboratory. The mean is Fe 64·51, Mn 13·51, Zn 25·30 (excluding Zn in No. 4) =103·52, equal to

	Fe	Mn	Zn	O	
	45·16	9·38	20·30	25·16	= 100·00
Equivalents,	4·8	1·	1·8	9·3	

The atomic proportion between the metals and the oxygen is 7·6 : 9·3=1 : 1·2=5 : 6 or R^5O^6. Rammelsberg proves by experiment that at least a *portion* of the manganese must be sesquioxyd, and after the consideration of several hypotheses in regard to the oxydation of the iron and manganese, which show the impossibility of the composition of the mineral corresponding to the spinel formula, he is led to assume the *whole* of the manganese to exist as sesquioxyd. He then calculates the oxygen remaining, after deducting that united with the zinc and manganese, as belonging to the iron. This gives—

	a	Oxygen.		*b*	Oxygen.	
Mn	13·51	4·13	13·61	13·51	4·13	12·38
Fe	31·64	9·48		27·50	8·25	
Fe	29·55	6·55	11·55	33·31	7·38	12·38
Zn	25·30	5·00		25·30	5·00	
	100·00		25·16	99·62		24·76

Column *b* is a correction of the calculation so as to make the oxygen of the protoxyds and sesquioxyds equal, and corresponds to the amount of iron found in the analyses. The oxygen thus calculated differs but 0·4 per cent from the total oxygen.

The calculation of the compound R^5O^6 in which Mn : Zn : Fe=1 : 2 : 5 gives Fe 45·24, Mn 8·92, Zn 21·02, O 24·81, or Fe 64·66, Mn 12·80, ZnO 26·19=103·65, corresponding very well with the results of the analyses, and giving the formula $(\dot{F}e\dot{Z}n)^3(\dot{F}e\dot{M}n)$ or $\dot{R}^3\ddot{R}$. Rammelsberg considers the isomorphism of $\dot{R}\ddot{R}$ with $\dot{R}^3\ddot{R}$ as a necessary consequence of the isodimorphism of the oxyds $\dot{R}$ and $\ddot{R}$, already shown in his recent investigations on hornblende, augite, and the different varieties of specular and titanic iron. [An analysis of *franklinite*, made by the writer in the autumn of 1858, gave results agreeing very nearly with the composition obtained by Rammelsberg, viz. Fe 65·05, Mn 14·77, Zn 23·80, insoluble 0·30=103·12.—C. J. B.]

GALENA [p. 36, II—IV, VII].—The so-called *steinmannite*, already referred to galena by Kenngott (Uebersicht, 1855, 109), has recently been analyzed by Schwarz (*Reuss*, Wien. Akad. Ber., xxv, 561), with the following results:

PbS	As_2S_3	SbS_3	ZnS	FeS	
76·48	9·25	0·77	11·38	2·10	= 99·98

Another specimen gave 2 p. c. less lead, a trace of zinc, and scarcely any arsenic, but contained a large amount of antimony. In a third sample a small portion of silver was found. From these facts Prof. Reuss draws the conclusion that sulphid of lead is the only constant constituent, and that *steinmannite* is an impure galena.

GARNET [p. 190, I—VII].—Analyses of *lime-iron-garnet* from the Schischimsk Mts. (1.), and from Achmatowsk (2.), and the *grossular* from the Slüdianka River (3), made under the direction of N. v. Iwanow in the Mining-Department Laboratory in St. Petersburg gave (Kokscharow, Mat. Min. Russlands, iii, 79):

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		$\ddot{S}i$	$\dot{A}l$	$\dot{F}e$	$\dot{C}a$	$\dot{M}g$	$\dot{M}n$
1.	G.=3·798.	35·21	*tr.*	34·11	30·96	*tr.*	*tr.*=100·28
2.	———	37·22	6·04	24·81	31·07	0·49	*tr.*= 99.63
3.	G.=3·427.	40·99	14·90	10·94	32·94	0·98	—=100·75

An extended notice of the occurrence of *garnet* in Russia, with description of interesting crystals is given by Kokscharow in his Materialen zur Mineralogie Russlands, iii, 1—40.

GERSDORFFITE [p. 58, VII].—Dr. Genth has detected crystals of this mineral occurring as an incrustation upon partially decomposed galena and blende at Phœnixville, Pa. The crystals are cubes with octahedral planes, and sometimes, though rarely, pentagonal dodecahedrons are found.—This Jour., [2], xxviii, 248.

GLASERITE [p. 365, III].—Prof. W. J. Taylor refers to *glaserite*, with a query, a sulphate of ammonia and potash from the Chincha Islands. It occurs in concretions a half or three-quarters of an inch in diameter. Color yellowish-white; structure crystalline; taste pungent and bitter; opaque; permanent in air. Hardness 2. B.B. blackens and fuses with difficulty, giving a white bead. The results of two analyses were:

	$\ddot{S}O_3$	NH^4O	KO	NaO	
1.	48·40	5·37	43·45	1·68	= 98·90
2.	48·30	5·10	46·49		= 99·89

Both specimens contained traces of organic matter. The composition gives the formula $(NH^4O, KO, NaO)\ddot{S}O^3$, which differs from that of glaserite only in having a portion of the potash replaced by ammonia and soda (Proc. Acad. Nat. Sci. Philad., Nov., 1859).

HALLOYSITE [p. 251].—Nöggerath has described as a variety of opal, a mineral occurring in a soft gelatinous state in trachyte in the opal mine at Czerweitza in Hungary (Verhand. d. naturhist. Ver. d. Rheinlande, xv, cii). Upon exposure to the atmosphere the mineral hardens, and its characters approach those of jasper-opal. Analysis by Landolt showed the mineral to lose 5·30 p. c. by drying over sulphuric acid, and the dried substance gave:

$\ddot{S}i$	$\dot{A}l\dot{F}e$	$\dot{C}a$	$\dot{H}$	
46·96	36·56	*tr.*	16·10	= 99·62

[Assuming the iron to be an unimportant ingredient, this composition corresponds to the varieties of *halloysite* from Anglar and Housscha (Min., p. 151, anal. 1, 2).—G. J. B.]

HAYESINE [p. 394, III, IV].—Analyses of very pure *hayesine* by Reichardt (Kopp's Jahresbericht, 1858, 737):

	$\ddot{B}a$	$\dot{C}a$	$\dot{N}a$	$\ddot{S}$	Cl	Insol.	$\dot{H}^b$	$\dot{H}^c$
1.	52·05	11·56	*tr.*	0·53	0·94		33·53	1·38
2.	50·42	12·10	*tr.*	1·07	1·21	0·67	33·67	0·87

(*a*) by the difference. (*b*) expelled at a red heat. (*c*) expelled at 100° C.

No. 1 was a specimen from the German importers, and 2 was received direct from Lima. Reichardt gives the formula $\dot{C}a\ddot{B}^4+10\dot{H}$. An analysis of *hayesine* by F. W. Helbig (Dingler's Polytech. Jour., cxlvii, 319) gave $\ddot{B}$ 46·46, $\dot{C}a$ 14·03, $\dot{N}a$ 5·17, $\dot{H}$ 32·61, NaCl 1·89, $\dot{M}g$ and $\ddot{S}i$ traces. Additional analyses of the commercial article are given in Barreswil's Réperetoire de Chimie Appliquée, i, 215.

HEMATITE [p. 113, II, III, IV, VII].—A specimen of tabular crystalline hematite from Vesuvius analyzed by Rammelsberg (Pogg. Ann., cvii, 453) gave $\dot{F}e$ 98·05 $\dot{M}g$ 1·40=99·45. It contained no protoxyd of iron.

Analyses of hematite from the Lake Superior region by Prof. J. D. Whitney (this Jour., [2], xxviii, 13):

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	I.			II.		III.	
	a.	*b.*	*c.**	*a.*	*b.*	*a.*	*b.*
Insoluble,	1·02	·80	·54	7·92	7·96	1·99	2·05
Iron,	69·41	70·22	69·96	64·42	64·01	68·81	
Oxygen and traces, lime, &c.,	29·57	28·98	29·50	27·66	28·03	29·20	

* Mean of three closely-agreeing determinations.

I. from the Jackson Mountain, II. from the Cleveland, and III. from the Burt or Lake Superior Mountain.

HOMICHLINE [VII].—This mineral was referred to *barnhardtite* in the last supplement, but the recent analysis of Richter, published by Breithaupt in the B. u. H. Zeitung, xviii, 321, gives its composition as Fe 25·81, Cu 43·76, S 30·21, and the formula $(Cu^2S)^2$, Fe^2S^3+2FeS. Sp. gr. 4·47—4·48, (Breithaupt.) [The identity of crystalline form of this mineral with chalcopyrite, together with its less degree of hardness, and the difficulty of obtaining it pure and free from admixture with chalcopyrite, would seem to indicate that it might be a product of decomposition of this latter species, or perhaps a mixture of this species, with some of the richer sulphids of copper, such as erubescite or copper-glance. It is interesting in this connection to note Dr. Genth's remarks upon the occurrence of barnhardtite with copper-glance and chalcopyrite at the Pioneer Mills mine (this Journal, [2], xxviii, 248). —G. J. B.]

HORNBLENDE [p. 170, I—IV, VI, VII].—A. Knop has published a description and several analyses by himself and W. Hoffman of an interesting soda hornblende from the serpentine rock at Waldheim in Saxony (Ann. d. Chem. u. Pharm., cx, 363). Color leek-green; translucent; occurring in veins of an inch in thickness and resembling actinolite. H.=5. Sp. gr. 2·957.

	Si	Al	Fe	Mn	Ca	Mg	Na	
1.	58·71[a]	1·52	5·65	0·25	11·53	10 01	12·38=100·05	Knop.
2.	58·45[a]	1·92	5·53	0·51	10·28	11·12	12·61=100·42	Hoffmann.
3.	58·45[a]	1·74	5·79	0·32	10·76	10·83	12·93=101·12	"

(*a.*) Mean of two determinations.

It lost 0·5 per cent by ignition. [The analyses give too much silica for the hornblende formula, but this may be accounted for on the supposition that the mineral was partially decomposed, when treated by hydrochloric acid to free it from adhering carbonates. The large percentage of soda is remarkable.—G. J. B.]

IODYRITE [p. 95, 506].—An analysis of iodyrite from Delirio's Mine, Chañarcillo, afforded F. Field, Ag 45·98, I 54·02=AgI.—(Quar. Jour. Chem. Soc., x, 241).

IRIDOSMINE [p. 19, I].—Analyses of *iridosmine* from various localities by Deville and Debray (Ann. Chim. et Phys., [3], lvi, 481).

	Ir	Rd	Pt	Ru	Os	Cu	Fe	
1. Columbia,	70·40	12·30	0·10	—	17·20	—	—	=100·
2. "	57·80	0·63	—	6·37	35·10[a]	0·06	0·10	=100·06
3. California,	53·50	2·60	—	0·50	43·40	—	—	=100·
4. Australia,	58·13	3·04	—	5·22	33·46	0·15	—	=100·
5. Borneo,	58·27	2·64	0·15	—	38·94	—	—	=100·
6. Russia,	77·20	0·50	1·10	0·20	21·00	*tr.*	—	=100·
7. " G.=18·9,	43·28	5·73	0·62	8·49	40·11	0·78	0·99	=100·
8. " G.=18·8,	64·50	7·50	2·80	—	22·90	0·90	1·40	=100·
9. " G.=20·4,	43·94	1·65	0·14	4·68	48·85	0·11	0·63	=100·
10. " G.=20·5,	70·36	4·72	0·41	—	23·01	0·21	1·29	=100·

(*a.*) In this analysis, the osmium was determined directly, in the others by difference.

IRON [p. 17, II, VII].—F. A. Genth describes in this Journal, [2], xxviii, 246, a specimen of what appears to be telluric iron. It is said to occur near Knoxville, in Tennessee, although its exact locality is not known. It contains neither carbon, phosphorus or sulphur, and its peculiar appearance together with its being associated with a silicate of magnesia, iron and lime, render it probable that it may be a genuine specimen of *native iron*. Dr. Genth describes the mass examined, to have been about one and a half inches square, and three-eights of an inch in thickness. The

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iron had a grayish-white color, a hackly fracture, and broke easily into fragments, which though crystalline, did not show any distinct planes. It was soft, scarcely scratching fluor-spar. Lustre eminently metallic. Readily dissolved by nitric acid. Composition:—

Fe	Ni	Co	Mg	Ca	Si	
99·790	0·140	*tr.*	0·022	0·121	0·075	= 100·148

A similar mineral has been received by Dr. Genth from Northern Alabama, and it is exceedingly desirable that more definite information should be obtained in regard to the locality and mode of occurrence of this problematical substance.

KERARGYRITE [p. 92, IV].—A specimen of chlorid of silver from the "Republicana Mine," Chañarcillo, analyzed by F. Field (Quar. Jour. Chem. Soc., x, 239) contained:

Ag	Cl		
75·27	24·73	=	AgCl.

LABRADORITE [p. 237, II, VII].—Vom Rath gives as the composition of the *labradorite* from the gabbro of Marmorera in Graubündten:

	Si	Al	Fe	Ca	Mg	K	Na	
G.= 2·840.	55·45	22·12	4·28	9·68	1·30	1·64	5·73	=100·20

This mineral lost 2·76 p. c. on ignition.—Zeitsch. d. deutschen geolog. Gesellschaft, ix, 246.

LIBETHENITE [p. 420].—Analyses of *libethenite* from Congo in Portuguese Africa by Hugo Müller (1.) (Quar. Jour. Chem. Soc., xi, 242); from Libethen (2.) by Bergemann, and from Nischne-Tagilsk (3.) by Chydenius (Kopp's Jahresbericht, 1858, 726):

	Cu	P	As	H	Fe	O	
1. Congo,	67·21	28·76		4·03			=100·00
	66·76	29·02		4·22			=100·00
2. Libethen,	66·29	26·46	2·30	4·04			= 99·09
3. Nischne-Tagilsk,	64·47	29·48	*tr.*	3·68	1·77	0·82	=100·22

LILLITE, *Reuss* (Wien Akad. Ber., xxv, 550).—This name has been given by Reuss to a mineral which occurs at Przibram in Bohemia. In physical characters it resembles glauconite, and appears to be a product of the decomposition of pyrites. It is an amorphous, lustreless, earthy substance, having a hardness = 2, and sp. gr. 3·043. Color blackish-green, in very fine powder under the microscope is leek-green by transmitted light. Material selected as pure as possible gave Payr on analysis:

Si	Fe	CaO	FeS (insol.)	H	
32·48	54·95	1·96	0·63	10·20	= 100·22

On treating the mineral with nitric acid, Payr found after ignition and making allowance for the water, the sulphur of the pyrites, and the carbonic acid, that the iron in the mineral had absorbed 3·43 per cent of oxygen, so that deducting this from the 54·95 Fe we have 51·52 per cent of iron plus oxygen. This gives a total of 96·79, a loss of 3·21 per cent in the analysis. The author places the species near hisingerite and cronstedite. It corresponds very closely in chemical composition with the variety of hisingerite from Riddarhyttan in which Rammelsberg found (Min., p. 290):

Si	Fe	Fe	Ca	Mg	H
33·07	34·78	17·59	2·56	0·46	11·54

MAGNESITE [p. 447, II, III].—Analyses of the magnesite from Snarum and Frankenstein by T. Scheerer (Jour. f. prakt. Chem., lxxvi, 424):

	O	Mg	Fe	Ca	
Snarum (crystallized),	52·13	46·66	0·78	0·43	= 100·00
Frankenstein (amorphous),	55·34	47·43	—	0·22	= 100·00

The small amount of mechanical mixture, amounting in the Snarum specimen to 0·05—0·1405 per cent, and in the Frankenstein specimen to 0·048 p. c., have been subtracted from the above.

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MAGNOFERRITE.—Rammelsberg (Pogg. Ann., cvii, 451) gives this name to the octahedral iron which occurs interlaminated with hematite, in the fumaroles formed at Vesuvius after the eruption of 1855. His former analyses showing the presence of a considerable amount of magnesia are contained in Suppl. VII. Two additional analyses of portions selected out by means of the magnet from the finely pulverized mineral gave:

		Ḟe	Ṁg	Ċu	Insol.	
1.	G.=4·568	82·91	13·60	0·99	2·51	= 100·01
2.	G.=4·638.	83·30	13·41	0·59	2·00	= 99·30

which, excluding the oxyd of copper and insoluble portions, gives (1.) Ḟe 85·92, Ṁg 14·09=100·01, and (2.) Ḟe 85·51, Ṁg 13·77=99·28.

The former analyses (Suppl. VII) thus calculated are: *a.* Ḟe 86·96, Ṁg 12·59= 99·55; *b.* Ḟe 84·20, Ṁg 16·00=100·20; *c.* Ḟe 84·35, Ṁg 15·65=100. Analysis *a* was made from selected crystals, *b* was a portion extracted by the magnet from the associated hematite, while *c* was a specimen thus selected from one of the older Vesuvian hematites. Rammelsberg considers the composition of the crystals as $\dot{M}g^m\ddot{F}e^n$ in which probably $m=3$ and $n=4$, the regular (monometric) form being due to the isodimorphism of Ṙ and R̈.

MARGARODITE [p. 223].—An analysis of an authentic specimen of margarodite from the original locality at Pfitsch gave Hlasiwetz (Kenngott's Uebersicht, 1858, 67):

S̈i	Ä̇l	Ḟe	Ċa	K̇	Ṅa	Ign.	
45·48	33·80	6·25	0·48	7·31	6·22	0·36	= 99·90

Kenngott remarks that this composition may be represented by the formula $\dot{R}\,\ddot{S}i + 2\ddot{R}\,\ddot{S}i$, but adds that this is of little value, as on closer examination with the magnifier, the specimen proved to be an intimate mixture of a mica with granular quartz and minute crystals of feldspar. [The mica from Lane's Mine, analyzed by Smith and myself, and referred by Dana to margarodite, is distinctly foliated and apparently perfectly homogeneous. It is identical in composition with the so-called margarodite from St. Étienne analyzed by Delesse (Min., p. 224).—G. J. B.]

MARIONITE (*Elderhorst*), see *Zinc-bloom*.

MEGABROMITE (*Breithaupt*), see *Embolite*.

MIKROBROMITE (*Breithaupt*), see *Embolite*.

MISPICKEL [p. 62, 509, I, II, III, V].—Analyses of mispickel from Sahla in Sweden by J. Potyka (Pogg. Ann., cvii, 304):

	S	Fe	As	Sb	Bi	
G.=6·095.	19·18	34·78	43·26	1·29	0·14	= 98·60

These results give the received formula, $FeAs_4 + FeS_2$, differing from the analysis by Behncke (Suppl. III), which corresponded to $3FeS^2 + 2Fe^3As^2$. Potyka shows the want of agreement between the analyses to be due to the fact that mispickel suffers partial decomposition by simply boiling in water. The sp. gr. of small fragments he found to be 6·043—6·047, that of the powder boiled for some time in water was only 5·819 to 5·874, and on examination of the water appreciable quantities of sulphuric acid, iron and arsenic were found in solution.

C. v. Hauer obtained in two analyses of mispickel from Kindberg in Styria:

S̈i	Ä̇l	Ċa	Fe	As	S	
5·0	1·0	0·3	30·8	43·2	18·9	= 99·2
0·7	0·3	*tr.*	32·7	45·0	21·0	= 99·7

—Jahrb. d. k. k. geolog. Reichsanstalt in Kopp's Jahresbericht f. 1858, p. 678.

According to Daubrée (Compt. Rendus, 1858, xlvii, 959) the lignite of the tertiary formation at Lobsann (Lower Rhine) contains from 0·002—0·0008 per cent of arsenic, and on dissolving the bituminous limestone from the same locality a fine amorphous residue (amounting to about 2 p. c.) is obtained, which gives the reactions of mispickel.

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MOLYBDATE OF IRON [p. 144, I, II].—The so-called *molybdate of iron* described by D. D. Owen has recently been examined by Dr. Genth (this Jour., [2], xxviii, 248), and from the varying proportions of the iron—in one case 35 p. c., in another 24·3—he questions whether the substance may not be a mechanical mixture of molybdine and limonite.

MOSSOTTITE.—See *Aragonite*.

NAGYAGITE [p. 65].—Folberth has analyzed the foliated-tellurium from Nagyag. It occurs in six-sided tables in a pearl-gray quartz, and has a specific gravity =6·68. Treatment with sulphid of carbon extracted 25 p. c. of the amount of sulphur. Two analyses gave (Verhandl. d. siebenbürg. Ver. f. Naturwissensch., viii, 99, in Kenngott's Uebersicht f. 1856–7, 179):

Pb	Au	Sb	S	Te	Se
60·83	5·84	3·69	9·76	17·22	*tr.* = 97·84
60·27	5·98	3·86	9·68	18·04	*tr.* = 97·83

differing very materially from the previous analyses by Klaproth, Brandes and Schönlein.

NATROLITE [p. 327, VI, VII].—A variety of translucent natrolite from Fassa in Tyrol, analyzed by Hlasiwetz, gave the following composition (Kenngott's Uebersicht, 1858, 72):

$\dot{S}i$	$\dot{A}l$	$\dot{C}a$	$\dot{M}g$	$\dot{N}a$	$\dot{H}^a$	$\dot{H}^b$
43·34	27·43	3·60	0·40	9·00	10·30	0·90 = 99·97

(*a*) basic water. (*b*) hygroscopic water.

agreeing very nearly with the composition of galactite, which has been shown to be a variety of natrolite by Dana and Heddle (Suppl. I and III).

In an article upon *Spreustein* (natrolite) (Pogg. Ann., cviii, 431) Scheerer shows the cause of color, of the red and brown varieties of this mineral, to be due to mechanical impurities. A microscopical examination of several varieties showed that only the perfectly white specimens were entirely free from mechanical mixture. The white varieties were perfectly decomposed by chlorhydric acid giving a homogeneous jelly, while with the colored varieties the gelatinized mass always contained suspended more or less of an opaque white powder. If however, the decomposition was made with nitric acid, this powder retained the original color of the natrolite. A separation of the insoluble powder on some twenty grams of the mineral gave material to determine the character of this substance. The results of two analyses prepared from different varieties gave:

	$\dot{S}i$	$\dot{A}l$	$\dot{F}e$	$\dot{H}$
1.	1·58	76·75	6·77	14·70 = 99·80
2.	0·82	82·56	1·52	15·00 = 99·90

These give the formula $\dot{R}\dot{H}$, and the powder is *diaspore* in which a portion of the alumina is replaced by iron. The quantity of this mineral in the specimens of natrolite analyzed by Scheerer varied from 4 to 7 p. c. This will explain the reason of the different analyses of Spreustein differing from each other, and also from pure natrolite. The following analyses may serve as examples. I. Crystallized colorless natrolite from Brevig analyzed by Dr. Sieveking in Scheerer's laboratory. II. Dark brownish-red Spreustein from an island of Brevigfjord, by Scheerer.

	$\dot{S}i$	$\dot{A}l$	$\dot{F}e$	$\dot{C}a$	$\dot{N}a$	$\dot{H}$
I.	47·16	26·13	0·53	0·53	15·60	9·47 = 99·42
II.	44·50	30·05	0·98	0·83	13·52	9·93 = 99·81

II. contained 6¼ p. c. of diaspore, which when subtracted from the above gave $\dot{S}i$ 47·47, $\dot{A}l$ 26·83, $\dot{F}e$ 0·60, $\dot{C}a$ 0·88, $\dot{N}a$ 14·42, $\dot{H}$ 9·61=99·81; this is almost exactly the composition obtained for I. Scheerer adds that the so-called *brevicite* (Min., 327, anal. 14, 15, and 16) is nothing more than natrolite, which contains a considerable percentage of diaspore. For the further consideration of the disputed points upon the paramorphous nature of *Spreustein* (Palão-Natrolith) see the original paper in Pogg. Ann., cviii, pp. 416—435.

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NEPHELINE [p. 232, II].—P. v. Pusirewsky has analyzed the *elæolite* which is associated with cancrinite, zircon and other minerals in the Graphite-Mine at Mariinskaja in the Tunkinsk Mts. (Koksharow Mat. Min. Russlands, III, 78), and J. P. Kimball has described the same variety of nepheline, occurring with sodalite at Salem, Mass. (this Jour., [2], xxix, 65):

	Si	Al	Fe	Ca	Mg	Na	K	Ign.	
1. Mariinskaja,	44·94	30·29	0·72	1·15	0·15	21·80	1·48	——	=100·53 P.
2. Salem, G.=2·63.	44·31	32·80	*tr.*	0·40	——	16·43	5·50	1·47	=100·91 K.

Nickel and Copper, arseniuret of.—This ore, mentioned by T. Sterry Hunt (this Jour., [2], xix, 417) as a mixture of *domeykite* and *copper-nickel*, has since been thoroughly examined by both Prof. Hunt (Rep. Geol. Survey, Canada, 1853–6, p. 388) and Prof. J. D. Whitney (this Jour., [2], xxviii, 15) giving analyses which confirm the above conclusion.

NICKEL-GYMNITE [p. 286, VII].—An ore, apparently an impure variety of this mineral, is described by T. Sterry Hunt (this Jour., [2], xix, 417, and Rep. Geol. Survey, Canada, 1853–6, p. 389), as occurring with the nickel ores of Michipicoten Island, Lake Superior.

OLIGOCLASE [p. 239, I].—Vom Rath has given analyses of *oligoclase* from the granite of Albulaberge, and also of a compact *lime oligoclase* from the diorite of Piz-Rosag, in Granbündten (Zeitschr. d. deutsch. geol. Gesell., ix, 226, 259):

	Si	Al	Fe	Ca	Mg	K	Na	
Albulaberge, G.=2·725.	62·01	21·16	2·54	3·53	0·78	4·33	5·94	=100·29
Piz-Rosag, G.=2·835.	57·64	22·99	3·92	8·09	0·37	1·79	5·25	=100·05

The analyses were made on the ignited mineral. The specimen from Albulaberge lost 1·05 per cent, and that from Piz-Rosag 1·32 per cent on ignition.

The leek-green *feldspar* associated with pyrrhotine at Bodenmais (Bavaria) has been analyzed by Potyka (Pogg. Ann., cviii, 366). It is triclinic, and has the characteristic striæ on the cleavage surfaces. Sp. gr. 2·604. Composition:

Si	Al	Fe	Ca	Mg	K	Na	
63·12	19·78	1·51	0·66	0·13	12·57	2·11	= 99·87

This gives an oxygen ratio of R : R : Si, of 1 : 2·86 : 10·17 or nearly 1 : 3 : 10,—a ratio intermediary between that of oligoclase and orthoclase; the sp. gr. (2·604) is also between that of oligoclase (2·56) and orthoclase (2·67).

ORTHOCLASE [p. 242, II, III, V—VII].—J. D. Whitney has analyzed the interesting crystallized *orthoclase* which is associated with native copper, calcite and the zeolites in many of the Lake Superior copper mines (this Jour., [2], xxviii, 16). It occurs in distinct crystals of a reddish flesh color, having a striking resemblance to stilbite. The crystals are rarely as much as one-tenth of an inch in length. Composition:

Si	Al	Fe	Mn	K	Na	
65·45	18·26	0·57	*tr.*	15·21	0·65	= 100·14

OKENITE (*Breithaupt*), see *Aragonite*.

PECTOLITE [p. 305, II, III, VI, VII].—Analyses of three specimens of the very pure variety of this mineral from Bergen Hill, N. J., by J. D. Whitney (this Jour., [2], xxix, 205):

	Si	Ca	Mn	Fe	Na	H[a]
1.	54·82	33·12	0·66	2·6	8·78	2·36
2.	54·76	32·88	1·16		9·17	2·03
3.	54·27	32·83	1·24		8·94	2·72

(*a*) By the difference.

The direct determination of the water on the substance dried at 80° C. gave, for 2, 3·03, and for 3, 2·75 p. c. These results agree very closely with the previous analysis published by Prof. Whitney (Min., anal. 5). Specimen 3, from the Wheatley Collection in Union College, was considered the purest, and the analyses gives

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the oxygen ratio for Ḣ, Ṅa, Ċa, S̈i as 1 : 1·05 : 3·83 : 11·84 or nearly 1 : 1 : 4 : 12, or the formula, as expressed by Prof. Whitney, $\dot{N}a^2\ddot{S}i^4 + 4\dot{C}a^2\ddot{S}i^3 + 3\dot{H} = \ddot{S}i$ 54·22, Ċa 33·78, Ṅa 9·33, Ḣ 2·74. This corresponds much better with the results obtained than v. Kobell's formula, in which the oxygen ratio is 1 : 1 : 4 : 11.

Prof. Whitney calls attention to the relations of pectolite to spodumene, and also to wollastonite and pyroxene, the latter connection being more apparent when the formula is written $(\dot{C}a\frac{4}{5}, \dot{N}a\frac{1}{5}, \dot{H}\frac{1}{5})^3\ddot{S}i^2$ or $\dot{R}^3\ddot{S}i^2$.

Pennine [p. 295, II, IV, V].—A new analysis of *pennine* from Zermatt by Victor Merz, gave (Kenngott's Uebersicht f. 1858, 63):

S̈i	Ä̇l	Ḟe	Ṁn	Ṁg	Ḣ
33·26	11·69	7·20	*tr.*	35·18	12·18 = 99·51

not differing materially from the previous results of Marignac, Schweizer and MacDonnel. For an extended discussion of the chemical composition of this mineral by Dr. Kenngott, see loc. cit., pp. 62—66.

Pholerite (p. 251].—F. A. Genth describes (this Jour., [2], xxviii, 251) *pholerite* as occurring in the coal mines of Schuylkill Co., Pa., in yellowish white scales which become of a snow white color, and pearly lustre, on being treated with chlorhydric acid. Under the microscope the scales appear to be clinorhombic, having the planes $i\text{-}i$ and $-1\text{-}i$. The specimens examined were from Tamaqua near Pottsville. Analysis No. 1, was made on the original mineral, in Nos. 2 and 3 the substance had been previously treated with chlorhydric acid. Nos. 1 and 2, were decomposed by fusion with carbonate of soda—No. 3 by treatment with sulphuric acid.

	S̈i	Ä̇l	Ḟe	Ċa	Ṅa	K̇	Ḣ
1.	46·93	37·90	0·18	0·93	*undetermined*		13·98 = 99·92
2.	46 98	39·65	—	—	0·11	0·06	13·69 = 100·49
3.	46·81	39·56	—	—	0·11	0·06	13·91 = 100·45

giving the formula $\dot{Al}^6\ddot{S}i^4 + 6\dot{H} = \ddot{S}i$ 47·06, Ä̇l 39·20, Ḣ 14·71.

Phosphochalcite [p. 425, II, VI, VII].—Bergemann has found arsenic acid in all the native phosphates of copper.—Analysis of the *phosphochalcite* from Linz gave:

	Ċu	P̈	Ä̇s	Ḣ
Phosphochalcite,	69·97	19·89	1·78	8·21 = 99·85

(Abstract from Pogg. Ann., civ, 190, in Kopp's Jahresbericht, 1858, 726).

Pitchblende [p. 107, IV, V].—Hermann has given (Jour. f. prakt. Chem., lxxvi, 326) the name *uranoniobite* to the crystallized pitchblende from Strömsheien in Norway, previously described and analyzed by Scheerer (see Min., p. 108, anal. 5, under pitchblende). Scheerer remarks in his description (Pogg. Ann., lxxii, 568) that it is possible that the metallic acids found in the analysis may be due to admixture with a substance he calls *Niob-pelopsaures Uran-Manganoxydul* (columbate of uranium and manganese) with which the pitchblende is associated;—further investigation is needed to establish its claims to be considered a distinct species.

Hermann also gives a new analysis of the *pitchblende* from Joachimsthal:

	PbS	S̈i	Ä̇l	Ḟe	B̈i	Ü	U̇	Ṗb	Ṁn	Ċa	Ṁg	Ḣ
G.=6·97.	2·84	2·45	0·33	1·88	1·23	52·37	28·84	0·74	0·14	5·78	0·41	2·59

with traces of arsenic. In the same paper Hermann communicates an analysis of the so-called *pittinite* (Pittinerz, Breithaupt) from Joachimsthal. The mineral occurs in amorphous opaque masses of a pitch-black color. It has an uneven and slightly conchoidal fracture, and a highly resinous lustre. Streak greenish-brown. H.=4. Sp. gr. 5·16. Heated in tube yields water containing traces of fluorine and ammonia; fused with soda on charcoal gives a globule containing lead and bismuth. Easily decomposed by nitric acid with separation of gelatinous silica on evaporation. Composition:

S̈i	Ü	Ḟe	B̈i	Ṗb	Ċa	Ṁg	Ḣ	Insol.
5·00	68·45	4·54	2·67	2·51	2·26	0·55	10·06	3·20 = 99·23

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with traces of fluorine, ammonia, phosphoric and carbonic acids. [This substance is evidently very nearly related to pitchblende, and is probably a result of the alteration of that mineral. Hermann endeavors to show that silica is an essential constituent of pitchblende and allied uranium minerals, but as most of these substances are amorphous, and as their composition varies considerably, it seems possible that the silica may be due to admixture with some earthy silicate.—G. J. B.]

PITTINITE.—See *Pitchblende.*

PLATINUM [p. 12, I—IV].—Analyses of platinum from various localities, by H. St.Claire Deville and H. Debray (Ann. de Chimie, [3], lvi, 449):

	Pt	Ir	Rh	Pd	Au	Cu	Fe	X[a]	Sand	Pb(?)	Os & loss	
1.	86·20	0·85	1·40	0·50	1·00	0·60	7·80	0·95	0·95	—	—	=100·25
2.	80·00	1·55	2·50	1·00	1·50	0·65	7·20	1·40	4·35	—	—	=100·15
3.	76·82	1·18	1·22	1·14	1·22	0·88	7·42	7·98	2·41	—	—	=100·28
4.	85·50	1·05	1·00	0·60	0 80	1·40	6·75	1·10	2·95	—	—	=101·15
5.	79·85	4·20	0·65	1·95	0·55	0·75	4·45	4·95	2·60	—	0·05	=100·00
6.	76·50	0·85	1·95	1·30	1·20	1·25	6·10	7·55	1·50	0·55	1·25	=100·00
7.	51·45	0·40	0·65	0·15	0·85	2·15	4·30	37·30	3·00	—	—	=100·25
8.	45·70	0·95	2·65	0·85	3·15	1·05	6·80	2·85	35 95	—	0 05	=100·00
9.	59·80	2·20	1·50	1·50	2·40	1·10	4·30	25·00	1·20	—	0·80	=100·00
10.	61·40	1·10	1·85	1·80	1·20	1·10	4·55	26·00	1·20	—	—	=100·20
11.	77·50	1·45	2·80	0·85	*undet.*	2·15	9·60	2·35	1·00	—	2·30	=100·00
12.	76·40	4·30	0·30	1·40	0·40	4·10	11·70	0·50	1·40	—	—	=100·50

(*a.*) Iridosmine.

Nos. 1, 2, and 3 from Choco (Columbia), South America; 4, 5, and 6 from California; 7, Oregon; 8, Spain; 9, 10, Australia; 11, 12, Russia.

For analyses of platinum ore from Goenoeng Lawack in Borneo by Prof. Bleekrode see Pogg. Ann., cvii, 189.

PYRITES [p. 54, I, IV].—G. Rose has described a pseudomorph of *pyrites* after *pyrrhotine*, the crystals are six-sided prisms, two inches across and one inch in thickness (Zeitschrift d. deutsch. geolog. Gesellschaft, x, 98).

PYROMORPHITE [p. 400, II, IV].—Analyses of Russian *pyromorphite* by Struve (Kokscharow, Mat. zur Min. Russlands, iii, 42):

		$\dot{P}bCl$	$\dot{P}b$	$\dot{F}e, \dot{C}r$	$\dot{A}s$	$\ddot{V}$	$\dot{P}$	
1. Beresowsk,	G.=6·715.	9·94	73·36	0·59	—	*tr.*	15 82	= 99·71
2. Altai (Tomsk),	G.=5·537.	10·13	73·40	—	2·61	—	12·90	= 99·04

PYROXENE [p. 158, I, II, V—VII].—Reuss has described a compact white *pyroxene* from Oberrochlitz in Bohemia. Under the microscope it shows a crystalline structure. The mineral is snow white when pure, but sometimes has a light green color from admixture with chrysocolla, malachite and allophane. H.=5·5—6. G.=3·398. Decomposed by chlorhydric acid with gelatinization. Analysis by v. Payr (Wien Akad. Ber., xxv, 557):

$\ddot{S}i$	$\dot{C}a$	$\dot{M}g$	$\dot{F}e$	$\dot{M}n$
55·03	20·72	15·71	4·84	3·16=99·46

PYRRHOTINE [p. 50, I, II].—Analysis of *pyrrhotine* from Bernkastel on the Moselle by Baumert gave Fe 61·0, S 39·4, and no nickel (Verhandl. d. naturhist. Ver. d. Rheinlande u. Westphalens, xiv, s. lxxxv). For observations on nickeliferous pyrrhotine from Snarum see Müller in B. u. H. Zeitung, xvii, 304.

QUARTZ [p. 145, II—IV, VII].—Blum and Carius have described *quartz* as pseudomorph of *celestine*, from Girgenti. The crystals contained $\ddot{S}i$ 98·80, $\dot{S}r\ddot{S}$ 1·78 (Pogg. Ann., ciii, 628, in Kopp's Jahresbericht, 1858, 745).

REALGAR [p. 31, VI].—Analysis of *realgar* from Pola de Lena in Asturia, Spain, by Dr. Hugo Müller (Quar. Jour. Chem. Soc., xi, 242) gave S 30·00, As 70·25.

RIPIDOLITE [p. 296, I, V].—An interesting and peculiar variety of this mineral from Steele's Mine, Montgomery Co., North Carolina, has been described by Dr. F. A. Genth in this Journal, [2], xxviii, 250. Composition:

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Si	Al	Fe	Fe	Mn	Mg	H
24·90	21·77	4·60	24·21	1·15	12·78	10·59

SAPONITE (Nicklès).—For a more extended description of this silicate noticed in the Suppl. VII, see Ann. de Chimie, [3], lvi, 46.

Saualpite.—A synonym for a variety of zoisite from the Saualp in Carinthia.

SCHEELITE [p. 347].—F. A. Genth has found *scheelite* at the Bangle Mine in Cabarras Co., and also at Flowe mine, Mecklenburgh Co., North Carolina. At the former place it occurs in granular masses three-fourths of an inch in diameter; it has a pale yellowish-brown color, and a distinct octahedral cleavage. Composition (this Jour., [2], xxviii, 252):

W	Sn	Cu	Fe	Ca	
79·52	0·13	0·08	0·18	19·31	= 99·22

The variety from Flowe mine was observed in crystals, in one case a modification of the octahedron 1, truncated by 1-*i*,—crystal about three-tenths of an inch in length; another specimen, half this size, had an orange color and was a combination of the planes $\frac{1}{2}$ and *i*-*i*.

Another variety from Flowe mine, forming what Dr. Genth calls *rhombic tungstate of lime*, occurs in small indistinct crystals—the largest one-quarter of an inch long. Each crystal has a nucleus of wolfram, and the following planes are given: *I*, *i*-$\frac{1}{2}$, $\frac{1}{2}$-$\frac{1}{2}$, 1, and 1-$\frac{1}{2}$; cleavage could not be observed. Dr. Genth does not believe these crystals to be pseudomorphs, and suggests that tungstate of lime is dimorphous,—a conclusion which, though extremely interesting, we hesitate to accept until the subject has been more fully investigated.

SERPENTINE.—Observations on the crystalline structure of serpentine by Websky in Zeit. d. deutschen geol. Gesellsch., x, 277.

SMITHSONITE [p. 447, I, III, VII].—For analyses of zinc ores from Arkansas by Dr. Elderhorst see First Geological Report of Arkansas, pp. 147–155.

SODALITE [p. 229, II, VI].—J. P. Kimball has published a description and analysis of sodalite, from an erratic block of compact syenite at Salem, Mass. (this Jour., [2], xxix, 67). The mineral was associated with elæolite, orthoclase, biotite, zircon, and albite (?) Occurs in crystalline, sub-translucent masses; cleavage indistinct; lustre greasy; color lavender-blue. Sp. gr. on three specimens 2·294, 2·303, 2·314. Chemical composition:

Si	Al	Fe	Na	Cl	
37·33	32·70	*tr.*	24·31	6·99	= 101·33

Calculating the chlorine to exist as chlorid of sodium we have:

Si	Al	Fe	Na	Na	Cl	
37·33	32·70	*tr.*	18·17	4·57	6·99	= 99·76

corresponding very closely to the analyses of the sodalite from Litchfield in Maine by Whitney (Min., anal. 5, 6). Dr. Kimball remarks that the sodalite from both Litchfield and Salem, is found in erratic blocks, but the absence of cancrinite as an associating mineral in the Salem specimens, would seem to favor their being derived from different sources.

STROMEYERITE [p. 48].—Prof. W. J. Taylor has described and analyzed a variety of *stroymeyerite* occurring at Copiapo in Chile (Proc. Acad. Nat. Sci. Phila., Nov. 1859). It is found in small six-sided trimetric crystals not larger than one-eighth of an inch in diameter. Its hardness is 2·5—3. Lustre metallic; color dark steel-gray; streak nearly black and shining. Sectile, crystals brittle. It occurs in barytes in small cavities associated with quartz crystals, and upon the latter are implanted the crystals of stroymeyrite, together with small crystals of pyrargyrite. Analysis gave:

S	Ag	Cu	Fe		
16·35	69·59	11·12	2·86	=	99·92

This composition differs materially from the published analyses of stroymeyerite, although not more than the analyses of specimens from different localities vary

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from each other. Cu and Ag appear to replace each other in this mineral in all proportions. The formula is (Cu, Ag, Fe)S.

TALKOID, *Naumann* (Mineralogie, 5te, Aufl. 255).—The sparry crystalline talc from Presnitz described by Scheerer (Pogg. Ann., lxxiv, 321, this Jour., [2], xiv, 39) has been named *talkoid* by Naumann. It is snow white and broadly foliated occurs with magnetite at Presnitz. Sp. gr. 2·48. Composition, according to Scheerer and Richter:

	Si	Al	Fe	Mn	Ca	Mg	H	
1.	58·46	0·09	1·09	——	0·61	32·83	6·56	= 99·64
2.	58·70	0·06	1·01	0·39	0·81	32·07	6·56	= 99·50

for which Naumann gives the formula Mg^3Si^4+H.

TANTALITE [p. 351, III—VI].—A. E. Nordenskiöld has analyzed tantalite from a new locality at Björtboda in Finland (Pogg. Ann., cvii, 374):

Ta	Sn	Fe	Mn		
83·79	1·78	13·42	1·63	=	100·62

the oxygen ratio between the bases and the metallic acids is 1 : 4·83, most nearly resembling the composition of the Tammela tantalite.

TENNANTITE [p. 84, II].—Vom Rath has published the following analyses of tennantite from Cornwall (Verhandl. d. naturhist. Ver. d. Rheinlande u. Westphalens, xv, s. lxxii, in Kopp's Jahresbericht f. 1858, 680):

	Density.	S	Cu	Fe	Zn	As	
1*a*.	4·652.	25·22	46·88	6·40	1·33	18·72	= 98·55
1*b*.	——	27·13	44·43	6·88	1·43	20·13	= 100·00
2.	4·69.	26·84	52·97	2·82	——	18·06	= 100·19

No. 1*a* is the direct result of the analysis—the mineral was associated with black oxyd of copper, and assuming the amount of this substance to be seven per cent, and averaging the analysis to one hundred, gives the result as in No. 1*b*. Analysis No. 2 was by Baumert. According to v. Rath, the ratio between the metallic sulphids and the sulphid of arsenic in tennantite is 5 : 4, while the analogous ratio in tetrahedrite is 4 : 3.

TOURMALINE [p. 270, II, IV, VII].—Jenzsch (Pogg. Ann., cviii, 648) has examined a crystal of *tourmaline* from Elba which he considers to be optically bi-axial. He suggests, from his investigations, that although the tourmaline crystals from Elba and Penig (Saxony) approach very nearly the hexagonal form, that they belong either to the trimetric or monoclinic system—a view previously suggested by Breithaupt's measurements. Breithaupt publishes a preliminary notice in the Berg und Hüttenmannische Zeitung, xix, 93, of a forthcoming monograph on this subject.

TRIPHYLINE [p. 406, 513].—F. Oesten obtained from the analysis of a very pure specimen of triphyline from Bodenmais in Bavaria (Pogg. Ann., cvii, 438):

	P	Fe	Mn	Ca	Mg	Li	K	Na	Si	
G.=3·545—3·561.	44·19	38·21	5·63	0·76	2·39	7·69	0·04	0·74	0·40	=100·05

This gives the oxygen ratio between the bases and phosphoric acid 15·34 : 24·77 = 3·09 : 5, and the formula, R^3P, the same as first proposed by Fuchs. Wittstein, in a recent note (Pogg. Ann., cvii, 511), calls attention to the fact that eight years since he published results giving the above formula, and says moreover, that a portion of the iron exists as sesquioxyd. Oesten has since (Pogg. Ann., cviii, 648) published proof that the specimen he examined was entirely unaltered, and that all the iron existed as protoxyd.

TYRITE (?) [I, III, IV].—Potyka (Pogg. Ann., cvii, 590) has analyzed specimens of supposed tyrite from Norway which prove to be a new columbate containing several per cent of potash, and distinct from the tyrite of Forbes. The chemical composition was found to be:

Cb	Zr	W	Sn	Pb	Cu	Y	Ce	Fe	U	Ca	Mg	K	H	
43·49	0·80	1·35	0·09	0·41	0·35	31·90	3·68	1·12	4·12	1·95	*tr.*	7·23	3·71	=100·20

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The ratio between the metallic acids and bases, exclusive of the water is, as 1 : 1·04 or $\dot{R}^3\ddot{C}b$. The mineral occurs implanted in red feldspar in small irregular masses having an uneven fracture, but no distinct cleavage. Lustre, sub-metallic; color black, in thin splinters reddish-brown and translucent on the edges; streak reddish-brown; hardness that of apatite (5). Sp. gr. in coarse powder =5·124 (16·6° C.). When hot water is poured upon fragments a crepitation or crackling takes place. B.B. with borax gives a reddish-yellow bead while hot, which on cooling becomes yellow; with salt of phosphorus is completely dissolved to a greenish-yellow bead while hot, becoming green on cooling. No reaction for manganese with soda. Treatment with concentrated sulphuric acid gave no reaction for fluorine. [This mineral corresponds in many of its physical and blowpipe characters with the *bragite* of Forbes (see Suppl. III). Possibly a thorough analysis of authentic specimens of bragite would show them to be very nearly related, if not identical. —G. J. B.]

Uranium, silicates of, see Hermann's paper in Jour. f. prakt. Chem., lxxvi, 320.

Uranoniobite (*Hermann*), see *Pitchblende*.

URANOCHALCITE, *Hermann*.—This name has been given by Hermann to a mineral from Joachimsthal (Jour. f. prakt. Chem., lxxvi, 321). It occurs in reniform amorphous masses having a metallic appearance. Fracture compact, and slightly conchoidal, with a feeble metallic lustre; brittle; opaque; color between steel-gray and pinchbeck-brown; streak black. H.=4. Sp. gr. 5.04. Heated in a closed tube the mineral at first gives off water, and then a sublimate of realgar, and finally metallic arsenic, leaving a black residue consisting chiefly of bismuth, uranium, copper, and iron. Treated with nitric acid the mineral is dissolved with separation of sulphur. On evaporation of the solution, silica separates in the gelatinous form. The analysis gave:

S	As	Cu	Ni	Fe	$\dot{S}i$	$\ddot{B}i$	$\ddot{U}$	$\dot{F}e$	$\dot{F}e$	$\dot{H}$	Ag	
5·79	7·23	10·21	0·97	2·31	4·40	36·06	14·41	11·95	3·27	2·40	*tr.*	=99·00*

Hermann writes the formula $5(\dot{R}^4\dot{S}i+4\dot{R}\dot{S}i+10\dot{H})+\dot{R}(AsS)$. [It is quite improbable that this composition is that of a simple mineral, and until further investigation we may reasonably doubt the homogeneousness of the substance analyzed.—G. J. B.]

Vanadinite [p. 362, II—IV].—Kokscharow considers the *vanadinite* crystals from Beresowsk to be pseudomorphs of *pyromorphite*. Struve found in the interior of each vanadinite crystal a portion of unaltered pyromorphite. The mean of two analyses gave:

	PbCl	$\dot{P}b$	$\dot{F}e\ddot{C}r$	$\ddot{V}$	$\dot{P}$
G.=6·863.	9·60	71·13	0·43	15·92	2·92

Struve represents this composition by the formula $PbCl+\dot{P}b(\dot{P}\frac{1}{4}, \ddot{V}2\frac{1}{4})$ or $(3\dot{P}b^3\dot{P}+PbCl)+5(3\dot{P}b^3\ddot{V}+PbCl)$.—(Kokscharow, Mat. Min. Russlands, iii, 44).

Vivianite [p. 415, III, IV].—For an article on the composition and formation of *vivianite* by Alphonse Gages, see L., E. and D. Phil. Mag., [4], xviii, 182.

Water [p. 110].—Analysis of water from the Dead Sea, by Dr. F. A. Genth.—Ann. d. Chem. u. Pharm., cx, 240.

Wolfram [p. 351, I—IV].—F. A. Genth has published (this Jour., [2], xxviii, 253) an analysis of the wolfram which forms the nucleus of the peculiar tungstate of lime crystals alluded to under scheelite. One crystal showed the planes *I*, i-i, ½-i, and 1-i. Sp. gr. 7·496 (at 25° C.). Composition:

$\ddot{W}$	$\ddot{S}n$	$\dot{F}e$	$\dot{M}n$	$\dot{C}a$	
75·79	*tr.*	19·80	5·35	0·32	= 101·26

corresponding to variety II. (Min., p. 352), having the formula $4\dot{F}e\ddot{W}+\dot{M}n\ddot{W}$.

* The original gives 100, but owing to a typographical, or other error the analysis adds up only 99.

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WULFENITE [p. 349, II, V].—The massive wulfenite from Garmisch, is a mixture of molybdate of lead, with carbonate of lead and other substances, as shown by Wittstein's analysis (Kopp's Jahresbericht, 1858, 721):

Pb	Ca	Mg	Fe	Mo	Si	P	Loss (C & trace V)
33·60	10·85	3·57	2·80	20·00	16·20	0·04	12·74

ZINC-BLOOM [p. 460, 513, VII].—Dr. Elderhorst has described a hydrous carbonate of zinc from Marion County, Arkansas, as a new species under the name *marionite* (First Geol. Rep. Arkansas, p. 153). The chemical composition he found:

Zn	C	H	
73·26	15·01	11·81	= 100·08

[This is identical with analyses 1*a*, of zinc-bloom from Santander in Spain, by Peterson and Voit, published in the last supplement. This analysis gave Zn 73·1, C 15·1, H 11·8. These analysts found that zinc-bloom undergoes a change on exposure to the air, thereby losing both carbonic acid and water. A specimen of 1*a*, exposed to the air for three months was found to contain Zn 74·73, C 13·81, H 11·45. Other analyses by Braun are quoted in the last supplement. Peterson and Voit (Ann. d. Chem. u. Pharm., cviii, 50) give the formula for zinc-bloom $Zn_5C_2H_6$, which is the same as that given by Dr. Elderhorst for *marionite*;—it is an interesting fact that this is also the composition of the precipitate, produced by adding an equivalent of carbonate of soda to a zinc salt at the boiling temperature. Marionite may be considered as zinc-bloom, and the earlier analyses of this species by Smithson and Berthier, are undoubtedly less correct than those of Karsten and the more recent ones by Peterson, Voit, Braun, and Elderhorst.—G. J. B.]

Terreil mentions the occurrence of zinc-bloom at Santander in oolitic grains (L'Institut, No. 1347).

ART. XXXIV.—*Theoretical Determination of the Dimensions of Donati's Comet;* by Prof. W. A. NORTON.

IT is proposed in the present article to investigate the dimensions of the great comet of 1858, at certain specified dates, upon the theory developed in this Journal, (vol. xxvi; No 79), and compare the theoretical determinations with the results of observation. Resuming the equation of the approximate orbit of a particle emitted from the nucleus, obtained in the investigation alluded to, viz.

$$\frac{\sqrt{2pr}}{k\sin a}\left(1-\sqrt{1-\frac{k\sin a}{pr}z}\right)=\sqrt{\frac{2x}{k\cos a}}. \quad . \quad . \quad . \quad (1.)$$

in which the axis of z coincides with the original direction of motion, a denotes the angle of inclination of this initial line of direction to a line perpendicular to the radius-vector, r the radius of the nucleus, p the acceleration due to the repulsive force of the nucleus at its surface, and k the opposite acceleration produced by the sun's repulsion; let us pass to a new system of rectangular axes, x' and z', of which the axis of z' is coincident with the radius-vector of the orbit of the comet. Effecting the transformation of coördinates, reducing, and denot-

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ing by H the distance of the vertex of the cometary envelope from the nucleus $\left(=\frac{p\,r}{k}\right)$, we have

$$x'^2 - 2H\sin 2a\,.\,x' = -2H\sin 2a\cot a\,.\,z'. \quad . \quad . \quad . \quad . \quad . \quad (2.)$$

Putting $H\sin 2a = K$,

$$x'^2 - 2Kx' = -2K\cot a\,.\,z'. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3.)$$

Let $z'=0$, and we obtain for the half-breadth of the envelope, $\frac{b}{2}=2K$; and thence, for the coördinates of the vertex of the curve described by the particle, $X'=\frac{b}{4}=K$, and $Z'=h=\frac{K}{2}.\tang a$. Transferring the coördinates to this point, we get for the equation of the curve, referred to its vertex,

$$x''^2 = 2K\cot a\,.\,z''. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4).$$

This is the equation of a parabola, of which the parameter, $2p_1 = 2K\cot a = 4h\cot^2 a$; and the distance from the focus to the vertex $= \frac{K}{2}\cot a = h\cot^2 a$.

It is also the equation of the curve that would be described by a particle if it were projected from the nucleus with a certain velocity, and subsequently repelled by the sun alone. From which it appears that the path pursued by a particle repelled from the nucleus is very nearly the same, and, for the purposes of the present investigation, may be regarded as the same, as that which would be followed if the particle were simply projected from the nucleus. If we had occasion to trace accurately the trajectory of the particle in the vicinity of the nucleus, another investigation would become necessary. It should also be observed, that in the case of any particle, which, on its return from its excursion toward the sun, comes into proximity to the nucleus, the parabolic projectory becomes materially modified by its repulsive action, and equations (3 and (4) are inapplicable.

We may conclude from the result just obtained that, so far as the form and dimensions of the nebulous envelope are concerned, the theory of a repulsion exerted by the mass of the nucleus does not differ materially from that of the projection of the cometary matter by an instantaneous force from its surface; which, it appears, has been advocated and discussed by Bessel.

Other determinations relative to the envelope of the comet may be effected by the following formulas; in which Z = the greatest distance attained by a particle, in the initial direction of motion; Y = the actual distance from the nucleus, of the particle when in this extreme position; φ = the angle included between Z and Y; β = the inclination of the tangent drawn to

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any point of the curve followed by the particle, to the radius-vector of the orbit of the comet; $v =$ the velocity of the particle at the vertex of its parabolic path; and $v' =$ its velocity at any other point of the curve;

$$H=\frac{pr}{k}. \qquad Z=\frac{H}{\sin a}. \qquad Y=\frac{H}{\sin^2 a}. \quad . \quad . \quad . \quad . \quad (5.)$$

$$\varrho=\text{co.}\,a. \qquad \text{tang}\,\beta=\sqrt{\frac{H\sin 2a\cot a}{2z''}}=\sqrt{\frac{p_1}{2z''}}. \quad . \quad . \quad (6.)$$

$$v=\sqrt{kp_1}. \quad . \quad . \quad (7.) \qquad v'=\frac{v}{\sin\beta}. \quad . \quad . \quad . \quad . \quad (8.)$$

$$v'=\sqrt{k(p_1+2z'')}=\sqrt{k\left(p_1+\frac{x''^2}{p_1}\right)}. \quad . \quad . \quad (9.)$$

More accurately, we may obtain the velocity v'' at right angles to the radius-vector, for any point of the actual curve, from the following equation:

$$v''=\cos a\sqrt{2pr\left(1-\frac{r\cos a}{x'}\right)}. \quad . \quad . \quad . \quad . \quad (9^a.)$$

in which $x' =$ distance of the point from the radius-vector. The distance from the nucleus to any point of the trajectory of the particle, whose coördinates are known, may be readily obtained from the polar equation of the curve.

Equs. (1) to (9^a) have been obtained on the supposition that the nucleus is at rest; or, in other words, they refer to the relative motion of the cometary particle and nucleus, on the supposition that the two have the same velocity, and a constant direction of motion through space. Strictly speaking, there is not a perfect accordance between the two motions, even during the short interval of time that the particle remains within the limits of the envelope; but no material modifications of the theoretical results are required on this account, in investigating the form and dimensions of the envelope. But when we undertake to follow the cometary particle, after it has left the region of the envelope, and is receding from both the nucleus and the sun, under the influence of the solar repulsion, it will no longer answer to neglect the orbitual motion of the nucleus.

The general problem, to find the relative positions of a repelled cometary particle, and the nucleus of a comet, after any interval of time, appears to have been first effectually solved by Bessel. This important problem has recently been taken up independently, and solved anew by Prof. Peirce; who has shown that the orbit of the repelled particle is a hyperbola convex towards the sun, and has verified the supposed law of variation of the sun's force of repulsion. In pursuing the line of investiga-

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tion in hand, we are led to take a point of view somewhat different from that occupied by either of these eminent astronomers. It is now proposed to determine both the true and apparent positions of the receding particle, after the lapse of any interval of time, directly from the initial velocity and direction of motion; in order to take account of the various circumstances of the original motion of the different particles supposed to proceed from the nucleus. The following formulas will serve for this purpose. Equs. (12) to (15) have been deduced from the general equations of motion of a body around a centre of attraction, by changing the sign of the force, and adapting them to convenient computation. Equ. (17) for calculating the true anomaly of the particle in its hyperbolic orbit, from the time, was independently investigated. It is sufficiently accurate for our purpose, and the calculation can be more readily effected with it than by the intervention of the eccentric anomaly. The constants which enter into the equation can be determined by very simple formulas, for any comet the elements of whose orbit are known, and for any position of the comet in its orbit; their values having been determined by other means for the perihelion of any one comet. They depend upon the initial circumstances of motion of the particle emitted from the nucleus. Equ. (16) was deduced from equ. (17).

If any particle, on leaving the sphere of influence of the nucleus, is subject to a diminished attraction from the sun, it will describe a hyperbola concave toward the centre of attraction, and will recede from the nucleus, though less rapidly than if it were effectively repelled by the sun. Equs. (22) to (25) serve for this case. There will be occasion to make use of them when we shall undertake to determine all possible particles that at any assumed date may go to make up the concave outline of the tail.

New Haven, March 28th, 1860.

(*To be continued.*)

ART. XXXV.—*The Great Auroral Exhibition of Aug. 28th to Sept. 4th*, 1859.—4TH ARTICLE.

IN the three preceding numbers of this Journal we have given observations of the Aurora of Aug. 28th to Sept. 4th, from almost every part of North America between the parallels of 13° and 48° north latitude. We now present a summary of observations of the same aurora in Europe, with some reports from Asia, and accounts of a simultaneous auroral exhibition in the southern hemisphere.

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1. *Observations at Christiania, Norway,* (lat. 59° 54′), *by* Prof. CHRISTOPH HANSTEEN.

1859, Aug. 28th. At 10 P. M. only an indistinct coruscation behind the clouds in the north.

Aug. 29th, $12^h\ 10^m$ A. M. perfectly bright, almost as at full moon; the air dim with cirro-stratus, nevertheless the aurora shone through everywhere with strong radiating and flaming motion, very irregularly and unsteady. Corona was often formed; best formed at $12^h\ 17{\cdot}5^m$. Altitude 71° 37′ from south, azimuth 9° 57′ east. At $12^h\ 18{\cdot}5^m$ a purple-colored beam shot in east to γ Andromedæ. At $12^h\ 21{\cdot}5^m$ altitude of corona 72° 27′; azimuth 14° 55′ east. At no time were there regular bows. There was always a vacant space over the south horizon, but often of a suspicious character. It continued after 1 A. M. without essential variation in strength or character.

Aug. 29th, evening, rain—heavens covered.

Aug. 30th, " " " "

Aug. 31st, $11\frac{1}{2}$ P. M., lightning and thunder in southwest.

Sept. 1st, heavy rain—thunder.

Sept. 2d, radiating and strong flaming aurora, $12\frac{1}{2}^h$.

Sept. 3d, radiating aurora over the whole northern heavens to a little south of zenith; rather dimly. It continued to illuminate the heaven after it was almost covered. At $1\frac{3}{4}^h$ A. M. very clear behind the skies everywhere.

Sept. 4th, 10 P. M., radiating aurora in the north to 30° altitude. Later in the night, vehemently flaming with broad flames.

Sept. 5th, 10 P. M., elegant radiating aurora which dilated from the whole northern horizon to south of zenith, mostly behind a veil of cirro-stratus. At an altitude of 45° it was partly flaming. At 12^h it had nearly ceased.

Sept. 6th, at 10 P. M., an arc from 6° to 8° broad, the lower edge of which had an altitude of 5°.

The following table shows the state of the Bifilar magnetometer between Aug. 28th and Sept. 6th:—

1859.	Hour.	Bifilar.	1859.	Hour.	Bifilar.	1859.	Hour.	Bifilar.
	h m			h m			h m	
Aug. 28	9 23 A. M.	704·09	Aug. 30	9 15 A. M.	670·10	Sept. 2	4 27 P. M.	1436·91
" "	2 10 P. M.	764·98	" "	1 55 P. M.	751·18	" "	5 30 "	1104·69
" 29	9 28 A. M.	243 0	" "	5 30 "	732·08	" "	6 30 "	1020·41
" "	9 43 "	349·3	" 31	9 16 A. M.	667·72	" 3	9 21 A. M.	650·12
" "	10 9 "	638·78	" "	1 55 P. M.	737·95	" "	1 58 P. M.	926·13
" "	10 15 "	581·81	Sept. 1	9 21 A. M.	678·02	" 4	9 19 A. M.	670·05
" "	10 27 "	709·11	" "	2 5 P. M.	711·26	" "	4 28 P. M.	1084·2
" "	10 30 "	670·18	" 2	9 21 A. M.	609·70	" "	4 45 "	959
" "	10 33 "	723·43	" "	9 49 "	780·07	" 5	9 14 A. M.	705·15
" "	1 55 P. M.	941·50	" "	11 29 "	616·87	" "	9 15 P. M.	1060·49
" "	5 35 "	801·42	" "	2 25 "	1881·77	" 6	9 11 A. M.	641·85

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The greatest difference observed during this period was 1195·91 parts of the scale. One division of the scale corresponds to $\frac{1}{11\frac{1}{3}\frac{1}{7}\frac{1}{7}}$ of the horizontal intensity. Hence the variation of the horizontal intensity from Aug. 29 to Sept. 2 amounted to nearly $\frac{1}{13}$ of its whole value.

The inclination of the magnetic needle was observed as follows:

Aug. 29th,	10h	21m	A. M.	71°	31′·5	Sept. 2d, 10h 23m A. M. 71° 29′·0	
	5	23	P. M.	71	19·8	4 16 P. M. 70 26·9	
						6 26 P. M. 71 5·8	

The mean inclination of the needle in 1859 was 71° 18′.

The effect of this aurora upon the telegraph lines in Norway was much greater than in France and Germany. The effect was noticed from the opening of the stations at 7 A. M. On the 29th communication was interrupted till 1·1 A. M. on almost all the lines; and likewise Sept. 2d, but with a long repetition after 2 P. M. Sept. 3d, only towards 8½ A. M. During the remaining parts of those days, the perturbations were more or less uninterrupted, nevertheless communication could be maintained in some degree. Strong currents caused simultaneous attractions of all the armatures. The galvanometer showed strong deviations, sometimes with slow, sometimes with sudden movements, from one side to the opposite.

The intensity of the currents was greatest upon the longest lines going towards the north, on which sparks and uninterrupted discharges were from time to time observed. Pieces of paper were set on fire by the sparks of these discharges. In Bergen, where the line to Stavanger runs in a north and south direction, the current was at times so strong, especially Sept. 2d and 3d, that it was necessary to connect the lines with the earth in order to save the apparatus from destruction. The phenomena appeared less strong in Christiansand, in the southern part of Norway, where the lines run east and west.

2. *Observations made in different parts of England; extracted from the London Times.*

A. Durham (lat. 54° 46′).

Sept. 1, aurora; Sept. 2, vivid white aurora; Sept. 3, aurora. Sept. 4, faint aurora.

B. Preston (lat. 53° 45′), by R. C.

Sept. 2d, there was a brilliant auroral display, continuing from 11 to 12 o'clock, and a second appearance, though not so brilliant, at a little before 2 o'clock on the morning of Sept. 3d. During the first display, the whole of the northern hemisphere was as light as though the sun had set an hour before, and luminous waves rolled up in quick succession as far as the zenith, some of a brilliancy sufficient to cast a perceptible shadow on

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the ground. To the northwest there was a large patch of light of a deep crimson hue, while the waves of light were white, as also were the streamers which occasionally shot across the northern part of the sky. It was the most brilliant aurora that has been witnessed here for many years.

C. Nottingham (lat. 52° 57′), by E. J. Lowe.

On the evening of Aug. 28th and morning of Aug. 29th there was an unusually brilliant auroral display. From $8^h\ 40^m$ P. M. until 9 P. M. Aug. 28th, curtains of red light were visible near the zenith. By $11^h\ 40^m$ P. M. the glare of orange light in the north was powerful enough (even through much cloud) to make the hands of a watch visible. At $12^h\ 25^m$ A. M. the light was so strong that it gave the impression of daylight. At $12^h\ 45^m$ an opening in the clouds near the zenith disclosed the cupola which was situated exactly on Alpha Andromedæ. At $1^h\ 15^m$ A. M. magnificent rays of light met two degrees east of Alpha Andromedæ. At this time three-fourths of the sky was covered with aurora. At $2^h\ 30^m$, there being more clear sky, a splendid mass of aurora was visible, forming an ever changing cupola close to Gamma Trianguli. All the coruscations moved slowly eastward. At $3^h\ 15^m$ the cupola was formed near Gamma Andromedæ.

Sept. 3, strong aurora near the horizon.

Sept. 4, aurora.

D. Grantham (lat. 52° 55′).

Aurora Aug. 28th, 29th, 30th, 31st, and Sept. 3d.

E. London (lat. 51° 37′).

Aug. 28th, at $11^h\ 30^m$ P. M., auroral light in the north. At $0^h\ 15^m$ A. M. Aug. 29th it assumed the form of a luminous arch, similar to daybreak, and in the southwest there was an intense glare of red covering a very large extent; at $0^h\ 20^m$ streamers; at $0^h\ 25^m$ the streamers rose to the zenith and were tinged with crimson at their summits; at $0^h\ 45^m$ frequent coruscations; at $1^h\ 0^m$ the arch which had partially faded was re-formed, the body of light being very strong, but not sufficient to enable one to read any but very large print; at $1^h\ 30^m$ light equally strong, but outline indistinct; at 2 A. M. much less light and very indistinct. Continued till $2^h\ 30^m$ A. M.

Sept. 3, aurora.

F. Clifton (lat. 51° 27′), by William C. Burder.

Aug. 28th, about $10^h\ 45^m$ P. M., commenced a brilliant auroral display. At first there were several fine streamers, some of them white, and some faint crimson, extending from near the horizon almost vertically to α and β Ursæ Majoris. From that time till midnight there were generally very beautiful streamers, but without lateral motion, most of them being not quite vertical, but inclining slightly towards the east at the top. There was

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also always a general light, extending at midnight from northeast to west, and sometimes bright enough to enable a person to read the time on the face of an ordinary watch.

The aurora was repeated Sept. 1st, Sept. 2d, and Sept. 3d.

G. Aldershot (lat. 51° 15′).

Magnificent display of aurora Aug. 28th, and till early morning Aug. 29.

H. Brighton (lat. 50° 50′).

Aug. 29th, about half past one o'clock, a fine aurora occupied more than one-half the sky. It had the appearance of an irregular hemisphere of white light fringed with a band of crimson from twenty to thirty degrees broad, stretching from southwest to northeast by east.

3. *Observations at St. Valery, France*, (lat. 50° 10′ N., long. 1° 37′ E.), *by* H. LARTIGUE, *from Comptes Rendus*, T. XLIX, p. 367.

Near St. Valery a white light of considerable intensity was noticed in the north at $11^h\ 40^m$ P. M. Aug. 28th. A red column, with sides nearly parallel, and 4° or 5° in breadth, rose from the N.N.W. nearly to the zenith, but disappeared after a few minutes. About $12^h\ 10^m$ the white light near the horizon had increased in intensity; a large part of the heavens was colored red, and the exhibition attained its greatest brilliancy at $12^h\ 20^m$. Magnificent columns and brilliant rays, changing from red to green and white, rose to the zenith, sometimes passed beyond it, and occupied the entire space between Aquila and the meridian, and a few minutes later extended to the constellation Auriga. The light was bright enough to allow objects to be seen at a distance of one mile, as during a clear night with a full moon. The illumined portion of the sky increased till $12^h\ 40^m$. After this time the brightness diminished near the meridian, but the east and west portions continued red. At $1^h\ 15^m$ the vertical columns again appeared very brilliant, and nearly as extensive as at $12^h\ 40^m$, but they soon disappeared. The red light grew fainter, and disappeared entirely at 2^h. The white light which marked the commencement of the phenomenon continued three-quarters of an hour longer.

4. *Observations at Paris, France*, (lat. 48° 50′), *by* M. COULVIER GRAVIER, *from Comptes Rendus*, T. XLIX, p. 338.

The aurora was first noticed at Paris at 2^h on the morning of August 29th, and it soon rose to a great height above the horizon. About $2^h\ 45^m$ the vertex of the grand arch had reached the trapezium in Cetus, being 150° from the northern horizon, and it extended from Monoceros to 10° south of θ Aquilæ; having an amplitude of more than 200°. The vertex of the small arch rose to η Draconis, being a height of 26°; and it

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extended from Cerberus to Leo Minor, having an amplitude of more than 100°. The exhibition continued until the morning twilight. A motion of translation from W.SW to E.SE. was suspected, but the motion was not very appreciable. When the aurora appeared in its greatest brilliancy, the substance which composed it appeared to be in a state of great agitation; and the rays exhibited a red color, sometimes like that of iron heated to redness and sometimes to a white heat. The space occupied by the small arch was, as usual, of a greenish color; the centre near the horizon being black, and the whole destitute of rays. The aurora exhibited the greatest brilliancy between the W. and N.E. points of the horizon. A few cirrus clouds were noticed during the exhibition; they were all black, without any reflection of the light of the aurora, proving that this light emanated from a region much above that of the clouds.

Magnetic effects of the Aurora; from the Comptes Rendus, T. XLIX, p. 473.

On the 26th of August some anomalies were noticed in the motions of the magnetic instruments at the Observatory of Paris, the declination having changed 22′ between 9½ A. M. and noon.

Aug. 28th at 5 P. M. the motion of all the magnetic instruments was very irregular. Between midnight and 1 A. M. of Aug. 29th the horizontal intensity varied 0·0074. At 9 A. M. of the 29th the horizontal intensity had diminished by 0·01, while the vertical component had increased 0·0013.

During the forenoon of the 29th the declinometer was very much disturbed, and at 11 A. M. it oscillated 41′ on each side of its mean position. Towards evening the disturbances disappeared; but a fresh disturbance commenced on the 1st of September, at $11^h\ 30^m$ A. M. About 4 P. M. Sept. 2d, there commenced a new magnetic storm, more violent than that of Aug. 29th. The magnets were carried beyond the range of their scales, showing a change of the horizontal intensity exceeding 0·014, but as the observations were only recorded photographically, the extreme range could not be determined.

Effect on the Telegraph Wires, from the Comptes Rendus, T. XLIX, p. 365.

From the evening of Aug. 28th until the morning of the 29th the needles of the magnetic telegraph at Paris were almost constantly in motion, as if a permanent current was passing through the telegraph wires. Business was therefore entirely interrupted, and could not be resumed until 11 A. M. Aug. 29th. The same effect was noticed on the telegraph lines from 4^h to 8^h on the morning of Sept. 2d, although no aurora was noticed on that day. Business was again interrupted, the needles were disturbed, and the bells were rung.

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The galvanometers were violently deflected, sometimes to the right and sometimes to the left. The needles were turned from zero 10° or 20°, remained there stationary for a short time, then suddenly moved to 30° or 50°, then returned and were deflected in like manner on the other side of the zero point. The effect was more powerful and longer continued on the lines from Paris to Bordeaux, Marseilles and northward, than it was on the east and west lines. During the night of Aug. 29th some intelligible signals were received from Strasbourg.

During the day, Aug. 30th, the telegraph operators experienced frequent interruptions. On the afternoon of Sept. 1st some difficulty was experienced in telegraphing; but Sept. 2nd, at 4^h 50^m A. M., there was a general disturbance on all the lines, first on those to Bordeaux, Toulouse, Marseilles, London and Brussels, and a few minutes later on those to Basle, Strasbourg, Havre and Brest. At 7 A. M. *bright sparks* were noticed on the conductors of the lines to Bordeaux and Toulouse. The line to Strasbourg was less affected than the others. About 3 P. M. telegraphic communication was resumed on all the lines; but during the evening and the next morning it frequently happened that the communication was difficult.

Observations of Ozone.

Regular observations are made at Versailles on the amount of ozone in the atmosphere. During the auroras of Aug. 29 and Sept. 2, the quantity of ozone was decidedly greater than usual. The following table shows the sums of ozone collected during each period of six days, from Aug. 4, to Sept. 8, 1859:—

	Morning.	Evening.
From Aug. 4 to Aug. 10	64·0	55·6
Aug. 10 to Aug. 16	87·0	59·0
Aug. 16 to Aug. 22	82·0	60·0
Aug. 22 to Aug. 28	65·0	55·0
Aug. 28 to Sept. 2	97·0	64·0
Sept. 2 to Sept. 8	81·0	58·0

5. *Observations at Brussels, (lat.* 50° 51′), by M. QUETELET, from L'Institut of Feb. 1, 1860.

At 12^h 35^m A. M. Aug. 29th, the sky was overcast with a light and uniform veil, with the exception of the northern horizon, which presented a slight appearance of twilight. Soon there appeared in the N.W. a rosy light, which, in a few seconds, assumed enormous dimensions. It rose to an altitude of 60°, and illumined all that portion of the sky. The rosy light rapidly extended, and soon changed to purple, presenting the appearance of a vast conflagration. There was a constant oscillatory movement, and the light varied from a bright yellow to

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the deepest red. Near the horizon the sky presented a greyish and dirty appearance. There were faint traces of an obscure segment, whose centre was on the magnetic meridian. Bright rays of a yellowish white shot up from this part of the horizon, traversed the rosy light in the N. W., and terminated in a bundle at a distance of 90° from their origin.

About $12^h\ 45^m$ A. M., the twilight which illumined all the northern region became more intense; the general tint continued of a yellowish white, but on the eastern and western borders passed into a yellowish green. Then there appeared on the N. N. E. a second rosy light, but less decided than that of the N. W. This was also traversed by yellow rays; but the latter were much more brilliant and broader than those which traversed the light in the N. W. Those rays also terminated in a bundle at a distance of 43° from their origin.

Subsequently the aurora presented frequent alternations of brightness, but the general appearance continued the same until 2 o'clock, when observations were suspended.

At 9 A. M. Aug. 29th a disturbance of the magnetic instruments was noticed at the Observatory. The following table shows the extreme indications of the instruments for each hour, from 9 A. M. to 9 P. M. of Aug. 29th. Between 9^h and 10^h A. M. the fluctuations of the horizontal intensity were too great to be observed by the fixed telescope.

Hour.	Declination. Max.	Min.	Horizontal Inten. Max.	Min	Hour.	Declination. Max.	Min.	Horizontal Inten. Max.	Min.
h. h.	d.	d.	d.	d.	h. h.	d	d.	d.	d.
9 to 10	50·12	58·63	?	?	3 to 4	53·54	57·55	9·63	4·93
10 to 11	49·33	53·60	1.07	−2·84	4h 30m	57·96	"	8·83	4·60
11 to 12	51·77	55·89	1.89	−0·85	"	57·02	"	5·54	
12 to 1	51·68	53·58	6·50	2·53	6h 30m	56·60	"	3·78	
1 to 2	52·68	53·32	6·47	5·00	8h	55·43	"	6.12	
2 to 3	53·13	53·85	6·04	5·40	9h	55·73	"	5.85	

About midnight Aug. 28th—29th, the employés in the telegraph office at Brussels noticed signals from their bells, such as often occur during a storm. The employés in the offices at Mons, Antwerp, Gand and Ostend were also awakened by their bells, and enquired what was wanted. Communication with Paris, London, and Berlin were interrupted till $1^h\ 30^m$. Paris and London inquired of our operators if they saw a light in the heavens. The effect ceased at $1^h\ 30^m$ on all the lines except the submarine line from Ostend to Dover, which was charged with electricity throughout the entire morning. It was not till $3^h\ 30^m$, and after nearly doubling the battery, that communication was re-established.

September 2, between 5^h and 6^h A. M., there was a second disturbance on all the telegraph lines, and communication between

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Brussels, Paris, and London was interrupted. The following observations were made at the Observatory of Brussels:—

Date.	Declination Max.	Declination Min.	Hor. Inten. Max.	Hor. Inten. Min.
	d.		d.	
Sept 2, 9 to 10 A. M.	54·75	57·65	10·43	6.25
10 to 11 "	53·62	59·40	9·82	3·36
11 to 12 "	53·72	58·15	0·20	4·64
12 to 1 P. M.	52·06	66·24	8·05	7·36
1 to 2 "	49·34	62·81	?	?
2 to 3 "	43·40	57·87	17·79	0·00
3 to 4 "	48·32	58·40	15·53	10·12
4 to 5 "	51·15	55·00	14·01	10·48
5 0m	54·75		10·24	
6 0	54·44		8·87	
7 5	56·67		7·80	
8 24	56·59		7·21	

Date.	Declination. Max.	Declination. Min.	Hor. Inten. Max.	Hor. Inten. Min.
Sept. 2, 9h 0m P.M.	60·62		5.64	
10 0	59·47		8 04	
Sept. 3, 9 to 10 A. M.	57,17	58·23	4·89	4·09
10 to 11 "	54·56	56·53	5·21	4·46
11 to 12 "	53·73	54 96	7·08	5·55
12 to 1 P. M.	50·59	53·63	10·64	7·40
1 to 2 "	51·03	53·15	11·28	8·88
2 to 3 "	51·63	51·90	10·42	8·75
3 to 4 "	48·37	51·52	13·75	8·36
4 30m	51·52		12·50	
5 0	53·50		14·83	
9 0	57·58		7·31	

6. *Effects of the Aurora upon the Telegraph Lines of Wurtemburg; from Poggendorff's Annalen*, Band 108, p. 506.

During the night of Aug. 28th, from 11h 15m P. M. to near noon of the 29th, there was remarked from time to time on all the telegraph lines proceeding from Stuttgard, an extraordinary attraction of the armatures, which continued from 20 to 40 minutes, and generally appeared first on the line to Heilbronn, after about 5 minutes on the Ulm line, next on the line to Carlsruhe, and last on the Tubingen line. This attraction was repeated every 5 or 10 minutes, and, towards morning, every 2 or 3 minutes. After 5 o'clock only bell signals could be obtained from the local stations, as the armatures were held fast. During this period the deflections of the galvanometers were very remarkable. In a single minute the needles changed their position 5 or 6 times even to 40° west. While on the Ulm line the deviation was easterly, on the Bruchsal line the deviation was westerly.

The cause of this phenomenon is found in a brilliant aurora which was everywhere observed from 9 P. M. Aug. 28th till towards morning of the 29th.

7. *Effects of the Aurora upon the Telegraph Lines of Prussia; from Poggendorff's Annalen*, Band 108, p. 504.

The electrical currents on the conducting wires exhibited themselves in violent deflections of the galvanometers. The needles swung violently from 30° to 70° to one side, returned slowly to zero, and then moved slowly to the other side. On the line proceeding from Berlin westward, the disturbance commenced between 1 and 2 o'clock on the morning of Aug. 29th, when all connection with the stations ceased. Notice had previously been received of disturbance at the easterly stations, Konigsberg, Kowno, Riga and Petersburg. During the day of

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the 29th, on the western lines, communication was uninterrupted, while on the eastern lines it was occasionally suspended.

On the 2nd of September, when at 7 A. M. almost all the lines were in use, the disturbance occurred on all the lines, and interrupted communication from 5 to 40 minutes. The interruption was first experienced at Konigsberg 5 A. M.; at Stettin 5^h 55^m; Coblentz and Cologne 6^h 45^m; Berlin 6^h 50^m; Kowno and Riga at 7 A. M. About 9 A. M. the disturbance was greatest, and it declined till 9^h 45^m, when communication was resumed with most of the stations. At Stettin communication on all the lines was resumed at 9^h 24^m, and at Cologne at 10^h. At Konigsberg the disturbance still continued, and at Berlin it increased to 1 o'clock, so that all communication was suspended with the west. In the course of the day, news was received of disturbance at Hamburg, Breslau, Brussels, Paris, and Amsterdam. From the latter station came the intelligence that the submarine line to England was also interrupted by the aurora.

8. *Auroral Observations in Austria; communicated by* Prof. W. HAIDINGER, Vienna, to Prof. SILLIMAN.

The Aurora of Aug. 28th to Sept. 4th, was seen at the following places in the Austrian empire:

	Latitude.		Long.		
Bodenbach, Bohemia,	50°	47′	14°	10′	Aug. 29th, from 2 to 3 A. M.
Schoesl, do.	50	27	13	30	From 11¼ Aug. 28th to 2½ A. M. Aug. 29th.
Prague, do.	50	5	14	25	Aug. 29th, morning. Also faint in the night from Sept. 1st to 2nd.
Rzeszow, Galicia,	50	3	22	4	Sept. 2nd and 3rd, evening.
Biala, do.	49	50	19	5	Sept. 3, 8 o'clock.
Iristen, Austria,					Aug. 28, from midnight onwards.
Lintz, do.	48	17	14	15	September 3, 8 P. M.
Vienna, do.	48	13	16	23	Great magnetic disturbances were noticed from noon Aug. 28th to the evening of Aug. 29th. Also from Sept. 2 early in the morning to Sept. 3, morning; and from Sept. 4, morning, to Sept. 5th in the evening.
Kremsmünster, do.	48	3	14	7	Aurora, Sept. 3rd, from 8¼ to 8½ P. M.
Schemnitz, Hungary.	48	27	18	50	From Aug. 28th, 10 P. M. to Aug 29th, 3 A. M. Also Sept. 3rd, 9 P. M.
Neutra, do.	48	17	18	50	Sept. 3, 9 P. M.
Mitterdorf, Styria,					Sept. 2, evening.
Laibach, Carniola	46	3	14	30	Sept. 3, after 8 P. M.

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9. *Effects of the Aurora upon the Telegraph Lines of Switzerland; from the Comptes Rendus,* T. XLIX, p. 662.

The intensity and direction of the currents excited in the telegraph wires, during the aurora of September 2d, were determined by M. Hipp, at Berne, by the deviation of a magnetic needle, surrounded by a wire, making thirty coils. The regular current employed in telegraphing should have a sufficient force to deflect this needle 30°. M. Hipp found that the short lines gave no indication of a current, while the most marked effects were indicated by the longest lines, and especially by those which were directed from north to south, as the line from Zurich to Berne, Fribourg and Lausanne. The current on this line, directed from Zurich to Lausanne, would increase gradually, until the needle was deflected 42°. It would then slowly decline, and at the end of two or three minutes become zero. It would then change its direction, returning from Lausanne to Zurich, and attain a maximum of 30 degrees. The latter current, after continuing 60 or 90 seconds, became zero, and again changed its direction.

It appears from these observations that two currents succeeded each other on the telegraph wires, having a general direction from north to south, the one proceeding from north to south having a double intensity and a double duration, the other proceeding from south to north having a less intensity and a less duration.

M. Hipp obtained deviations of 58 degrees between Zurich and Berne, and of 64 degrees between Berne and Basle, indicating currents at least threefold the ordinary current employed in telegraphing.

10. *Effects of the Aurora of Aug. 28th and 29th upon the Telegraph Lines of Tuscany, by* M. CH. MATTEUCCI; *from the Annales de Chimie et de Phys.,* Tom. LVII, p. 419.

About 6 A. M., Aug. 29th, the disturbance became sensible on the telegraph lines. About 10 A. M. a current, which marked 25 degrees on the galvanometer, and equal to about 30 feeble elements of Daniell, traversed the upper wire of our telegraph lines, in the direction from Pisa to Florence. The current slowly increased, attained its maximum in about five minutes, and then rapidly declined. These periods were renewed a great number of times, and, during the intervals, telegraphic communication was held in the usual manner. About 3 P. M. the auroral effect upon our telegraph lines had ceased.

During the disturbance, on all the lines where there are several wires stretched one above the other in the same vertical plane, the strongest current was uniformly observed in the upper wire, while in the wire nearest the earth, the current was either feeble

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or inappreciable. This extraordinary current was the most intense on the longest wires.

11. *Observations at Rome, Italy, (lat.* 41° 54′), *by* M. Secchi; *from the Comptes Rendus*, T. xlix, p. 347 and 458.

On the 29th of August we had a superb aurora. The sky at Rome was covered with a red veil, and was crossed by the most brilliant rays, in the form of luminous columns. The magnetic instruments were very much disturbed. The declinometer deviated 34′ from its normal position, and the inclination varied 42′. The instruments for measuring the horizontal and vertical force both passed beyond the range of their scales, showing that the variation of the horizontal force must have been at least 0·0135, and of the vertical force at least 0·0075. The disturbance continued for a long time during the forenoon, and the vertical magnet, which, before noon, was beyond the scale, in consequence of the elevation of its north pole, at one o'clock passed beyond the scale on the opposite side, from a depression of its north pole.

A still more remarkable disturbance of the magnetic instruments occurred on the 1st and 2nd of September. At 4 P. M. Sept. 1st, the vertical magnetometer passed beyond its scale, showing a diminution of vertical force.

Sept. 2, at 7 A. M., the magnets were very much disturbed. At $7^h\ 10^m$ the declinometer pointed 2° 50′ to the west of its ordinary position. After this the needle returned rapidly to the east, and at $7^h\ 30^m$ pointed 1° 23′ east of its mean position, thus describing an arc of 4° 13′ in less than half an hour. This disturbance is the more remarkable, as the greatest range heretofore observed at Rome was only 45′ or 50′.

The bifilar indicated a diminution of the horizontal component, amounting to 0·129, or about *one-eighth* of its mean value.

These disturbances continued with variable intensity all day. At $4^h\ 15^m$ P. M. the vertical magnet again passed beyond the range of its scale. At 9 P. M. the magnet was more tranquil, and at midnight they had all returned nearly to their normal condition.

The variations of the declinometer, the bifilar and the vertical magnetometer were not simultaneous, but their maxima occurred at different times. The great vibrations were cotemporaneous with the currents observed on the telegraph lines. The clouds observed in the heavens had the exact appearance of those of the aurora borealis when it occurs by day, and such as were noticed at Rome Aug. 29th.

Similar observations were made at Leghorn, where at $6^h\ 30^m$ A. M. Sept. 2nd the declination was 15° 10′, while at $6^h\ 30^m$ P. M. it was only 14° 18′. The inclination of the magnetic needle was also very much increased during the day.

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12. *Observations from Western Asia.*

A. Yozgat (lat. 39° 45′), by Fayette Jewett, M.D., American Missionary.

The auroral phenomena referred to in your circular were *not* observed at Yozgat. On the 28th of August, and for several days before and after that date, I was in Arabkir, a town nearly 300 miles almost east of Yozgat. The aurora was not noticed there. While I was at Arabkir, owing in part to the mildness of the temperature, and also to the peculiar clearness of the atmosphere, my attention was almost every evening directed to the study of the constellations. The natives, too, at that season, slept upon the roofs of their houses.

B. Kharpoot, (lat. 38° 40′), by Rev. C. H. Wheeler, American Missionary.

Aug. 28th and the following nights nothing unusual was seen here by me or by others of whom I have made inquiries. It is also a fact, so far as I know, that the usual displays of the aurora are less brilliant here than in New England.

C. Mosul, (lat. 36° 22′), by H. B. Haskell, M. D. Missionary Physician.

No unusual appearance was observed Aug. 28th, 1859, either here, at Mardin, or Diarbekir. During the residence of American missionaries in Mosul (ten years) no auroral phenomena have been noticed.

13. *Observations in the Southern Hemisphere.*

Ship Southern Cross, (near lat. 50° S., long. 80 W.), from the Alta California.

On the night of Sept. 2d, during a tremendous gale, the rare spectacle of an aurora australis was witnessed. It commenced about half-past one o'clock in the morning, and increased in splendor until towards daylight, when it gradually faded before the light of day. The whole heavens were of a deep red, which color was reflected from the ocean. During the night a tremendous squall with hail burst upon the ship. Through the whole of this the flames assumed the same roseate hue; and when a spray flew over the ship, it fell to the leeward in ruddy showers. Between the squalls, in the clear places in the sky, the mysterious lights were seen shooting up in spiral streaks nearly to the zenith now flashing out with meteoric brilliancy, and now looming up against the horizon, as with the blaze of some terrible conflagration. During the gale, several times at night, brilliant balls of fire appeared flickering at the mast-heads, yard-arms, and other salient points. The captain and his officers say that they have never witnessed anything equaling this display for magnificence.

14. *Observations at Concepcion, Chili, (lat. 36° 46′), from the Mercurio of Valparaiso.*

An aurora was visible here on the nights of Sept. 1st and 2nd. It appeared at midnight in the south part of the horizon, and

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was visible until two o'clock in the morning. It had a movement of translation from east to west. In appearance it resembled a cloud of fire, or a large ignis fatuus, which threw out some flame or vapor, and spread a light like that of the moon. For more than an hour the city was brilliantly illuminated by this heavenly light.

15. *Observations at Santiago de Chili, (lat. 33° 28′), from the Mercurio of Valparaiso.*

On the morning of Sept. 2nd, about two o'clock, the sky to the south of Santiago was brilliantly illuminated by a light, composed of blue, red, and yellow colors, which remained visible for about three hours. This phenomenon is very rare in Chili. The aurora was also seen in Valparaiso (lat. 33° 6′).

15. *Observations at Kapunda, South Australia, (lat. about 35°), by* J. B. Austin; *from the London Times of Nov.* 14, 1859.

On Monday evening, Aug. 29th, just after dark, the aurora appeared like a large and brilliant pink cloud, extending about 25° or 30° above the horizon, and 60° or 70° in length. It continued visible for about twenty minutes, during the last five of which, splendid streamers of pink and white light were shooting vertically through it. It was seen almost throughout these colonies at the same time, and on four nights in the same week; but I saw it only twice, once Aug. 29th, and again on Friday, Sept. 2nd, when the most gorgeously brilliant display took place. It commenced immediately after sunset, and increased in splendor during the evening. For several hours, little was to be seen but a deep rich pink light over the southern part of the sky; but by degrees it extended, and, about nine o'clock, a huge pillar of fire appeared in the west, where it remained until midnight. After the moon went down, the brilliancy of the aurora increased, and from about half-past eleven till past twelve, a beautiful pale, soft, greenish-blue light, like the dawn of morning, extended itself above the southern horizon for about 100° or 110°, and about 18° or 20° in height. From this, streamers or radii of red, white and blue light shot upward to beyond the zenith, fully half the sky being covered with this splendid illumination, the light from which equalled that of the full moon in England. These radii converged towards a point about 15° north of the zenith, but did not themselves extend more than half that distance beyond the zenith. This was its last appearance, and a splendid finale it was. The powerful electric excitement in the atmosphere had an extraordinary effect on the telegraph wires, agitating the instruments violently in some places, and quite interfering with the transmission of messages.

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ART. XXXVI.—*Geographical Notices;* by DANIEL C. GILMAN, Yale College Library. No XII.

REPRINT OF A TRACT, BY NICOLAUS SILLACIUS, (A. D. 1494), ON THE SECOND VOYAGE OF COLUMBUS.—Although the principal object of these "Geographical Notices" is to record the recent progress of our knowledge of the world, yet we cannot forbear to make mention of a remarkable publication which pertains to the discovery of the New World at the close of the fifteenth century.

Christopher Columbus, in his second voyage across the Atlantic, set sail from Cadiz, September 25, 1493. Soon after his return, Guglielmo Coma wrote from Spain to Nicolaus Sillacius in Pavia an account of the journey. These letters were translated by Sillacius into Latin, and such other information was added to them as could be gathered from current reports; and this whole account of the voyage of Columbus, was published under the title, "*De Insulis Meridiani atque Indici Maris nuper inventis.*" This curious tract has been almost forgotten for nearly four hundred years; and, at the present time, but two copies of the original edition are known to be in existence,—one belonging to the Marquis Trivulzio of Milan, and the other to James Lenox, Esq., of New York. The last named gentleman, with characteristic liberality, has made this pamphlet accessible to all scholars, by causing it to be carefully re-printed in the original Latin, with an English translation by Rev. James Mulligan, a biographical introduction, notes, and a bibliographical appendix, in which much important information is given in respect to the early printed account of the various voyages of Columbus. The whole work forms a quarto volume of about 180 pages, printed in a truly elegant style.

It cannot be expected that this tract will add very much to what is known from other sources of the great navigator and his voyages, but as a contemporaneous record of most important discoveries, the volume will always be prized not less by the geographer than by the historian and bibliographer.

VOYAGE AROUND THE WORLD OF THE AUSTRIAN FRIGATE NOVARA.—The reference which has been made in a previous page of this number of the Journal to the Austrian circumnavigatory voyage, furnishes us with an appropriate occasion for speaking of that exploring expedition.

The Imperial frigate "Novara," under the command of Commodore von Wüllerstorf, set sail from Trieste April 30, 1857, and returned to the same port August 26, 1859, having successfully completed a voyage of scientific observation around the world,

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the first which was ever undertaken by the Austrian Navy. It is of course too early for the results of this expedition to be fully made public, but various accounts of the whole voyage, and of particular observations, have been given in the journal of the Geographical Society of Vienna, and in Peterman's *Mittheilungen*. *L'Institut* of Paris has also published a series of articles on the subject, communicated by M. Marschall of Vienna.

The "Novara" is a frigate of 1800 tons burthen, and 44 guns. It was manned by 354 men. The scientific corps, in addition to the commodore and other naval officers, consisted of the following naturalists, viz., Dr. Hochstetter, physicist and geologist; Frauenfeld and Zelebor, zoologists; Dr. Scherzer, ethnologist, having charge also of investigations in national economy, the botanist Jellinek and the artist Selleny.

Sailing, as we have stated, from Trieste, the expedition touched for longer or shorter periods at Gibraltar, (eleven days), Funchal, (nine days), Rio Janeiro, (three weeks), Table Bay, Cape of Good Hope, (twenty-four days), Island of St. Paul, (seventeen days), Point de Galle, (eight days), and Madras, (eleven days). Sailing from the last named port Feb. 10, 1858, from that time until August 11, a period of six months, the vessel was directed to various island and continental seaports of south-eastern Asia, including Nikobar, Singapore, Batavia, Hong Kong, Shanghai, etc. In September the island Puynipet was visited, and afterwards Sydney, (a month), Auckland, (seventeen days), Papeiti, on Tahiti, (eleven days), Valparaiso, (twenty-four days). Leaving the latter port May 11, 1859, the Novara reached Trieste at the end of the following August. The whole extent of the voyage was nearly forty thousand nautical miles.

Various letters and partial reports, submitted to the Academy of Sciences in Vienna, have already been printed, and a complete narrative of the voyage, and full reports of all the scientific observations which were made upon it, is soon to be prepared and printed.

Dr. Hayes's Proposed Arctic Journey.—A meeting of the American Geographical and Statistical Society was held in New York, March 22, for the purpose of encouraging Dr. Hayes in respect to his proposed voyage to the Northern ocean. Dr. Hayes was present, and, in addition to his statements, an eloquent exhibition of the importance of this expedition, together with an appeal for material aid, was made by Dr. Francis Lieber.

Hon. Geo. Folsom, E. H. Vielè, Esq. Profs. Mitchell and Silliman, and Dr. A. H. Stevens also took part in the meeting, and letters were read in approval of the undertaking from Profs. Bache, Henry, Guyot, Dr. Gould, etc.

The general purpose of Dr. Hayes has already been set forth

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in this journal, in a paper from his own pen, "On the Practicability of Reaching the North Pole," (vol. xxvi, p. 305–23, Nov., 1858.) At the recent meeting it was stated that ten thousand dollars had already been subscribed in aid of his enterprise, and at least ten thousand more are needed to insure the sending forth of the expedition.

The various weighty problems which are proposed for solution, especially the determination whether or not there be an open Polar Sea, present the strongest claims to the liberal contributions of all who are interested in the promotion of geographical discovery, or in the progress of physical science.

JOURNAL OF THE AMERICAN GEOGRAPHICAL AND STATISTICAL SOCIETY.—We are informed that this Journal, which has heretofore been published monthly, will hereafter be issued quarterly each number comprising at least 128 royal octavo pages. The first number, announced for the month of April, will comprise nearly 150 pages, consisting in part of original articles by the following writers: Commander Matthew F. Maury, Prof. Alexander D. Bache, Prof. Arnold Guyot, E. George Squier, Paul B. Du Chaillu, Dr. David Livingstone, Joseph C. G. Kennedy, James Wyne, M.D., together with late geographical and statistical intelligence, and careful notices of new scientific works bearing upon the objects embraced in the Society's labors. The subscription price to those not members of the Society will be three dollars a year.

Letters in reference to any matters connected with the Journal should be addressed to DANIEL WILLARD FISKE, General Secretary of the Society, New York.

EXPLORATIONS IN THE AMOOR REGION.—We have already called attention to the great efforts which are making by the Russian government to ascertain the resources and characteristics of Eastern Siberia, and to bring the immense region drained by the Amoor and its tributaries into connection with the commerce of the world. To our own countrymen, these investigations are especially important, when we consider the probable effect which will be produced upon the commerce of the Pacific.

At a recent meeting of the Royal Geographical Society of London, a paper was read, presenting extracts (prepared and translated under the direction of Capt. R. Collinson) from various official Russian reports, respecting the districts adjacent to the Amoor river, These extracts from the writings of Messrs. Pescurof, Vasilief, Radde, Usoltzof, Paragchefski, etc., are printed in the Transactions of the Royal Geographical Society, vol. 28,—together with an original map compiled by J. Arrowsmith. As our space at the present time allows us to quote but one of these reports, we have selected that by M. G. Radde, upon the table lands east and

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southeast of the great Lake Baikal—or, as he terms it, the Dáuro-Mongolian Frontier of the Trans-Baikal region.

"If by the word 'Steppe' be understood an extensive, treeless and arid plain, without any considerable undulations, that term cannot, in its full sense at least, be applied to the tracts now under consideration. Scientifically, and with regard to the formation of its surface, this region should be described as an elevated extent of country, intersected by many bare mountain ranges; the valleys and low plains between which are in some places strongly impregnated with salt, and produce exclusively *chenopodeæ* whilst in others they receive the waters of many small springs and atmospheric moisture in the shape of snow and rain, giving rise to innumerable small, turbid, and muddy lakes, seldom containing water fit for use, but more often contaminated with saline and alkaline solutions. An ordinary observer, one who has not penetratrated into the external structure of the earth's surface, or, what is of greater importance, into the properties of the soil from which he derives his sustenance, would see here only a contrast of conditions, namely, the contrast of the wooded surface to the treeless and bare, inducing him to call such a country a steppe. Whether the latter surface be level, or high and undulated, it would equally by him be termed a steppe; and only perhaps in distinguishing two contiguous regions would the mountainous and desert zone be designated as the 'high' steppe.

The Dáur country on the Mongolian frontier cannot, both with relation to its absolute height and its topographical features, be even approximately compared to a regular steppe; nor can any parallel be drawn between the chemical properties of their vegetable strata. Whilst in many regions, as for instance in the extensive Orenburg, Taurida, and Bessarabian steppes, the chernozem, so favorable to cultivation, penetrates the surface to 2 and 3 feet, there is a total absence of organic matters in the woodless valleys of the Dáurian frontier table-lands; and the soil of that extensive region has not undergone any considerable change for many centuries, owing to all the elevations, and frequently the valleys, abounding in siliceous ("jasper and flint") formations, which either do not admit of precipitation at all, or with great difficulty; added to which, the decomposition of hard rocky masses is materially retarded by the dryness of the atmosphere, and the want of snow and rain.

A further exposition will show that, leaving aside the peculiar stamp which characterises the organic nature of this region, the material and moral condition of its scanty population have succumbed to the influence of the physical conditions above-mentioned. The greater part of this desert track, perfectly unsuitable for the production of grain, is apparently, like the inhabited regions of the Gobi desert on the south, destined by na-

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ture for the nomadic life of the wild and superstitious Mongol, who, spurning the ties of a fixed abode, scours the level plain on his fleet steed.

With respect to geographical position, the Dáurian frontier-steppes occupy a narrow zone between longitude 112° 30′ and 119° E.; their chief extension is from west to east, and they are only in a few places intersected by the parallel of 50° N. latitude. If the treeless elevations are alone to be denominated steppes, the boundary of the Russo-Dáurian steppes must be drawn southwards from Nijni-Ulhun frontier station, as the mountains on the banks of the Onon, extending farther west, are covered all over with dense forests; on the east, on the other hand, from the above station, and between Akshinsk and Mogoitu, along the right bank of the Onon, extends a forest of tall trees, the predominating family of which, the pines suddenly disappears a few versts east of Mogoitu, and is succeeded by a straggling wood of birch, as far as Kubuhai.

The steppe district thus only crosses the Onon at Niji-Ulhun, occupying also a small zone, well irrigated with numerous small streams, on its left bank.

In its easterly extension, parallel to the course of the Onon, the steppe is not bounded on the north by this river, but by a very thick forest extending between the Onon and the desert, in some places 10 miles in breadth. This forest is worthy of notice for its historic associations as the sojourn of Chingis-Khan, and also in a botanico-geographical respect, forming, as it does, a natural boundary between the river and the steppe, which is remarkable for its small breadth and its clearly defined limit on the south. The forest thins gradually towards the east, down the Onon, and terminates entirely at the place where the river bends abruptly to the north on meeting the western spurs of the Adoncholon mountains; farther in that direction, with a lesser fall, and often contracted between banks of granite, the river pursues its course as far as its confluence with the Shilka, through a wooded country more frequently overgrown only with bushes.

The frontier steppe, which has already a breadth of about 53 miles between the old Chindan fortifications and the Uldza river, extends towards the south along the confines of this pine forest, acquiring a greater width farther on. The Onon-Borza* rivulet, flowing from the northeast, and which likewise approaches the southern offshoots of the Adoncholon mountains at 116°, and after bending to the north unites after a course of twenty miles with the Onon at Ust-Borzinsk, belongs at its western middle course to the steppe region. In like manner, the more sloping southern

* This stream is called Onon-Borza in distinction to the three Borza rivulets which fall into the Argun.

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declivities of the Adoncholon mountains, of which the summits alone are overgrown with stunted birch-trees (these are often, however, found in great density along the entire northern slope of that chain), are referable to the same region.* To the eastward, however, almost on the meridian of Tsagan-olu (116° 43′), two rows of woody elevations extend from east to north, intersecting each other at the most westerly lower range of the Buko-Hada, where the eastern branch terminates. The bare elevations running from this knot to the south expand the farther they extend, and form, near the frontier, the wooded table-land of Altangan, so called after one of its principal valleys.

The abovementioned mountains, which terminate in Buko-Hada, form first on their eastern, then on their northeastern extension, a water-shed between the affluents of the Onon and Argun; the Gazimur river takes its rise on its northern side. The Altangan table-land lies between two systems of saline waters; the lake of Tarei-nor is the largest representative of the western basins, whilst to the eastern belongs Ubudk, Tsagan-nor, Hara-nor, and many others. The culminating points of this region occur in the Steppe district, which here increases in width, being more than 67 miles in breadth between Tsagan-olu and Abagaitu. Having by barometrical measurement taken the height of Tsagan-olu at 2711 feet English, 500 more must be added for the mountain pass of Soktui. Only one valley, the largest and broadest of those occurring in the frontier steppes, on the Russian side, intersects the Altangan plateau from east to west, continuing on the other side of the mountains from their western slopes.† This is the valley of the Urulungui rivulet, which flows for 100–113 miles in a direction towards the Argun, and terminates there at Novo-Tsuruhaitui military station. At its lower course, the Urulungui flows gently along a winding channel, bordered at first occasionally by bushes of the willow, the precursors of a more luxuriant vegetation than that of the steppe. The region, however, between the Urulungui Argun, and Altangan plateau loses its vegetation more and more towards the south; on the frontier at Abagaitu it is intersected by parallel

* In Dáuria trees and bushes are only found on the northern slopes of mountains from two causes. The first is, that the southern slopes are much drier than the northern, which longer preserve the moisture of the soil, and so assist vegetation; whilst almost every plant withers in summer on the southern side of the mountain. The second cause is attributable to the circumstance that the fires which occur in the steppes in spring become sooner extinguished on the north side than on the opposite, where the snow leaves the ground earlier (being, in fact, perfectly dry by the end of February), thus offering no obstacle to the spreading of the fire. The limits of wood and bush vegetation are not governed in these regions by the rigor of the winter, but solely by the dryness of the soil and sultriness of the summer months.

† The Urulungui valley probably commences in the vicinity of the Chinese frontier, to the west of the Altangan plateau.

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40° 35′ and is so unproductive and barren, that on that account alone, and without reference to its topographical features, it may be considered the extreme northeastern end of the Gobi desert, which extends to the lakes of Buir-nor and Dalai.

Broad, light-green, and low tracts, overgrown with reeds, and winding only along the very edge of the Argun, intersect the bare and rocky desert, the uniformity and character of whose vegetation is at last broken by the Urulungui rivulet, at Novo-Tsuruhaitui. Lower down from the mouth of that stream the valley of the Argun assumes another aspect; and the river itself, taking a bend to the northeast, visibly contracts and flows more rapidly. Here the chernozen soil of the valley with its diversified flora also makes its appearance, so that the Urulungui may not only be considered as the limit of the high Dáurian steppes, but also the sharply-defined natural boundary of their vegetation.

To the north of the Urulungui commences the district of the metalliferous deposits of the Nertchinsk mountain region, remarkable also for its vegetation, which, lower down in the valley of the Argun, is very rich in forms, particularly at Chalbuchi village. It is here that the Mongolian oak, the Corylus heterophylla and Betula dahurica, seen nowhere in Siberia, first occur. Lastly, possessing a sufficiently thick population, some portions of this region are highly favorable to the production of cereals; but it is less adapted to the depasturage of cattle than the steppes, on account of the many mountains by which it is intersected.

To describe in a few words the boundaries of the high Dáurian steppes, it suffices to say that their limit on the north is formed by a pine forest, extending along the right bank of the Onon, by the Onon-Borza rivulet and the Adoncholon mountains, together with the elevations at the upper courses of the Gazimur and Urulungui rivulets; on the southeast by the Argun; and on the south by the Chinese frontier laid down in 1727; the western extremity of the steppe being bounded by the forests on the right bank of the Onon,

The whole of this country, occupying an area of 380 square miles, attains an absolute height of 2200 (English) feet at its greatest depressions (namely, at Kulussutaefski military station at Bayrn-Tarei lake), and almost 3000 feet at its highest elevations. Numerous mountain chains, rarely however detaching isolated spurs, intersect it in various directions, forming broad valleys, with a saline soil, and which are often found to contain accumulations of precipitated Glauber salt and soda, but seldom any water-basins. Even where the latter occur, they never attain any considerable depth, and are mostly so shallow and level that after a snowless winter or hot summer they completely dry up and frequently

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remain in that state for many years. The most striking example of this is afforded by the great Baryn-Tarei lake, lying south of the Kulussutaefski frontier station, which was found dry by Pallas in 1772: since then it filled with water, which again entirely evaporated five years ago, so that it now only presents a dry saliferous and muddy bottom, cracked in numerous wide fissures by the burning rays of the sun. With the exception of a few rills, generally filled only by snow water in spring, and remaining perfectly dry during the greater part of the year, a small number of spring morasses are alone to be found there. Not unfrequently such morasses occur in the vicinity of saline lakes; but often, having no efflux, they drain themselves, when, owing to the pressure of water beneath, the surface around their swampy edges rises several fathoms in winter with its icy covering. The ice remains in such places until the middle of summer; and even so late as the month of June have I seen on a freshwater morass near Kulussutaefski, in the neighborhood of Tarei lake, blocks of ice one inch thick, capped, as it were, with a layer of earth of the same thickness, overgrown with reeds.

With such a scarcity of water and so great an elevation, it is conceivable that the atmosphere of this region must be very dry. To the south of this frontier zone, at the same time, extends an immense desert, and on the north, the rain-clouds, being attracted by a dense forest, and arrested by elevated ranges, discharge their waters to superfluity over the wooded district of Nertchinsk; whilst some 7 to 14 miles to the south not a drop of rain or dew will fall for months together. At the village of Tsagan-olu, I witnessed, at the beginning and latter part of the month of June, examples of such an unequal distribution of moisture; whilst the heaviest rains and storms, continually interfering with my excursions, prevailed at midday in the forests only 5 miles to the north, buckwheat was being scorched 3 miles to the south of the village, and no rain had fallen since the middle of May at the frontier stations of Soktuisk, (40 miles farther to the south), and Kluichefski and Chindan (33 miles more westerly). It is to be regretted that scarcely any observations on the moisture of the atmosphere of this elevated region have hitherto been made, as, together with a better knowledge of the chemical properties of the soil, they might have led to some definite conclusion on the greater or lesser fitness of the country for agriculture. At the same time we find that almost useless experiments on the growing of corn have for many years been repeated with great perseverance at the military settlements on the frontier. In none of the extensive and remote regions of Russia, in the same latitude, are there, probably, presented so many local conditions unfavorable to agriculture as in the frontier steppes of Dáuria; and it is very doubtful whether, even with increased

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labor, and the introduction of a better system of tillage, any regular or even moderate harvests can be obtained. Not only is there on one side the want of rain and snow, and the great elevation to influence the early autumnal frosts, but on the other the very properties of the soil offer still greater obstacles to cultivation; to be surmounted perhaps only by a Chinese density of population, and Chinese industry.

The very soil of these regions is of a twofold nature: a great part of the steppes, and all the mountain-chains in particular, are as if sown with flint, jasper, and chalcedony, deeply buried in a hard argillaceous sand, and forming also the upper vegetable strata, which present no traces of fertility; whilst all the depressions of the surface are impregnated with salt, and therefore produce only a few saline plants. The climate is at the same time unfavorable to the growth of any plant. Severe snowless winters prevent the cultivation of winter wheat, while the early autumnal frosts are generally prejudicial to crops, and impede the fallow tillage. Spring wheat and buckwheat are consequently alone sown; and even these crops perish in great part from the droughts in May and June, no shade being afforded to their roots by their thin foliage and feeble growth of stem, which rises only one foot from the ground. As a rare exception, a snowy winter will sometimes follow a series of dry years; but this, although acting beneficially on the fields, is of great injury, by its long continuance, to the cattle, which are not unfrequently entirely destroyed by the want of fodder. Under ordinary climatic conditions, the want of snow is the chief impediment to their safely passing the winter; so that, on the freezing of the few fresh springs, the animals suffer much more from thirst than from hunger, and from the first half to the end of December are often so reduced that, even with a sufficiency of food, they are unable to survive the second half of that month.

Appreciating the advantages which Eastern Siberia derives from the opening the Amúr to commerce, Mr. Radde proceeds to consider the present agricultural wealth of the Dáurian Steppes, and its future influence and development. The first part of the paper has already shown the unproductiveness of the country, and the great obstacles presented by its climate. Cattle-breeding and sheep-farming in particular, would alone appear to admit of some development, as the lowlands and steppes afford good pasturage; and the prejudicial climatic conditions might, with perseverance, foresight, and industry, be rendered less unfavorable.

Wool is the only article which Mr. Radde adduces as an export, and he considers there will be no difficulty in finding a mar-

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ket for it in the United States. The frontier region of Dáuria and Mongolia is capable, the author thinks, of producing two millions of sheep; whilst cattle-rearing must for some years remain in its present state, owing to the scantiness of population, and the difficulty of making provision for the winter."

KHANIKOFF'S TRAVELS IN PERSIA.—Through the attentions of D. W. Fiske, Esq., General Secretary of the American Geographical Society, we have received the *Proces-Verbal* of the meetings of the Imperial Geographical Society of St. Petersburg, held Dec. 16, 1859, and Jan. 13, 1860.

At the latter sitting, M. Khanikoff presented an account of his researches in Persia, to which, in a former number of this Journal, a brief allusion was made. His remarks were chiefly directed to the Province of Khorassan, as will be seen from the following abstract which we translate from the *Proces-Verbal.*

The limits of this vast province, bounded on the north by a plateau which stretches in the direction of latitude from Hindou-Kousch to the southern extremity of the Caspian Sea, and toward the west by another plateau, making an angle of from 20 to 30 degrees with the meridian, are far less clearly defined toward the east. This traveller is of the opinion that Khorassan may be justly considered as bounded in this direction by the western slopes of Hindou-Kousch, which stretch from Hérat to Kandahar, as well as by the mountains which separate Séistan from Béloudchistan. The space thus enclosed presents four natural sub-divisions, to which M. Khanikoff gives the name terraces. The first embraces the salt desert lying between Kaschan, Koum, Bastam, Nichibour, and Tebbès. Its general inclination is directed from northeast to southwest, and its lowest point is on the line joining Bastam and Tebbès. The second comprehends the dry desert of Lut, and toward the north borders on the preceding; the mountains of Kirman are its southern limit; its general inclination is from north-northwest to south-southeast, and its lowest point is probably no more than 500 feet above the level of the sea. The lowest point of the third, which includes Séistan, is at the surface of Lake Hamoun, of which the waters are 1,545 feet above the level of the sea. Finally, the fourth terrace, which is the least extended, is bounded by the line which, on one side, joins Birdjand and Sebzar, and on the other stretches from the first of these villages to Toun, Haff, and Yezdoum; its general slope inclines from southwest to northeast. The limits of these four divisions of Khorassan are not everywhere well defined, but they are clearly indicated by the directions of the water courses and the inclinations of the ravines. The northern frontier of Khorassan coincides with the isothermal line of 12° Cent., a fact which gives plausibility to the conclusion, that over all the expanse of the northern plateau of cen-

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tral Asia, from Orenbourg to Meshed, over a space of 20 degrees in breadth, the annual temperature seldom falls to 6° Centigrade; at the southern limit of the first terraces described above, date trees grow and produce fruit in abundance, from which we must conclude that the annual temperature here is not below 18° Cent.; hence, in this direction and in moving toward the equator two geographic degrees only, the mean annual temperature acquires an increase equal to that gained to the north of Meshed by a progress of 20 degrees along the meridian. M. Khanikoff calls the attention of the Geographical Society to this point, that the rapid elevation of the degree of the annual temperature cannot be explained by the astronomical and hypsometrical coördinates alone, of the regions where this increase has been observed. He thinks that one of the essential causes to which it should be referred is the dryness of the air, which rapidly increases from the southern shore of the Caspian Sea to the frontier of Béloutchistan, so that in the desert of Lut the atmosphere contains only $\frac{1}{1}\frac{3}{0}$ths of relative humidity.

After entering into detail relative to the probable limits of the highest temperature, determined from the softening observed in the stearine which was in the baggage of the members of the expedition, M. Khanikoff has described some of the most striking atmospheric phenomena which he has himself studied in Khorassan. He mentions, among others, waterspouts and whirlwinds of dust, the dry mist, the atmospheric fluctuations, the mirage, and finally, observations upon the zodiacal light, which was seen by the expedition while traversing the space between Anarderré and Kirman. In conclusion, M. Khanikoff presented to the assembly the whole trigonometrical network, by aid of which the sketches which he has traced were drawn, adding the details of the circumstances which accompanied the operations. For want of time he reserved to another meeting the enumeration of the ethnographical labors of the expedition.

ART. XXXVII.—*Correspondence of Prof. Jerome Nicklès, of Nancy, France, dated Feb. 26th*, 1860.

French Academy of Sciences. Public meeting and distribution of the prizes.—This meeting was held Jan. 30; it was concluded with the eulogy upon Thénard, pronounced by Flourens, one of the Perpetual Secretaries. The following is a summary of the principal prizes awarded.

Prize for Astronomy.—This prize was awarded to Robert Luther, for the discovery of *Mnemosyne*, the only new planet of the year 1859. Mnemosyne is the 57th of the group of telescopic planets between Mars and Jupiter, and the 8th of those which are due to Mr. Luther. The

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Academy has already four times awarded the prize to this astronomer for the discovery of the five planets, Thetis, Proserpine, Bellona, Leucothea, and Fides.

The prize for Mechanics was awarded to Mr. Giffard for the invention of a new feeding apparatus for steam boilers, which he calls an "*automatic injector*." The report, made by Combes, gives great praise to this apparatus. The injector very advantageously replaces the feeding pumps of steam boilers. In addition to the fact that it avoids all loss of heat, other than that which results from the cooling of the exterior of the tubes in which the steam and hot water circulate, the absence of any movable solid parts, exposed to wear and derangement, the extreme facility with which the quantity of water supplied can be regulated between limits sufficiently narrow, &c., render it very valuable for locomotive machines. Accordingly several great railroad companies have already applied it to machines of this kind.

The automatic injector takes its origin from an observation made by Savart in 1832, in his experiments upon the fall of liquid veins; a current let fall from a vessel where the level is maintained at a given height, penetrates directly and quite unbroken into a vessel where the surface is less elevated.

The observations of Savart; the phenomena, long known, of the communication of lateral motion to fluids by which is explained the action of those blowing machines called *trômpes;* the forcible drawing in of air through the intervals which separate the bases of the tuyeres of high furnaces; the effects of the blast pipe of locomotive engines, &c., have given Mr. Giffard a hint which has led him to the invention for which the Academy have just awarded a prize.

Mr. Giffard, who is a person of very earnest spirit, is known in France by his attempts at guiding balloons. Some years since he obtained evident results in directing them, since he succeeded in making his balloon move *against* the *wind*, at the Hippodrome in Paris. This fact was stated in a report signed by intelligent men. Although without means to continue his researches upon this point, Giffard was not discouraged. Instead of making a show of his misery, and representing himself as persecuted by science, or as a martyr to an idea, he left balloons for the time and set about the construction of locomotive machines; he thought of his injector, and put off his researches upon ballooning until by his labors he should again be furnished with means to continue them; complete success crowned his investigations; he has now placed himself in a condition to take up again his favorite pursuits.

Physical sciences.—A prize has just been divided between Daubrée, Dean of the Faculty of Science at Strasbourg, and Delesse, mining engineer at Paris. The question proposed for competition was in respect to the *Metamorphism of Rocks.* The report of the committee will not convey to the readers any more knowledge than they have already obtained from this Journal which has often made mention of the labors of Messrs. Delesse and Daubrée.

The *Prize for experimental Physiology* has been awarded to Pasteur for his researches in regard to *fermentation.* They bear upon *alcoholic fermentation, lactic fermentation,* and *tartaric fermentation,* and of their

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isomeric compounds. The report of the committee has particular reference to the physiological side of the question; it was edited by Claude Bernard, an emimently distinguished physiologist. The following extract is from the report:

Following the example of Cagniard Latour, Pasteur considers the yeast of beer an organized body; he regards the modifications which it undergoes during alcoholic fermentation as of a nature essentially vital, and he shows that the chemical phenomena of fermentation are connected with a perpetual renewal of the yeast; whence it follows that, during the alcoholic fermentation, the sugar not only gives origin to chemical substances which disengage themselves or remain dissolved in the liquid, but at the same time, there is still a portion of the sugar which is taken up by the yeast in the form of cellulose, and another portion in the form of fatty matter, while the nitrogen of the old yeast serves to regenerate the new. Pasteur has made in this respect an experiment which, so to speak, reduces physiological conditions to the most simple relations which can connect living beings with mineral nature. He has shown, in fact, that the globules of yeast develop and multiply, and that the sugar ferments, when a quantity of the globules, so to speak, imponderable is sown in a medium composed at the same time of: 1st. A solution of pure candied sugar. 2d. An ammoniacal salt, the dextrotartrate, for example. 3d. Mineral substances containing phosphates. The ammonia is seen to disappear and to be transformed into the complex albuminous matter of the yeast, while at the same time, the phosphates give up their mineral constituents to new globules. The carbon which is one of the constituent elements of the yeast, is evidently furnished by the sugar. Before Pasteur, the lactic yeast was generally considered as organic matter in process of alteration, but not as organized matter. Our author has discovered and pointed out the special character of a lactic yeast, which is much more minute than the yeast of beer. During the lactic fermentation this yeast buds and multiplies, behaving in the matter of reproduction very much like the yeast of beer. In regard to the fermentation of tartaric acid and its congeners, Pasteur has arrived at very unexpected results, having a high chemical and physiological interest. Putting into conditions of fermentation with albuminous matter, and at a suitable degree of heat, the racemate of ammonia, which is formed by the union of the right- and left-handed tartrates of ammonia, and which has no effect on polarized light, it was seen that the phenomena of fermentation finally manifested themselves, and that new chemical products were formed at the expense of the racemate of ammonia. But it is remarkable that only the elements of the *right*-handed tartrate separate or ferment, to give rise to the products of fermentation, while under the same conditions the *left*-handed tartrate remains unaltered in solution in the liquid, which then acts very energetically upon polarized light. In this fermentation there is produced a yeast peculiar to the right-handed tartaric acid, which developes itself in presenting the characteristics of a mycodermic vegetable.

This example proves, in the plainest manner, the influence of the molecular dissimilarity of organic bodies, in the phenomena of fermentation. It is, indeed, impossible otherwise to interpret the marked differ-

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ence which, in this respect, the right and left tartaric acids exhibit, since both have exactly the same physical properties, the same chemical composition, and they differ only in the interior arrangement, which gives to their constituent parts a rotatory power equal, but in opposite directions, and which corresponds to the dissimilarity which is reproduced in their aptitude or inaptitude, to be influenced by ferments.

In short, Pasteur regards the chemical phenomena of fermentation as being always correlative to the vital phenomena of organization, and to the development which takes place, at the same time, in the organized yeasts which have the power to excite it.

The committee judged that the author, in thus pursuing the physiologic study of the yeasts, in the direction which he had chosen, would bring new light to bear upon a series of organic products, which are related to the phenomena of nutrition and histogeny.

Transplantation of the Periosteum.—Honorable mention was also made of Mr. Ollier, in reference to his interesting experiments on the transplantation of the periosteum, preserving its property of renewing the osseous tissue. The author showed, that if a strip of the periosteum is detached from the bone of a living animal, and is transplanted to the subcutaneous cellular tissue upon the same animal, or upon another individual of the same species, the fragment of periosteum becomes encrusted, and continues to live in such a manner that vessels are formed in its substance, and communicate with those in its vicinity, as can be proved by careful injection after death: Ollier has likewise proved that, several hours after death, the possibility of this transplantation of the periosteum still remains.

Prize relative to the unhealthy Arts.—This prize was awarded to the inventor of a lamp suitable for giving light to laborers at work beneath the surface of water. It is a lantern, consisting of a thick cylindrical covering of glass fixed between two iron plates. A reservoir, containing a mixture of alcohol and turpentine ("burning fluid") is placed in the interior. Where the apparatus is plunged into water, the air necessary to support combustion comes to the bottom of the lantern through two iron tubes opening up into the atmosphere. The products of combustion are likewise removed by means of a tube fitted to the centre of the upper plate, which is also prolonged so as to open into the atmosphere, and of which the section is double that of both the tubes, through which the external air is supplied.

The inventor of this apparatus was a simple workman, named Guigardot. With this lamp it is possible to work under water to the depth of twenty metres. It has been used with success at the works of the monumental bridge built over the Rhine, at Strasbourg. It illuminates a circle of $2^{m}\cdot 50$ radius, even in turbed water.

Medical Prize.—In the preceding years we gave much praise to the committee in charge of the medical prize. They sometimes expended even 90,000 francs in prizes of all kinds, awarded even to chemists whenever their labors were important to one of the branches of the healing art. This year the committee have been economical, one knows not why, inasmuch as important works are not wanting. They have awarded only honorable mention to physicians for the labors of their profession.

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Prize for Organic Chemistry.—The opposite state of things appears in the department of chemistry, which for the first time has awarded a prize to living chemists. A sum of 6000 francs was divided between Messrs. Wurtz and Cahours; to Prof. Wurtz for his researches upon glycol and its derivatives, and upon the new bases containing oxygen recently discovered; to Prof. Cahours for his labors in reference to the organic radicals. Be it understood, there is no question as to who is the discoverer of the composite radicals (Liebig), nor of the organo-metallic radicals (Lœwig).

Bréant Prize.—This prize is in reference to cholera and contagious diseases. It has not been awarded. The pieces sent for this *concours* were mostly mere letters, containing medical formulæ, all, according to their authors, infallible for the cure of the cholera, and all wanting, both in practical observations in regard to this dangerous disease, and in rational deductions as to the nature of its attack, and the symptoms which accompany and constitute it. The following are the principal questions proposed for the *concours* of 1861 and 1862:—

1st question. "Discuss carefully and compare with theory, the observations upon the tides, made in the principal ports of France."

2nd quest. "To complete in some important point the geometrical theory of polyhedrons."

3rd quest. "Establish the general equations of the movement of the earth's atmosphere, taking into consideration the rotation of the earth, the calorific action of the sun, and the attractive forces of the sun and moon."

4th quest. "Study of any question, at the option of the candidates, relative to optical phenomena."

5th quest. "At different points of the thermo-electric scale and for a difference of temperature reduced to 1° C. to determine the directions and compare the relative intensities of the electric currents produced by different thermo-electric substances."

6th quest. "Determine by experiment the causes which influence the difference of position of the optical and photogenic foci."

7th quest. "Comparative anatomy of the nervous system of fishes."

8th quest. "Study of the hybrid vegetables, with respect to their fecundity, and the perpetuity or non-perpetuity of their character."

9th quest. "Study of the mode of formation and of the structure of spores and of the other organs which contribute to the reproduction of fungi, their physiological office, the germination of the spores, and particularly with reference to parasitic fungi, their mode of penetration and developement in other living organized bodies."

Each of these prizes consists of a medal of the value of 3000 fr.

10th quest. "Essay upon carefully made experiments, to throw new light upon the question of so-called spontaneous generation."

11th quest. "Experimental study of the modifications which can be effected in the developement of the embryo of a vertebrate animal by the action of external agents."

12th quest. "Study of the distribution of the vessels of the latex in the different organs of plants, with particular regard to their relation to or connection with the lymphatic or spiral vessels, as well as with the fibres of the liber."

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13th quest. "Determine experimentally what influence insects may exercise upon the production of the diseases of plants."

Each of these prizes consists of a medal of the value of 2500 fr.

Besides these prizes there are others: especially the Bréant prize (100,000 fr.) is to be awarded "to the person who shall have discovered the means of curing the Asiatic cholera, or shall have discovered the causes of this terrible scourge."

Now that we are upon the subject of prizes we will also say that a prize of 6000 fr. is offered by the society of Pharmacy at Paris, for "the question of the artificial production of quinine, or in default of this, of a substitute possessing equivalent anti-febrile properties." The prize for artificial quinine has been open since 1849.

Time strictly limited to July, 1861.

These concours are open to all scientists without distinction of country.

Obituary. Death of Poinsot.—This illustrious geometer died on the 10th of December last, at the age of 83 years. Born in 1777, he carried on his studies at the Polytechnic School from which he went out in 1796 in the character of an engineer of bridges and highways. He was successively, Professor at a Lyceum in Paris, and member of the higher council of public instruction. He had been a member of the Institute since 1813 where he took the place of the mathematician Lagrange. We shall speak no further of the titles, the dignities, and the decorations of Poinsot, since all these objects of human pride are but a vapor, and have added nothing to his merits, whom mechanicians place, in the history of mechanics, immediately after Archimedes, Galileo, Huygens and Newton. His principal claim upon the memory of posterity is founded upon "*La Statique*," the "*Memoire sur l'équilibre et le mouvement des systèmes*, 1806," and the *Théorie nouvelle de la rotation* and the *Mémoire sur les cônes circulaires roulants;* the last memoirs were in 1853.

In the *Elémens de Statique*, he brought to light his beautiful theory of couples and its application to the conditions of the equilibrium of machines.

Poinsot had a truly philosophic mind; he knew how to render the most abstract matter accessible and to bring it down to the most elementary ideas; this power was one of the characteristic traits of his genius. It is equally recognized in his work upon the precession of the equinoxes, that remarkable phenomenon discovered by Hipparchus, and explained two thousand years afterwards by d'Alembert.

Poinsot retained his intellectual activity to the close of his life. He never experienced the trials which are often met by men of science in their pathway. While yet young he saw the most illustrious judges proclaim his rare talents, and his life has always been happy and honored; Poinsot was not only a great savant, but moreover a good man.

Discovery of an Intra-Mercurial Planet.—The year 1860 has been inaugurated in France by an astronomical discovery, all the more remarkable because made in unusual circumstances. It is the discovery of another planet between the earth and the sun, verifying the conclusion to which Leverrier had arrived by the power of computation, that there existed one or more planets within the orbit of Mercury. This conclusion was announced to the Academy at the session of September 12,

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1859, in a letter from Leverrier to Mr. Faye. Very soon reports were spread that the discovery of such a planet was no new thing. Among these reports, the most persistent was this, that such a planet had been observed towards the commencement of the year 1859, by an amateur astronomer, a poor country doctor. Leverrier, resolving to trace this story to its source, went personally to Orgères, a village in the Department of Eure and Loir, where this doctor resided. Great was the emotion of Dr. Lescarbault on seeing before him the illustrious director of the Observatory of Paris. He had little difficulty in satisfying Leverrier that the observation had really been made. He said, "On the 26th of March last, (1859), about four o'clock, in pursuance of my regular habit, I was exploring the sun's disc with my telescope, when suddenly I noticed near the border a small, well defined, round black spot, having a sensible motion. It advanced farther and farther on the sun's disc. Unfortunately just then occurred a call from a patient. I went down below, gave my advice to the patient, reascended to my observatory, the black spot continued its journey, and I saw it reach the opposite border of the sun and disappear, after being about an hour and a half on the disc."

The measurements made by Dr. L., with very simple means of his own, enabled Leverrier to determine that the chord of the sun's disc traversed by the planet was $9' 17''$; and it would have required $4^h\ 26^m\ 48^s$ to traverse the diameter of the sun. He estimates the angular diameter of the planet at about a quarter that of the planet Mercury when in transit. If this estimate is correct, the new planet is insufficient to account for the anomaly in the motion of the perihelion of Mercury; and there probably is, as M. Leverrier announced, a group of small planets between Mercury and the sun. Observers should give special attention to this subject at the next total solar eclipse.

The new planet has not been officially named, but it is already designated as *Vulcan*. Its return is looked for at Paris at the end of March or the beginning of April.

This discovery was not a matter of mere chance. Dr. Lescarbault, having observed the transit of Mercury, on the 8th of May, 1845, conceived the idea that if there were between the earth and the sun any planetary body besides Venus and Mercury, it must also sometimes cross the sun; and that by frequently observing its disc, such body might be detected thereon as a black point.*

Without fortune and without means of observation, Dr. Lescarbault was unable to obtain a telescope until 1853; and it was not until 1858 that he commenced systematic work. He made most of his auxiliary apparatus for himself, and went to work in astronomy very much as Scheele did in chemistry.

* In 1836 and 1837, M. Pastorff of Buchholz observed, several times, a pair of small, round black spots, of unequal size, passing, in a few hours, across the sun's disc, and each time in a different path. These must have been a planet with a satellite. Messrs. E. C. Herrick and Francis Bradley of New Haven, in 1847, endeavored to re-discover those bodies, by observing the sun's disc, twice a day, with a large telescope, and by exploring the vicinity of the sun with the telescope, armed in front with a pasteboard tube, blackened within. These efforts proved unsuccessful. (See this Journal, Nov. 1859, vol. xxviii, pp. 445–6.) The hypothesis of an intra-mercurial planet was also proposed, many years since, by M. Buys-Ballot, in his researches on a maximum and minimum of the solar heat.

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M. Leverrier further informs us that the explanations which M. Lescarbault gave him entirely satisfied him that the alleged observations were real, and are entitled to a place in science; that the long delay in making them public is due solely to the modest reserve of the observer, and the quietude of a residence remote from the excitement of great cities. In support of his assertions, M. Leverrier has exhibited the proofs which he found in the domicil of Dr. Lescarbault, viz. a paper on which the latter had marked, on the 26th of March, 1859, the appearance of the black point on the solar disc, and a pine board on which Dr. L. had chalked his computations and his drawings.

Hardly had these announcements been made, when several other similar observations were called to mind. Mr. Scott, Chamberlain of London, writes in Galignani's Messenger of January 14, 1860, that an Englishman, Mr. Lloft, observed, January 6, 1818, a black point traversing the sun's disc, and that he (Mr. S.) had made a like observation in the summer of 1847, had thence concluded the existence of a third inferior planet, and had so published. His son, five years old, standing by, was placed at the telescope, and confirmed the observation by crying out, "I see a little balloon on the sun."

M. Wolff of Zurich has published several ancient observations, which he thinks refers to the transit across the sun of an intra-mercurial planet, viz. by (1,) Scheutzer of Crefeld, June 6, 1761; (2,) Staudacher, towards the end of February, 1762; (3,) Lichtenberg, November 19, 1762; (4,) Hoffmann, at the beginning of May. 1764; (5) Dangos, January 18, 1798; (6,)Fritsch, October 10, 1801; (7,) Stark, October 9, 1819. All these observers saw a round well-defined point, of the apparent diameter of Mercury, crossing the solar disc in a short period, varying from two to three hours. Using only the observations 5, 6 and 7, Mr. Wolff finds that they accord with the supposition of a planet having a revolution around the sun in 19·25 days, a result surprisingly near to that (19·7 days) deduced by M. Leverrier, from the observation of Dr. Lescarbault.

In settling the question, who is entitled to the credit of the discovery, it is clear that Dr. Lescarbault's observations are the first which have the scientific seal, and are of such a nature as to be subject of computation.

New Members Elected.—

Fizeau was elected 2nd of January member of the section of Physics in place of *Cagniard de Latour*—deceased.

Plana was elected March 5, foreign associate in place of *Lejeune-Dirichlet* deceased.

At the session of the *Académie* held March 19, Mr. J. A. Serret was elected a member in the section of Geometry in place of *Poinsot*, deceased.

Dausey has lately (March 12) communicated to the Academy a memoir, setting forth the geographical and physical observations of Hommaire de Hell made in 1846–48 in Turkey and Persia, where he died at Ispahan in August, 1848. His manuscripts were taken to France by Jules Laurent the well-known artist who accompanied the expedition. Dausey reports the barometric and astronomical determinations of much value.

Hypnotism and Magnetism.—French society has been almost carried away with a kind of scientific epidemic which may be compared to the

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malady called "table tipping," which was so much in vogue a few years since. This epidemic is Hypnotism or nervous sleep. It was brought into notice by a French surgeon following a work of Dr. Braid (published 15 years since), in which this physician, describes under the name of Hypnotism, the nervous affection which is produced under the following circumstances:—When a brilliant object is placed directly before the face at a distance of from 8 to 15 inches, and the subject of the experiment is requested to fix his eyes steadily upon the object, in such a way as to produce a permanent contraction of the muscles of the eye and eyelid, there will be seen to come on, after the lapse of a few minutes, a state analogous to catalepsy. M. Broca, having successfully employed hypnotism in producing insensibility to pain, proposes it to surgeons as an anæsthetic agent capable, in many cases of being substituted in practice for ether or chloroform, "because," as he remarks to physicians, "this method, introducing as it does no substance into the system, appears to me absolutely harmless."

From the numerous experiments which have since been made, it results; 1st, that this kind of anæsthesia can be developed but rarely, and in persons whose nervous system is especially predisposed; 2nd, that hypnotism may give rise to attacks of epilepsy.

This kind of anæsthesia is therefore not less open to objection than the other. Its origin is more ancient than is supposed; it was established more than two centuries ago, under the name of *irradiation* or *phenomena of actinobolism*, by P. Kircher in his *Ars magna Lucis et Umbræ*.

It is concluded that in hypnotism is seen only a fact in the domain of animal magnetism, which is not yet a science. We will not enter further into the details.

Porous bodies.—Among the topics of scientific interest which awaken attention at present, is the research of Jamin, professor at the École Polytechnique, upon the equilibrium and movement of fluids in porous bodies. The new results at which he has arrived afford an explanation of the ascent of the sap in vegetables without the necessity of recourse to the vital force. It is apparently a question of capillarity only.

Jamin has applied the new facts which he has discovered to the construction of an apparatus composed entirely of inorganic materials, but showing in its structure a great analogy with vegetables. This apparatus has the property of raising water as trees do, to a height greater than that attained by means of atmospheric pressure, from a moist soil whence the water is constantly drawn to the factitious leaves where it is continually evaporated.

Reduced to its most simple form this apparatus is composed of a block of some well dried porous substance as chalk, lithographic stone, &c., or a porous battery cell filled with a powder well rammed in, white chalk for instance, oxyd of zinc, or even with earth. A manometer is imbedded in the interior of the mass, and the whole is plunged in a vessel full of water. The water immediately penetrates its pores and drives out the air, which collecting in the interior, exercises a pressure upon the manometer amounting with oxyd of zinc to *five* atmospheres and with starch it exceeds *six* atmospheres. This is not the limit of the greatest possible pressure; Jamin makes known the causes which diminish it in these cases and proves that the water is forced into porous bodies with a force which he calls π, and which is equal to that of a considerable number of atmospheres.

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A tube 1·20 metres long filled with plaster and terminated at the summit by an evaporating surface is inserted by its base into a reservoir closed and filled with water; a vacuum is caused measured by 15 or 20 millimetres of mercury or by 200 or 270 millimetres of water; and the water appears even at the upper extremity of the tube, which proves *that porous bodies are able to raise water higher than can be done by atmospheric pressure.* These facts cannot be explained by the ordinary laws of capillary attraction, since these bodies are not formed of impermeable tubes, but of corpuscles in juxtaposition, separated by small empty spaces. Jamin has therefore submitted the problem to the calculus and has come to results, of which we mention the following:

If in a damp porous body, the water is compressed by a power of several atmospheres, it can congeal only at a temperature below 0° C.* Consequently the old wood is able to resist frost, while the young shoots being less dense are unable to do so.

Since water in filtering through a porous body is compressed as it enters and dilates again as it runs out, it should exhibit electric currents and many other phenomena.

The theory can not be applied to non-homogeneous porous bodies. In the extended memoir which he has prepared, Jamin discusses the complicated results which may be occasioned by irregularity of structure; he makes an application of it to wood, and shows that the interior pressure must be augmented in the denser tissues; that the air must come from the larger tubes, which cannot serve for the ascent of the sap.

It is plain that the evident tendency of all these experiments is to explain the ascent of the sap in vegetables by capillarity. The idea is not new, but it has not been hitherto fully admitted, notwithstanding the experiments which have been heretofore made.

Jamin gives it probability in showing by decisive experiments, that porous bodies exercise a capillary action superior to the pressure of the atmosphere; further, he gives the physical theory of capillarity in porous bodies and succeeds in calculating the phenomena of the movement of liquids in trees. This is thoroughly physiological. If the Academy of Sciences could award the great prize for physiology, for a work upon fermentation, when it is not yet known whether this purely chemical phenomena is the result of vital action as Cagniard de Latour maintains, or the manifestation of a mechanical effect as Liebig explains it, in his beautiful theory of fermentations,—if this work in chemistry deserved the great prize for experimental physiology for a stronger reason should they award this prize for the splendid physical researches of which we have just spoken.

Application of electric light in Medicine.—The ordinary processes of illumination are of very difficult application when the object is to employ artificial light in diagnoses or in certain cases in operative medicine, inasmuch as the illumination is insufficient, or the light is more or less colored, and is accompanied with heat. It is not the ordinary electric light here spoken of, but that produced by induced currents. The problem to be solved consists in finding a source of light with little or no heating effect, which can be compressed into tubes of small capacity and of forms adapted

* This fact has just been demonstrated by Mr. Sorby for water contained in capillary tubes of a small diameter.—J. H.

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to circumstances, and which finally is of such whiteness as not visibly to alter the color of organic tissues illuminated by it. This problem has just been solved by Dr. Fonssagrive, physician of the naval school at Brest, with the coöperation of Messrs. Ruhmkorff and DuMoncel. The apparatus consists of an empty tube of Geissler, of very small diameter folded and turned upon itself after the manner of multipliers; this tube contains a gaseous mixture which gives a perfectly white light when it is traversed by an electric current produced by an induction coil.

Phosphorescence.—The electric lamp (*photophore*) of which we were just speaking calls to mind some observations upon phosphorescence which have just been made by Phipson; he has found that like cane sugar, the *sugar of milk* or *lactine* becomes luminous by concussion, and also by fracture. To evolve phosphorescence from the *nitrate of uranium*, it is sufficient to shake briskly a bottle containing a certain quantity of this salt in the crystalline state. The light is very vivid when the experiment is made with one or two kilograms of this substance. Calomel possesses the same property, although in a less degree.

Works of Arago.—The sixteenth and last volume of Arago's works has just appeared. It contains a great number of unpublished researches made by Arago during his career.

The sixteen triangles are here given which Messrs. Biot and Arago determined in the prolongation of the meridian of France to the island of Formentera. Arago alone, has measured besides, a seventeenth triangle having its summit at the enclosure of Galaro in the island of Majorca and resting in one direction upon Camprey in the island of Trizza, and the other upon the mole of Formentera, with the design of obtaining the length of an arc of the parallel nearly 3° from the extremity of the meridian, and to determine the curvature of that portion of the earth's surface. The results of these measurements, heretofore unpublished, are found in this volume.

Arago in the year 1853, the very year of his death, communicated to the Academy a memoir upon the figure and physical constitution of Mars, which is likewise contained in this volume, and is accompanied by more than 3000 micrometrical measurements of the diameters of Mars, Jupiter, Saturn and Uranus, which were taken from 1811–1847.

It is well known that Arago made numerous investigations in regard to the refractive power of atmospheric air, dry or humid, and of different gases and vapors. He labored in this department for nearly half a century, namely, in 1805 with Biot, in 1815 and 1816 with Petit, and in 1852 with Fizeau. Chemists and physicists will derive much advantage from the determination of the refractive powers not only of some simple gases, but also of compound gases such as oxyd of carbon, carburetted hydrogen, sulphuretted hydrogen, cyanogen, the vapors of sulphur and of carbon, of sulphuric ether and chlorohydric ether.

This volume also contains studies upon optics, atmospheric electricity, &c. United to the other fifteen volumes it forms a lofty monument to science as well as to one of its most noble representatives.

***Bibliography.*—The following works have just appeared at Paris.**

***Oeuvres d'Arago*, Tom. xvi, 1859; chez Gide, 5 rue Bonaparte.—This volume is devoted to scientific notices bearing upon the personal labors of Arago, many of which have never before been published.**

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At the *Librarie centrale des sciences rue de Seine; Recherches sur le non-homogénéité de l'étincelle d'induction*, par M. Th. du Moncel, 1860.—M. du Moncel, who gives all his leisure to the study of electricity and its applications, presents in this brochure the result of his researches upon the electric spark, and especially the spark of induction, of which the non homogeneity was discovered by him in 1855.

At Baillière Bros., Paris & New York. *Traité élementaire de Physique expérimentale*, Tom. 1. 12°, 1860. par M. Forthomme, professor of Physics at the Lyceum in Nancy.—This work, even by the confession of the author, contains nothing new, but is distinguished by its method. The most difficult questions in regard to gravity, hydrostatics and heat are explained in the first volume with great clearness, and thus rendered intelligible to persons little versed in these matters, which are so important in our day and have so many useful applications.

By *Lacroix & Baudry*, Quai Malaquais.—*Grands hommes et grandes choses, notices scientifiques sur les inventions et sur les decouvertes modernes et sur les auteurs, par Victor Meunier.* 8°, 1860—This work appears by numbers, once a week; its author, of whom we have often spoken, has acquired in France a great reputation as a popularizer. He established the *Ami des Sciences*, a journal for scientific discussions, which he has directed for six years, and is remarkable for the independence of his opinions and judgments. M. V. Meunier in this new publication proposes all the great scientific questions of the day.

Art. XXXVIII.—*Description of an Equatorial recently erected at Hopefield Observatory, Haddenham, Bucks;* by the Rev. W. R. Dawes.

(*From the Monthly Notices of the Royal Astronomical Society.*)

My observatory was furnished, in May last, with an equatorially-mounted telescope by Messrs. Alvan Clark and Sons, of Boston, U. S., which in several important points differs from any other in this country; and I therefore hope that a brief description of it may not prove uninteresting to the Royal Astronomical Society.

The form combines great firmness and compactness with considerable elegance of design. The massive part of its structure is of cast-iron, the base of which is firmly bolted down to a stone pier. The semicircular form of the upper part affords a secure position for most of the wheel-work of the driving-clock, of which the going-weight descends in a groove on the east side of the pier, and is not seen in the drawing. The space between the polar axis and the semicircular bed-piece is occupied at its lower part by the hour-circle. Immediately above this is a sector, which clamps on to the axis, and the wheel-work of the clock occupies the upper portion. The sector has a radius of rather more than 9 inches and an arc of 30°, or two hours of right ascension. This arc has a face of an inch and a half in breadth, between which and a cylinder 7 inches in circumference there is just room enough for two thin bands of sheet-brass, each of about three-fourths of an inch in width, to pass side by side. These bands are both keyed by the end into one groove in the cylinder, at such a distance that they cannot overlap or interfere with each other. They are then bent round the cylinder in

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opposite directions, the end of one being fastened to one extremity of the arc of the sector, and the end of the other at the other extremity of the arc to a piece of brass which is acted upon by a screw and nut, for giving to both the bands a due degree of tension. The sector and cylinder thus move together without friction, irregularity, or lost time.

Upon the same arbor with the cylinder is the wheel, 15 inches in circumference, in the racked edge of which the driving-screw works. This arrangement gives the screw about the same driving-power as if it acted on the edge of a wheel nearly 40 inches in diameter, fixed on the polar axis.

I have every reason to be satisfied with the going of the driving-clock; and the cylindrical bob of the pendulum being screwed on to its steel rod, the rate is capable of adjustment to the greatest nicety. Great care has been bestowed by the makers upon the accurate dividing of the wheel-work; and I have much pleasure in acknowledging that its performance fully bears out my expectations, founded on the character given by the Messrs. Bond of the clock-work applied by the same makers to the great Munich equatorial in Harvard Observatory, which has been so successfully employed for the purposes of telescopic photography. While the speed of the clock is regulated by the vibrations of the half-seconds pendulum, the action of the pendulum on the wheel-work is rendered smooth and equable by an ingenious application of Bond's *Spring-governor;* and so perfectly successful is this contrivance, that with the thread of the micrometer bisecting a star, and a power of 800 or 1000 on the telescope, no interruption or jerk from the escapement is perceivable.

For producing a slow motion in right ascension, the driving-screw is mounted on a brass frame, which, being carried by a fine screw under the observer's control, acts as a slipping-piece through nearly five minutes of time.

A firm clamp, close to the cradle of the telescope, fixes the declination-axis, and is accessible to the observer both at the eye-end, and also during the setting of the declination-circle. A slow-motion screw acts on an arm extending from the clamp to the bottom of the cradle to which the screw is attached.

To permit the adjustment of the polar-axis to the latitude and meridian of the place, the upper part of the cast-iron bed-piece is made with a groove which receives loosely a projecting keel on the portion bolted down to the pier. The form both of the groove and of the keel being semicircular, the upper portion is moved upon the lower by the stout screw which is seen in the drawing, and the polar-axis is thus easily raised to the required angle. The adjustment to the meridian is performed by the screws on each side of the groove in the upper piece pressing

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against the keel in the lower, which has play enough in the groove to allow of a moderate degree of azimuthal motion.

To facilitate the finding of objects in Mr. Clark's "Two-eye-piece Micrometer," when their distance exceeds the field of one of the eye-lenses, the finder is furnished at its eye-end with a small position-circle divided into degrees. The thick wires of the finder being placed in the direction of the objects to be measured, the reading of the position-circle indicates the approximate setting for the micrometer, whereby the two objects may be immediately found by their respective eye-lenses. The aperture of the finder being two inches, it will show a star of the $9\frac{3}{4}$ magnitude of Struve's scale.

The object-glass of the telescope has a clear aperture of $8\frac{1}{4}$ inches, and a focal length of about 110 inches. The materials were furnished by Chance and Co., of Birmingham. The figure is excellent to the circumference, and the dispersion but little over-corrected. Its performance fully supports the character of Mr. Alvan Clark's object-glasses, and I believe it to be capable of everything which can be performed by such an aperture. It clearly divides γ^2 *Andromedæ*, and shows the smallest companions among the stars of the Pulkova Catalogue.

Haddenham, near Thame, November, 1859.

SCIENTIFIC INTELLIGENCE.

I. CHEMISTRY AND PHYSICS.

1. *On Fraunhofer's Lines.*—KIRCHHOFF has communicated a preliminary notice of very remarkable investigations on the spectra of colored flames. These investigations have given an unexpected clue to the origin of Fraunhofer's lines, and justify some remarkable conclusions as to the constitution of the atmosphere of the sun and perhaps also of the more brilliant fixed stars.

Fraunhofer remarked that in the spectrum of the flame of a candle there are two bright lines which correspond with the two dark lines D of the solar spectrum. The same bright lines are obtained more easily and stronger from a flame into which common salt has been introduced. The author produced a solar spectrum and allowed the sun's rays before they fell upon the slit to pass through a powerful salt flame. When the sunlight was sufficiently weakened two bright lines appeared instead of the two dark lines D; when however the intensity of the sunlight exceeded a certain limit, the two dark lines D were seen with much greater distinctness than without the presence of the salt flame.

The spectrum of Drummond's light usually contains the two bright sodium lines, when the luminous portion of the lime cylinder has not been exposed to ignition for a long time; if the lime cylinder remains fixed, the lines become weaker and finally vanish. Under these circumstances, a

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salted alcohol flame placed between the lime cylinder and the slit, produces two dark lines of extraordinary sharpness and fineness, which exactly correspond to the lines D of the solar spectrum. In this manner these lines are artificially produced in a spectrum in which they do not actually occur.

If we introduce chlorid of lithium into the flame of Bunsen's gas lamp, the spectrum exhibits a very bright, sharply defined line, which lies in the middle between Fraunhofer's lines B and C. If we allow solar rays of moderate intensity to pass through the flame to the slit, we see the line bright upon a dark ground; but with a greater intensity of the sunlight a dark line occupies its place, which has precisely the character of Fraunhofer's lines. When the flame is removed, the line vanishes completely.

The author concludes from these observations that colored flames in whose spectra bright sharp lines occur, diminish the intensity of rays of the color of these lines, when these pass through them, to such a degree, that in place of the bright lines, dark ones occur, whenever a source of light of sufficient intensity is placed behind the flame. He further concludes that the dark lines of the solar spectrum which are not produced by the earth's atmosphere result from the presence in the ignited sun's atmosphere of those substances which produce in the spectrum of a flame bright lines in the same place.

We may assume that the bright lines corresponding with D in the spectrum of a flame always arise from the presence of sodium; the dark lines D in the solar spectrum allow us therefore to conclude that sodium is present in the sun's atmosphere. Brewster has found in the spectrum of the flame of saltpetre bright lines in the position of Fraunhofer's lines A, *a*, B; these lines indicate the presence of potassium in the sun's atmosphere. From the author's observation that no dark line in the solar spectrum corresponds to the red lithium line, it would follow that lithium is either not present at all in the sun's atmosphere or is present only in very small quantity.

The investigation of the spectra of colored flames has in this way obtained a new and high degree of interest. The author promises to pursue the subject in connection with Bunsen, and states that they have already obtained results which render it possible to determine the qualitative constitution of complicated mixtures from the appearance of the spectrum of their blowpipe flames. In pursuing together the investigation of Kirchhoff's discovery of the influence of flames upon rays of light, a remarkable fact has appeared which promises to be of great importance. Drummond's light requires a salt-flame of low temperature in order that the lines D may appear dark. The flame of alcohol and water is adapted to this purpose, but the flame of Bunsen's gas lamp is not. In this last case, the smallest perceptible quantity of salt causes the bright lines to appear. The authors reserve the developement of the consequences of this remarkable fact.—*Pogg. Ann.*, cix, p. 148, *January*, 1860.

Note.—Professor Stokes, in a letter to the editors of the L. and E. Phil. Mag. directs attention to the fact that Foucault, in 1849, published a short paper in l'Institut containing observations exactly analogous to those of Kirchhoff. Foucault's experiments were made by transmitting solar light through the galvanic arc, and appear to have escaped attention until their second and independent discovery by Kirchhoff.

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2. *On the direct conversion of lactic into propionic acid.*—The conversion of lactic into propionic acid was first observed by Ulrich who obtained chloropropionic acid by distilling lactic acid with perchlorid of phosphorus. Lautemann has succeeded in a more direct transformation of lactic acid by the agency of iodid of hydrogen. When concentrated lactic acid is diluted with half its volume of water and the cooled liquid is saturated with iodohydric acid gas, iodine is set free and if the liquid is heated in a closed tube to 140° C. iodine is separated in quantity. The filtered liquid neutralized with potash and then distilled with dilute sulphuric acid yields propionic acid mixed with some iodine and iodohydric acid which are easily separated by carbonate of silver. The reaction which yields propionic acid is expressed by the equation

$$\underset{\text{Lactic acid.}}{C_6H_5(HO_2)O_4} + 2HI = \underset{\text{Propionic acid.}}{C_6H_6O_4} + 2HO + I.$$

The same result may be still more easily obtained by distilling lactic acid with biniodid of phosphorus when propionic acid passes over colored by free iodine. Lautemann considers these results as confirming Kolbe's view that lactic acid is propionic acid in which one equivalent of hydrogen is replaced by one of the compound radical HO_2.—*Ann. der Chem. und Pharm.*, cxiii, 217.

3. *On the Formation of Alanin from Lactic Acid.*—Strecker's discovery that alanin may be converted into lactic acid by the action of nitrous acid is familiar to chemists. Kolbe has succeeded in effecting the reverse process, that is, in obtaining alanin from lactic acid. Lactate of lime is to be converted into chlorid of propionyl by distillation with perchlorid of phosphorus and this by treatment with absolute alcohol into chloropropionic ether. The ether is then to be treated with a concentrated solution of ammonia in a closed tube for several hours. The solution on evaporation gives a mixture of sal-ammoniac and alanin which are easily separated. The reaction in this case is expressed by the equation—

$$C_6H_4ClO_3, C_4H_5O + 2NH_3 + 2HO = C_6H_4(NH_2)O_3, HO + NH_4Cl + C_4H_5O, HO.$$

Ann. der Chem. u. Pharm., cxiii, 220.

4. *On the Constitution of Lactic Acid*—Kolbe has communicated a very interesting discussion of this subject, maintaining his view that lactic acid is monobasic and is to be regarded as oxypropionic acid. We must however refer to the original paper which does not admit of condensation.—*Ann. der Chem. und Pharm.*, cxiii, 223.

5 *Contributions to the Chemistry of the Platinum-metals.*—Claus has continued his investigations of this most difficult subject and has obtained many new and interesting results, the most important of which we shall here state.

In the present paper, the author confines his attention chiefly to ruthenium and its analogies with osmium. The hydrated deutoxyd of ruthenium has the formula $RuO_2 + 5HO$ and may be obtained either by digesting the corresponding chlorid with carbonate of potash and washing the oxyd with boiling water, or by precipitating the sulphate of the oxyd with potash or soda. The sulphate in question may be prepared by oxydizing the sulphid with nitric acid. The oxyd when freshly precipi-

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tated is a slimy ochre-colored substance which dries to a rust colored mass resembling hydrated sesquioxyd of iron. It dissolves in acids with a bright yellow color, and gives on heating with chlorhydric acid, a beautiful red solution of the chlorid. When heated to 300 C. the oxyd loses a portion of its water, and when more strongly heated, explodes in consequence of the instantaneous separation of the remaining water.

The author describes in full the double salts $RuCl_2$, KCl and $RuCl_3$, NH_4Cl and gives their reactions with the usual reagents. Ruthenium forms, like osmium, an acid containing four equivalents of oxygen, as well as the ruthenic acid RuO_3 already described by Claus. To this acid Claus gives the name of hyper-ruthenic acid; it may be prepared by passing a current of chlorine into an alkaline solution of ruthenic acid, prepared by fusing metallic ruthenium with caustic potash and saltpetre.

The hyper-ruthenic acid, being volatile, distils over, and may be collected in a receiver. The new acid is a golden yellow crystalline and volatile substance. On gentle heating, it melts into golden yellow drops, which again solidify as a crystalline mass. The acid is very volatile and evaporates at ordinary temperatures; its gas has a golden yellow color, and the acid has a peculiar smell which resembles that of nitrous acid. The gas irritates the lungs and produces a cough, but does not attack the eyes like osmic acid. It has little taste, but is somewhat astringent, though not acid. Its boiling point lies not far above 100° C.

The acid is very easily reduced. In the moist state its solution is decomposed after a few hours, with formation of the sesquioxyd. Alcohol and the greater number of organic bodies easily reduce it. Potash dissolves the acid slowly, but the solution passes after a time into one of ruthenate of potash. The author gives the relations of this body to other reagents in detail, but for these we must refer to the original memoir.

When a solution of $RuCl_3$, NH_4Cl is evaporated to dryness with an excess of ammonia, the chlorid of a new base is formed, the formula of which, according to Claus, is $2NH_3 . RuCl + 3HO$. The salt has an Isabel yellow color, and crystallizes in rhombic tables, which are soluble in water and yield on heating pure metallic ruthenium. When this chlorid is digested with an excess of freshly prepared oxyd of silver, it yields a solution of the oxyd. On evaporation, this solution gives a rather porous, yellow, crystalline mass; this base appears to be even more caustic than caustic potash itself. The author promises a more complete description of this interesting base, as well as additional contributions to the chemical history of iridium and rhodium.—*Journal für prakt. Chemie.* vol. lxxix, 28, *January* 31*st.*

6. *Synthesis of new Bases containing Oxygen.*—Wurtz has observed the remarkable and important fact that oxyd of ethylene $C_4H_4O_2$ combines directly with ammonia, forming very well defined and powerful bases. When concentrated aqueous ammonia is mixed with oxyd of ethylene and the mixture is allowed to stand, combination ensues with a strong evolution of heat. On evaporation an alkaline syrup is obtained which gives with chlorohydric acid a solution from which colorless rhombohedra separate. These have the formula $C_{12}H_{16}NO_6 + HCl$: the platinum salt is $C_{12}H_{16}NO_6, HCl + PtCl_2$. The mother liquor of these rhombohedra contains another chlorid, the platinum salt of which has the formula—

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$C_8H_{11}NO_4$, HCl+PtCl$_2$. The formation of these bases is indicated by the equations—

$$3C_4H_4O_2+NH_3=(C_4H_4O_2)_3H_3N=C_{12}H_{15}NO_6$$
$$2C_4H_4O_2+NH_3=(C_4H_4O_2)_2H_3N=C_8H_{11}NO_4.$$

These results may perhaps lead to the discovery of the true constitution of the complex organic alkaloids containing oxygen. They appear to show that Berzelius' view that the alkaloids are conjugates of ammonia may be true in some cases at least.—*Comptes Rendus*, xlix, 898.

7. *On a new series of Alcohols.*—Wurtz has also found that oxyd of ethylene unites with water to form new alcohols which he terms diethylene and triethylene alcohols. The reactions involved are represented simply by the equations—

$$C_4H_4O_2+2HO=C_4H_6O_4. \quad \text{Glycol.}$$
$$C_4H_4O_2+2HO=C_8H_{10}O_6. \quad \text{Diethylene-alcohol.}$$
$$3C_4H_4O_2+2HO=C_{12}H_{14}O_8. \quad \text{Triethylene-alcohol.}$$

Oxyd of ethylene also unites directly with glycol so as to form the diethylene and triethylene alcohols. The equations are

$$2C_4H_4O_2+C_4H_6O_4=C_{12}H_{14}O_8.$$
$$C_4H_4O_2+C_4H_6O_4=C_8H_{10}O_6.$$

All these substances behave like alcohols. The diethylene alcohol was also discovered by Lourenço and called by him the intermediate ether of glycol.—*Comptes Rendus*, xlix, 813.

8. *Researches on the Platinum metals;* from a letter of Dr. Wolcott Gibbs to one of the editors.—"The completion of my researches on the platinum metals has been delayed much longer than I expected. This has arisen partly from the intrinsic difficulty of the subject and partly from its expansion in particular directions in a very unexpected manner. A brief sketch of the results already obtained—imperfect as they are—may perhaps interest you.

The osmium-base of which Dr. Genth and myself published a brief notice about two years since in the Journal, proves to be the type of a very extensive series of compounds which promise to be of much theoretic interest. You will remember that the chlorid of that base is produced when osmite of potash, KO, OsO_3, is added to a solution of sal-ammoniac. I now find that new complex bases are formed when the osmite is added to solutions of the chlorids of narcotin-ammonium, cinchonin-ammonium, &c., &c.; in short, almost all the complex alkaloids which I have yet tried give analogous bases containing osmium in the radical. The new bases are very easily decomposed with evolution of osmic acid. They are more stable in the presence of an excess of chlorhydric acid and give crystalline double salts with the chlorids of gold and platinum.

These however are not the only or even the most remarkable basic compounds which I have discovered. Many of the ammonia-metal bases already described are capable of forming new bases into which osmium enters either as a conjugate body or as replacing hydrogen. When, for instance, osmite of potash is added to a solution of the chlorid of pallad-diamin, $2NH_3$. PdCl, a yellowish brown solution is formed

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which on addition of chlorhydric acid gives a beautiful yellow crystalline precipitate insoluble in cold water and containing palladium, osmium and the elements of ammonia.

When osmite of potash is added to a solution of chlorid or sulphate of luteocobalt, $6NH_3 . Co_2Cl_3$ or $6NH_3 . Co_2O_3 . 3SO_3$, a buff-yellow precipitate is thrown down, which on addition of HCl gives a wine-yellow solution. This solution after a short time deposits beautiful crystals of the chlorid of a new base containing osmium, cobalt, and the elements of ammonia. The chlorid gives well crystallized salts with the chlorids of platinum, gold and mercury. Its solution is decomposed by gentle heating, osmic acid being evolved while a black powder is thrown down.

The other ammonia-cobalt bases give analogous compounds which however are decomposed almost as soon as formed. It is my intention if possible to examine the relations of osmite of potash to one or two of the arsenic and antimony bases, as for example to the chlorids having the formulas $(C_4H_5)_4AsCl$ and $(C_4H_5)_4SbCl$.

When ammonia is added to a solution of osmite of potash, the red color of the latter passes immediately to wine-yellow. Fremy supposes that an osmiamid is formed here having the formula $OsO_2 . H_2N$. I find that the product is a new osmium base, the chlorid of which is formed at once by neutralizing the yellow solution with HCl. This chlorid has probably the formula $\left.\begin{matrix} H_3 \\ Os \end{matrix}\right\} NCl$, though it may be $\left.\begin{matrix} H_3 \\ OsO_2 \end{matrix}\right\} NCl$. In like manner I am still doubtful whether the true formula of the other ammonia-osmium base is $2NH_3 . OsO_2 . O$ or $2NH_3 . OsO + 2HO$. Experiments now making will decide this point. Meantime I may say that I do not agree with Claus in considering the formula $2NH_3 . OsO_2 . O$ improbable since we have many analogous cases, as for instance in xanthocobalt and flavocobalt, the formulas of the chlorids of which are, you will remember,

$$NO_2 . 5NH_3 . Co_2 O . Cl_2 \text{ and } 2NO_2 . 5NH_3 . Co_2 O . Cl_2.$$

The chlorid $2NH_3 . OsO_2 . Cl$ or $2NH_3 . OsCl + 2HO$ is decomposed by boiling with chlorhydric acid, giving sal-ammoniac and a new chlorid which is perhaps identical with that just mentioned. This is easily explained by the equation

$$2NH_3 . OsCl + HCl = NH_3 . OsCl + NH_4Cl,$$

and the decomposition would then be exactly analogous to that of pallad-diamin under the same circumstances

$$2NH_3 . PdCl + HCl = NH_3 . PdCl + NH_4 Cl.$$

I hope soon to decide these questions by experiment and analysis, but the analyses are very difficult and tedious, and I have to feel my way and find out new methods for almost every determination.

You will see from the above that osmium is likely to become one of the most interesting of the elements, and that it is capable of forming an extraordinary number and variety of compounds. I am also very busy with the remarkable class of double nitrites which I described at the meeting of the Am. Association for the Advancement of Science in August last. There are still some difficulties to be overcome, but I am

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confident that I shall be able to effect a perfect separation of all the metals of the platinum group. The white iridium salt appears to have the formula $Ir_2O_3.2NO_3+3KO.NO_3$, but it contains a small quantity of chlorine which may be essential. Its stability and insolubility are very remarkable, and will I think prove of great value in separating iridium from the other metals. The corresponding salts of the other metals of the group appear to be all soluble. The ruthenium salt is soluble even in alcohol and ether, and gives with sulphid of ammonium a magnificent red solution. This is by far the most delicate test for ruthenium yet discovered as the reaction is peculiar to that metal. Claus' beautiful reactions with sulphocyanid of potassium and sulphydric acid are much inferior for qualitative purposes."

New York, March 30, 1860.

TECHNICAL CHEMISTRY.

9. *Solution of Cellulose in Ammonio-oxyd of copper.*—Some time since ERDMANN, in his *Journal für praktische Chemie* (lxxvi, 386) expressed the opinion that cellulose is not really dissolved by cuprate of ammonia, as stated by SCHWEIZER (*ibid*, lxxii, 109), but only swollen to a sort of thin mucilage like the well known limpid "solution" of starch.

This view was based upon the fact that when a clear solution of cellulose in $NH_3\,CuO$ is diluted with a large excess of water, the cellulose separates entirely in the course of a few days.

In defense of his original statement Schweizer now urges that the cellulose must be really dissolved by ammonio-oxyd of copper: since its fibres are unquestionably destroyed when this reagent comes in contact with them—as may be distinctly seen with the microscope; and since the cellulose precipitated from a solution of cotton in the above mentioned reagent no longer exhibits any trace of definite structure.

A solution of cotton in $NH_3\,CuO$ may also be filtered perfectly clear—although this operation is somewhat difficult when large quantities of the liquid are operated upon. The solution is moreover capable of passing through the cell membranes of plants as shown by CRAMER (*ibid*, lxxiii, 6).

The destruction of the solvent power of the cuprate of ammonia by dilution appears to depend upon alterations which this compound itself undergoes under certain circumstances. It often happens that a solution of ammonio-oxyd of copper which at first dissolved cotton with the greatest ease gradually loses this power even when kept in carefully closed vessels completely filled with the liquid. It is moreover well known to chemists that solutions of ammonio-oxyd of copper, and of the ammonio-copper salts, undergo decomposition when diluted with large quantities of water;—hydrate of copper being precipitated.

This decomposing influence which water exerts upon solutions of the compound of ammonia and oxyd of copper is in the opinion of Schweizer the cause of the gradual precipitation of cellulose from such solutions when these are largely diluted.—*Journal für praktische Chemie*, lxxviii, 370; compare also CRAMER, *ibid*. lxxiii, 1, *et seq*.

10. *Decoloration of Indigo by Sesquioxyd of Iron.*—According to KUHLMANN, when a solution of blue indigo is acted upon at the temperature of 150° (C.) by hydrated oxyd of iron its color is, almost immediately, completely destroyed. The same thing occurs with a number of other coloring matters.—In noticing this fact BARRESWIL suggests that persul-

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phate of iron may perhaps be applied in calico-printing as a discharge for indigo and also in bleaching blue rags for paper making.—*Répertoire de Chimie Appliquée*, Oct. 1859, p. 429.

[The observation that salts of the sesquioxyd of iron have the power of bleaching indigo and other organic coloring matters was first made by Prof. H. Wurtz of Washington and published two years since in this Journal (vols. xxv, 378, and xxvi, 52)—also in the Proc. of Amer. Assoc. 1858 and in several foreign journals—to him unquestionably belongs whatever credit may attach to the discovery—E. S.]

11. *Aluminum Leaf*—A Parisian gold beater, Degousse, has succeeded in obtaining leaves of aluminum as thin as those from gold and silver. The aluminum must be reheated repeatedly over a chafing dish during the process of beating. This leaf is less brilliant than that of silver but it is not so easily tarnished as the latter. It is easily combustible, taking fire when held in the flame of a candle and burning with an exceedingly intense white flame.

According to Fabian, (Dingler's polyt. Journal, cliv, 438,) the chemical lecturer will find aluminum leaf to be well adapted, for exhibiting the characteristic properties of the metal. It dissolves, for example, with surprising rapidity in a solution of caustic alkali.

[A specimen of this leaf accompanies the description of it in *Répertoire de Chimie Appliquée*, Oct. 1859, p. 435, also Nov., p. 488.]

12. *Critical and Experimental Contribution to the Theory of Dyeing.*—Under this title a somewhat extended treatise by Prof. Bolley of Zurich has appeared in the L. E. and D. Philosophical Mag. [4] xviii, 481, Supplement to Dec. 1859.

Two questions have long been agitated among chemists interested in the theory of dyeing. (1.) In what part of the colored fibre is the coloring matter situated? Does it merely adhere to the surface, or does it penetrate the entire substance of the cell-walls of such fibres as cotton and flax? Or lastly, in the case of such fibres is it stored up in the interior of the cells? (2.) What is the nature of the union between the dye and the fibre? Is it a chemical combination, or is it due to mere surface attraction? After comparing the various theories which have been advanced during the last century and discussing the merits of each, the author records the results of his own experiments, from which it appears that wool and silk in all cases where they have not been dyed with colors in a mere state of suspension* seem to be impregnated with the dye throughout their entire mass; while in the case of cotton, by far the larger portion of the coloring matter adheres to the surface of the fibre, the penetration of the cell-walls by the dye being either very slight or altogether wanting.

That the theory of W. Crum (L. E. and D. Phil. Mag., April, 1844,—compare this Journal, [2], xxviii, 125), in accordance with which the tubular form of the cotton fibres is an essential condition to their taking a dye, is unfounded, appears from the fact that the amorphous cotton-gelatine precipitated from its solution in cuprate of ammonia (see this Journal [2], xxvii, 118) may be mordanted and dyed like ordinary cotton. In like

* In which case the coloring matter only adheres as a crust to the surface of the fibre.

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manner sulphate of baryta and other pulverulent mineral bodies may be mordanted and dyed with decoctions of dyewoods.

With regard to the nature of the force which binds the coloring matter to the fibre—whether or no it be chemical attraction? Bolley concludes that there is no sufficient reason for accepting the view, principally developed by Chevreul [and by Kuhlmann, Comptes Rendus. Tomes xlii, xliii *et* xliv], that dyeing is a direct consequence of chemical affinity. He believes that the power possessed by fibres of attracting certain bodies—whether salts or coloring matters or both—from their solutions, belongs to that class of phenomena which results from the action of finely divided mineral or organic bodies (charcoal or bone black for example,) on such solutions. The distinction between the action of charcoal and of fibres in thus removing saline matters, or dyes, from their solutions is one of *degree* only, the nature of the operation being identical in either case.

A given weight of well prepared animal charcoal can, as a rule, deprive a larger quantity of liquid of its color than an equal weight of wool or silk. Neither wool or silk can remove *all* the color from a solution as charcoal can, their effect extending only to a certain degree of dilution beyond which the particles of coloring matter resist their attraction. Dyes which may have been taken up without a mordant by wool or, especially, by silk may be removed again by long washing in water, a fact which is not true in the case of charcoal, or only to a very slight extent. The attraction of coloring matters for water is therefore more completely overcome by charcoal than by animal fibre; but even the cleanest vegetable fibres, as, unmordanted and completely bleached cotton, possess a certain power of attracting coloring matter. That cotton should have less effect in this matter than wool or silk is not surprising in view of the great difference in the structure of cotton fibre as compared with that of the two substances last mentioned. It is well known that wool and silk in consequence of their physical constitution belong to the class of strongly absorbent or hygroscopic substances, *i. e.* in consequence of a certain porosity or looseness of their particles they swell up when moist and become easily penetrated by a liquid throughout their entire mass; on the other hand the cell-walls of cotton fibres are denser, less penetrable and at the same time thinner and therefore unable to contain the same quantity of liquid.

It has been often urged that since fibres, especially those of animal origin, not only exert an attraction for salts &c. but also possess the power of decomposing some of them, their action must be chemical But in this respect the behavior of charcoal is similar to that of the fibres. So too with regard to the increased attraction for color exhibited by mordanted cotton which is on a par with the fact observed by Stenhouse that the decolorizing power of wood charcoal is considerably increased by precipitating alumina upon it.

According to the Author mordants act by producing insoluble colors (lakes). Their behavior towards coloring matters in solution must be ascribed to chemical affinity, with which however the fibres themselves have nothing to do.

The so-called substantive dyes become insoluble from some other cause than the addition of a mordant, for example oxydation of protoxyd of iron, or of white indigo.

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That common alum with which wool or silk has been impregnated is able to attract coloring matter from solutions and precipitate it on the fibres depends not upon the strength of the chemical affinity of these fibres for the coloring matter, but upon the fact (experimentally proved by Bolley) that they become saturated with the alum which cotton does not.

13. *Cellulose Digested by Sheep.*—The researches of several German chemists* have proved that the cellulose of plants is by no means so indigestible a substance as was at one time supposed, but that on the contrary it is digested in considerable quantities, by the ruminants at least, especially when a portion of the food of the animal consists of some substance rich in oil.

In order to ascertain to what extent the digestibility of cellulose may depend upon its state of aggregation, SUSSDORF and A. STŒCKHARDT have undertaken a series of experiments, of which only a very brief abstract can be here given. From their results it is evident that even the most compact kinds of cellulose can be in great measure digested by sheep. The experiments, commenced in July, 1859, were upon two wethers respectively five and six years old. These were fed: 1st, upon hay alone; 2d, upon hay and rye straw; 3d, hay and poplar wood sawdust which had been exhausted with lye; in order that the sheep should eat the sawdust it was found necessary to add to it some rye-bran and a small quantity of salt; 4th, hay and sawdust from pine wood mixed with bran and salt; 5th, hay, spruce sawdust, bran and salt; 6th, hay, paper-maker's pulp from linen rags and bran; after several unsuccessful attempts to induce the sheep to partake of the pulp when mixed with dry fodder it was at last given to them in a sort of paste or pap prepared by mixing bran with water. The experiments were continued until November, with the exception of a short intermission during which the animals were put to pasture in order that they might recover from the injurious effects—probably due to the resinous matters of the spruce wood,—of the fifth series of experiments.

The animals, as well as their food, drink and excrements were weighed every day. The amount of cellulose in the excrements was also daily determined by analysis. The composition of the food ingested having been previously ascertained.

It appeared that when the animals were fed: (1.) with hay (35 lbs. per week), 60 to 70 per cent of the cellulose contained therein was digested, *i. e.* it did not appear as such in the solid excrements. In this experiment the animals gained 7½ lbs. in 18 days. (2.) With hay 14 lbs., and straw 7 lbs. (per week), 40 to 50 per cent of the cellulose of the straw was digested. The animals having lost 2½ lbs. in 11 days. (3.) With hay 10½ lbs., poplar saw-dust 5¼ lbs., bran 7 lbs. (per week), 45 to 50 per cent of the cellulose of the poplar wood was digested. The animals having gained 2½ lbs. in 13 days. (4.) With hay 10½ lbs., pine wood saw-dust 7 lbs., bran 10½ lbs. (per week), 30 to 40 per cent of the cellulose of the pine wood was digested. The animals having gained 10 lbs. in 24 days. (5.) With hay 9⅛ lbs., paper-maker's pulp 7 lbs., bran 14 lbs. (per week),

* For a portion of these interesting results, see: *Agriculturchemische Untersuchungen und deren Ergebnisse angestellt u. gesammelt bei der landwirthschaftlichen Versuchstation in Mœckern.* Leipzig, WIGAND, 1852-57; also *Die landwirthschaftlichen Versuchs-Stationen.* Dresden, WERNER, 1858-59.—F. H. S.

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80 per cent of the cellulose of the paper pulp was digested. The animals having gained 7 lbs. in as many days.

These experiments are to be continued, and more particularly with a view of ascertaining whether any nourishing effect is to be attributed to the cellulose.—STOCKHARDT'S *Chemischer Ackersman*, 1860, No. 1, p. 51.

II. GEOLOGY.

1. *Notes on the Geology of Nebraska and Utah Territory*, (in a letter to one of the Editors from Dr. F. V. HAYDEN, dated Fort Laramie, March 3d, 1860.)—It will be seen by referring to the several memoirs, published in connection with my associate, Mr. Meek, and the second edition of a geological map of Nebraska and Kansas, that the great Lignite Tertiary Basin covers a vast area in the northwest. We find by personal observation that it occupies the greater portion of the country bordering on the upper Missouri, Yellow Stone and Big Horn rivers, that it extends far up into the Wind river valley and west along the North Platte road to the Sweet Water mountains, the Cretaceous rocks being exposed here and there by local upheavals, only except along the base of the mountains.

The lignite beds, which are well developed south of Fort Laramie extending along the base of the Laramie mountains to the Arkansas and southward, furnishing the coal or lignite in the vicinity of Denver City and probably forming a part of the same basin.

I have, in a former paper, suggested that fresh-water deposits near Fort Bridger are probably on a parallel with the estuary beds of Judith river, which at that time were not positively known to be Tertiary. The facts now in my possession show, with a good deal of certainty, that they form the lower portion of the great Lignite Basin. These estuary deposits, which occur in a number of localities in the west and northwest, as along the Grand and Cannon Ball rivers, at the mouth of the Judith on the Missouri, near the mouth of the Big Horn on the Yellow Stone, seem to have ushered in the tertiary epoch of the West, which had already been foreshadowed in Cretaceous formation No. 5,* by the Tertiary character of the Mollusca. We have already, in a former paper, noted the fact that a large portion of the fossils peculiar to the Cretaceous formation No. 5, are closely similar to true Tertiary types and in most of the localities the transition from No. 5 to the estuary beds is scarcely perceptible. On the North Platte, especially at Deer Creek, No. 5, which is very largely developed in this region, is not unfrequently thrust up through the overlying lignite beds, charged with its characteristic fossils. Along the bluff banks of the stream, where the beds are but slightly disturbed, the order of sequence of the strata is so perfect that I would not have been in doubt where to draw the line of separation until we came to the first seam of lignite, and even then I would have considered several beds of the Lignite formation as the upper portion of No. 5 had I not found in these lower lignite beds *Unios* and other fresh-water shells, together with impressions of leaves identical with those occurring so abundantly in the Upper Missouri and Yellow Stone Tertiary strata, and furthermore these beds on the

* The Cretaceous series of Nebraska has been divided into five formations, which for convenience have been numbered from the base in ascending order, 1, 2, 3, &c.

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North Platte have now been traced continuously over the intervening country from the mouth of the Yellow Stone river to the Platte. I have ascertained the fact that the lignite beds along the North Platte are a continuation of those on the Upper Missouri, and that they extend in their full developement far up into the Wind river valley and along the Platte road to the Sweet Water mountains. As yet I have seen no indications of lignite in any of the divisions of the Cretaceous period except in formation No. 1 near the Big Sioux river on the Missouri and in a series of sandstones and shales near Fort Benton which we have referred to the same rock. As we proceed south and southwest in this region No. 1 seems to disappear gradually, and along the Laramie mountains I cannot determine its existence at all.

The geographical extension of the great Lignite Basin seems to me to be one of the most interesting questions in the geology of the West at the present time. Very little is known as yet of its limits and from the interesting facts collected by Dr. Engelmann and from other sources it must occupy a large area to the southward and westward from this point, and we already know that it extends far northward into the Hudson's Bay vicinity.

In regard to the White river Tertiary Basin its boundaries have been published with a good degree of accuracy. Its limits north of the Platte river are now well known, and as I have already stated in a former paper, one of the upper members of that basin is revealed along this river, and these, in their southern and southwestern extension, pass by a gradual transition into the Yellow Marl or superficial deposits of the Quaternary period. That the White river Tertiary beds are of later date than those of the Lignite Basin, is clearly shown by the former having been observed resting conformably upon the latter in several localities.

2. *Note on Prof. Newberry's criticisms of Prof. Heer's determination of species of North American Fossil Plants*, in a letter to Prof. Asa Gray, Cambridge.—*Dear Sir:* When I offered for publication in this Journal, the translation of part of a letter from Prof. O. Heer, concerning some fossil plants of the Tertiary, I was far from supposing that any of the statements of my learned friend would not appear satisfactory to every one interested in the study of our American Palæontology. Much less could I foresee that those statements would be construed in a manner that I do not think quite justifiable. As Prof. Heer's letter was published without his knowledge and sanction, I am forced, much to my regret, to defend his position against Dr. Newberry, a personal friend also, and a true and faithful pioneer in the field of our botanical palæontology.

I know nothing about the discussion on the Cretaceous formations and fossils, except what has been published in this Journal. And although last year, during my connection with the State Geol. Survey of Arkansas, I had the opportunity of examining well exposed strata at different stages of the Cretaceous, I was unable to find there any fossil plants, and therefore I have never seen as yet an American Cretaceous plant. Thus I can take my arguments only from the statements of Dr. Newberry himself. It is unnecessary to recall the five points in discussion.

The two first statements are, even from the assent of Dr. Newberry, satisfactorily explained by the insufficiency for exact determination of

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sketches made from incomplete specimens. Nevertheless, it is but right to remark in favor of the opinion of Dr. Heer that *Populus Leuce* has not the leaves toothed as Dr. Newberry says, but only *subdenticulate*, following Unger's figure and description. Moreover, species of *Populus* bear toothed and entire leaves on the same branches.

The third assertion, about the value of the genus *Ettingshausenia*, concerns only the author of the genus and has nothing to do with the determination of the species. There was no call for a critique upon Prof. Heer on this account.

The fourth statement is the only essential one; it is: "that *excepting the so-called Credneria and Ettingshausenia, all the genera enumerated in Dr. Newberry's letter are Tertiary and not Cretaceous.*"

Nobody will consider as identical the genera *Salicites* and *Salix*, *Alnites* and *Alnus*. Then, from the eleven genera enumerated by Dr. Newberry, two only are found in the list of Hiehle, containing seventeen genera. These two genera are *Populus* and *Acer*, the true characteristic genera of the tertiary of Europe. The first is represented already in that formation by thirty-three species; the second by forty-five or more, and except the species admitted by Hiehle, not a single one has been found in the Cretaceous. From the nine other genera of Dr. Newberry which are not mentioned as Cretaceous by Hiehle, not one has been found by anybody else in that formation. Hence Prof. Heer was right to say that, except the two which he named, all the genera enumerated in the letter of Dr. Newberry were of the Tertiary and not of the Cretaceous. Every botanist without exception would have come to the same conclusion.

Therefore I cannot understand how the accusation of ignorance or of partiality could be brought against the celebrated Professor of Zurich. How it could be thought that a naturalist who has spent his life, without regard to personal interest, in the constant pursuit of his favorite science (the Botanical Palæontology of all the formations), could be supposed to ignore not only the fossil flora of the Cretaceous, but what Hiehle has published in the Palæontographia. This work is in every library and Prof. Heer has quoted it all along in the three volumes of his admirable Fossil-flora of the Tertiary! And how a man of such high moral standing as Prof. Heer, respected everywhere for his faithfulness and devotion to science, could be accused of giving his opinion or rates of determining the plants from a *judgment biased by erroneous oral testimony*, is still more inconceivable to me.

In the letter of Prof. Heer, there is not a word which could be construed as censurable or offensive to anybody. When the most learned Botanists take again and again species and genera for re-examination, new determinations, for a constant changing of names, of relations, of affinity, even with specimens of living plants; when such Palæontologists as Heer, Corda, Unger, Göppert, Braun, &c., who have on hand the largest libraries, numerous specimens, and every facility for comparing them are forced every day, either by new discoveries or by a more careful study, to acknowledge mistakes and to change their nomenclature; when the same fossil leaf is for different authors a *Populus*, a *Salix*, a *Laurus*, a *Ficus*, a *Quercus!* it cannot be expected that in America, where the science of Botanical Palæontology is scarcely born,

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where the age of the strata from which the fossil plants are taken is mostly uncertain, where we have no possibility of comparing specimens, and not a single library where we can find all that has been published on palæontology, we can come at once by some kind of divination to the correct determination of a fossil species. And most commonly the examination has to be made on pieces of broken specimens of leaves, of which the general outline and the details of nervation are obliterated. We have thus to begin and to break the way, and the only means of doing it with advantage to our successors is to publish our fossil species as fast as we can get them, figuring them carefully and determining the species as well as we can without caring for any foreign opinion.

About the fifth statement of Prof. Heer, I can not admit, as Dr. Newberry appears to do, that the fossil flora of the American Cretaceous ought to be closely related to the European. The Tertiary of North America, of which numerous specimens have recently been collected in Mississippi, Tennessee and Kentucky, has some of the genera of Europe, *Sabal*, *Phœnicetes*, *Laurus*, &c., represented by peculiar species different from those of Europe, with a large number of *Terminalia*, *Magnolia*, two genera scarcely represented in the European tertiary. But this question cannot be examined now.

Contrary to the supposition of Dr. Newberry, it is certain that the fossil plants obtained from three different stages of the Tertiary in Mississippi, Tennessee and Kentucky, indicate a warmer climate than that of the same latitude at our time. And it is certain also that already two marine fossiliferous beds (very rich indeed) have been discovered, the one near Cairo in Illinois, by Prof. A. H. Northen; the other near Oxford, by Prof. Hilgard, and perhaps a third one in Tennessee by Prof. Safford,—three State Geologists of the three different States. I have spent some time last fall in the examination of the formations; and besides the specimens of fossil leaves which I have collected, I have now for examination the rich collection of the Mississippi University of Oxford, and the private collections of fossil plants of both Prof. Hilgard and Prof. Safford. It is from these materials partly examined that I have taken the above conclusions.

Respectfully Yours, LÉO LESQUEREUX.

Columbus, Ohio, April 3, 1860.

III. BOTANY AND ZOOLOGY.

1. *Florula Ajanensis*, by REGEL and TILING. Moscow, pp. 128, 4to, 1858. (Extr. Trans. Imp. Soc. Mosc.)

Primitæ Flora Amurensis, by C. J. MAXIMOWIEZ. St. Petersburg, 1859. (Extr. Mem. present. Acad. Im. Sci. St. Petersb.), pp. 504, tab. 10, 4to.—The first-named work, with the *Florula Ochotensis* of Trautvetter and Meyer in Middendorf's Journey, gives an account of the botany of the confines of Northeastern Siberia bordering on the Ochotsk Sea. The latter, a bulky volume, contains the first fruits of the botanical explorations consequent upon the colonization of the lower part of the Amoor River by the Russians. These districts nearly abut upon the northern part of Japan, and therefore possess for us a peculiar botanical interest. They share notably, though not as largely as Japan, in those North American types which present so curious a problem in geographical dis-

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tribution. Having been supplied with an almost complete suite of the specimens upon which these works are based, through the generosity of the directors of the Imperial Botanic Garden and of the Museum of the Imperial Academy of Sciences of St. Petersburg, we wish to collate the various representative forms both with our Japanese materials and with their American relatives before we offer any definite remarks. Suffice it now to say, that the Amoor flora offers several additional species identical with peculiarly Eastern North American ones: e. g. *Acer spicatum*, *Pilea pumila*, *Asplenium thelypteroides*, and *Symplocarpus fœtidus!* Also several closely representative ones; such as *Caulophyllum robustum*, doubtless the same as the Japan plant, which in fruit answers perfectly to our *C. thalictroides*, and I still suspect not distinct from it; and *Maximowiczia Chinensis*, Rupr. (to which evidently belongs *Sphærostema Japonicum*, Gray), a close counterpart of our *Schizandra*; *Acer tegmentosum*, very nearly our *A. Pennsylvanicum*; *Hylomecon vernalis* which seems very close to our *Stylopnorum diphyllum*; and *Plagiorhegma dubium*, which has the look of a monstrous dicarpellary *Jeffersonia*. Indeed, good flowers are still wanting to make it at all certain it is not a *Jeffersonia!*

Very remarkable indeed is this division of monotypic or nearly monotypic genera or groups between Northeastern Asia and Northeastern America,—of which so extended a list can now be given,—and very suggestive is it (at least where the species are identical or nearly so) of a comparatively recent communication between the two countries. A. G.

2. *Harvey's Thesaurus Capensis*, No 2, has come to hand. The plates (26–50) are better printed and do more justice to Dr. Harvey's facile pencil than those of the first part. Among the illustrations is a new *Crotalaria*, "a sort of first fruits of the botany of Lake Ngami;" an interesting new Bixaceous genus (*Rawsonia*), with petaloid scales "evidently homologous with the crown of *Passiflora*, and with the inner stamens hypogynous, the outer perigynous"; also a fine new Scuphularineous genus, *Bowkeria*.—No. 3, just received, continues the work to plate 75. One of the plates illustrates a new genus of the Passion-flower family, with blossoms no larger than chickweed. A. G.

3. *Hooker's species Filicum, being Descriptions of all known Ferns*, illustrated with Plates. Part ix, or vol. iii, part 1. pp. 64, tab. 141–170.—This portion of this important and standard work contains the genera *Lomaria* and *Blechnum*, and excellent figures of 23 species. The most interesting Fern to us, here illustrated, is the *Blechnum doodiodes* of Hooker, known only from two specimens gathered by Douglas somewhere in Northwestern America, "probably up Frazer's River": the question is whether the plant may not prove to be a remarkable variation of *Lomaria Spicant*, which inhabits that region, and is the only Lomarioid Fern of so northern a latitude. A. G.

Part x, or vol. iii, part 2 of this important work has just come to hand. It comprises the first part of the very large and very difficult genus *Asplenium*, the reduction of which to order will be a great boon to botanists.

4. *Journal of the Proceedings of the Linnæan Society*, No. 14: Botany. —This part concludes Prof. Andersson's account of the *Salices* of the East Indies, 34 species, of which many are new. Mr. Spruce has a short article on the Piassaba Palm (*Leopoldinia Piassaba*) of the Rio Negro of the Am-

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azon, the long fibrous beard of whose petioles (vestiges of the membrane which envelopes the frond in vernation) is so important for cordage, &c. On young palms this pendant beard is four feet or more in length, and reaches to the ground. Besides this useful fibre, the pulp of the ripe fruit of this palm is said to yield the most delicious of all palm-drinks, having great resemblance to cream both in color and taste. The remaining article is one by Mr. Mitten, upon various new *Musci* of New Zealand, Tasmania, and various parts of the Southern hemisphere.

No. 15, contains Observations on the Growth and time of Appearance of some of the Marine *Algæ*, by Dr. Cocks; and another of the Præcursores ad Floram Indicam, by Drs. Hooker and Thomson. The present paper is devoted to the *Balsamineæ*, amounting to about one hundred species, notwithstanding a large reduction. The great mass of the order belongs to India; the extra-Indian Balsams known being about eight in more oriental parts of Asia and Maylasia; about as many more in Africa and Madagascar, three in Europe and Siberia, and two in North America. The Indian species are "so universally and excessively prone to vary" that they have given their monographer immense trouble and small satisfaction. We may remark that our two American species and *I. Nolitangere* of the Old World seem to run together; at least, intermediate forms occasionally appear which strongly suggest a common origin. A. G.

No. 16, contains a paper by Mr. Babington on the forms or species compared with *Fumaria capreolata*, L.; another on a new Butter-tree of Southeastern Africa by T. Caruel of Florence,—the tree which Bertoloni supposed to be the shea-tree mentioned by Mungo Park, and which he described as the type not only of a new genus but of a new order. Mr. Caruel shows that the present tree is a *Combretum*, and doubtless not the shea-tree, the only grounds for considering them the same being that both belong to Africa, and both produce a kind of vegetable butter. Mr. Oliver, a young English botanist of promise, describes some new South American species of *Utricularia* (two of which are figured), with notes on *Polypompholyx* and *Akentra*, showing that Benjamin established the latter in a mistaken observation, and that the plant is an *Utricularia*. Mr. Spruce gives a very interesting account of a visit to the *Cinchona* forests of the Quitenian Andes. The *Cascarilla roja* was said to have a milky juice, a remarkable circumstance for a Rubiaceous plant. It appears that the juice as it flows from a wound is colorless, but that it turns white the instant it is exposed to the air, and in a few minutes changes to red. Mr. Moore announces the discovery in England of *Lastrea remota* (*Apidium remotum*, Braun), a form which has been confounded with *A. rigidum* and which appears to be as intermediate between *A. spinulosum* and *A. Filix-mas*, as *A. rigidum* is between the former and *A. cristatum*. Mr. Oliver continues an account of the British Herbarium now forming for the Linnæan Society; and Mr. Hogg, a note on the *Rosa rubella* of Winch., giving evidence that it is a mere variety of *R. spinosissima*. A. G.

5. *Martius, Flora Brasiliensis.*—This great work is conducted with such spirit,—now that the distinguished editor is able to devote his whole attention to it,—that three fasciculi reach us at the same moment, viz.:

Fasc. 18, part 3, is a supplement to the *Myrtaceæ* of Brazil, by Berg, of Berlin. This young botanist is thought to have elaborated the Amer-

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ican forms of this great order in an able manner. Of the many new species and varieties described in this supplement, the greater part are from the Brazilian collection of Riedel, belonging to the Imperial Botanic Garden of St. Petersburg, and furnished to Dr. Berg by Dr. Regel, the accomplished curator of that establishment. To this is appended a tabular view of the geographical distribution of all the known American *Myrtaceæ*, now amounting to 1726 species, of which 1008 are nearly described by Berg (in the Fl. Brazil., and in the Linnæa), and 696 are indigenous to the Brazilian empire. The uses which the plants of the order are known to subserve in Brazil are likewise enumerated. Of four species the root, or its bark, is used medicinally: of three species the fibrous bark is turned to account, among which, from that of a *Couratari* the natives of the Uapes make clothing; the timber of *Couratari legalis*,—one of the largest trees of tropical Brazil—is so highly prized for a variety of purposes that its wasteful destruction is forbidden by law, whence the name *paos de ley*, i. e. *ligna legalia*: the leaves of several species are used medicinally for their aromatic properties combined with astringency: the flower-buds of a *Calyptranthes* are used as a substitute for the true Clove, which also is cultivated successfully in Brazil: the berries of no less than 55 species are enumerated as edible, or some of them medicinal; the most important being the *Guayavas*; five species of *Lecythis* and the famous *Bertholletia excelsa* furnish amygdaloid seeds of great richness and pleasantness (Brazil-nuts, &c.). Recent extrabrazilan genera are briefly noted. *Luma* (Bot. U. S. Expl. Exped.) is altogether overlooked, and in strictness may claim restoration, when the genera come to be revised and considerably reduced, as they probably will be. The three parts of fasc. 18, composing the *Myrtographia Brasiliensis* compose an entire volume, of 655 pages, with many plates.

Fasc. 23, is a continuation of the first fascicle (*Musci* and *Lycopodiaceæ*), and comprises the *Ophioglosseæ*, *Marattiaceæ*, *Osmundaceæ*, *Schizæaceæ*, *Gleicheniaceæ*, and *Hymenophylleæ*, by Dr. Sturm, of Nuremberg, with ten plates. One of these is a fine illustration of the rare and curious *Ophioglossum palmatum*,—a species which, by the way, Mr. Wright has lately collected abundantly in the eastern part of Cuba. Under the name of *Osmunda palustris*, Schrad., our North American *O. spectabilis*, a form of the European *O. regalis*, is figured: it appears to be rather common in the southern part of Brazil. It is interesting to remark that both this and two other more specially North American species, viz. *O. cinnamomea* and *Botrychium Virginicum*, occur on the one hand in Brazil, on the other in the Himalayas. The illustrations of the *Hymenophylleæ* are nature-printed, by the Vienna process. This does better for the fronds—so delicate in this tribe—than for the fructification.

Fasc. 24, of 215 pages, with 56 plates, contains the first part of Mr. Bentham's elaboration of the Brazilian *Leguminosæ*, including all the *Papilionaceæ* except the *Dalbergieæ* and the *Sophoreæ*. A double plate, filled with details, illustrates the structure of *Arachis*, the generic character of which, as well as of its allies, is remodelled, conformably to the observations of Dr. Niesler, as recorded in this Journal, several years ago. We perceive that the North American species referred to *Chæto-*

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calyx (the fruit of the original species being unknown), belong to the allied and older genus *Nissolia*. *Chætocalyx Wislizeni*, Gray, is the same as *Nissolia platycarpa*, Benth. *C. Schottii* must take the name of *Nissolia Schotti*. A. G.

6. *J. D. Hooker's Flora Tasmania*,—the third and concluding part of the *Botany of the Antarctic Voyage* under Capt. Ross,—is now finished in parts 10 and 11, issued by the enterprising publisher, Lovell Reeve, in February last. The work makes two large quarto volumes, with 200 colored plates. Part 10 concludes the *Algæ* (by Dr. Harvey) and contains the *Lichenes* (the foliaceous ones by Mr. Babington, the crustaceous by Mr. Mitten), also a series of additions and corrections to the earlier part of the work. The letter-press of part 11 is entirely occupied, excepting the key to the genera, by the *Introductory Essay* upon the Botany of Tasmania and of Australia in general, of 128 pages, which admirable production may claim to be regarded as the most profound and far-reaching discussion of the abstruse theoretical questions bearing upon the origin and distribution of species which has ever been attempted. Although the literary composition bears some marks of haste, the subject-matter has been elaborated with great care, and in a manner at once bold, independent, and conscientious, opening new views and propounding new problems of the widest interest and, we may add, of the utmost difficulty. The more generally interesting portions of this essay are reproduced in the present volume of this Journal. A. G.

7. *Poison of Plants by Arsenic.*—It may be recollected that Professor Davy of Dublin last year reported to the Gardener's Chronicle (whence extracts were transferred to our pages*) the results of his experiments which went to show that some plants might with impunity be watered even with a saturated aqueous solution of arsenious acid; that the plants took up this arsenic and accumulated it in their tissues, to such an extent that traces of this metal were discoverable in the bodies of animals fed upon vegetables so treated. These astonishing results naturally excited enquiry. They have now been contradicted in a late number of the Pharmaceutical Journal (as we learn from the Gardener's Chronicle for March 10) by Mr. Ogston, an analytical and agricultural chemist, formerly a pupil of Prof. Graham. Mr. Ogston finds that, on watering the ground around the roots of some vigorous Cabbage plants, some months old, with a saturated solution of arsenious acid, in every trial, after two doses at intervals of three days, the plants died within the week. The same occurred with Scotch Kale, the only other plant subjected to the experiment. On testing the dead plants arsenic was detected only in the portion of the stem close to the roots, and which showed in its darkened color the marks of disease. In no case was any of the poison found in the leaves, or in the stem at more than five inches above the ground. Prof. Davy also startled the English agriculturists and medical jurists by calling attention to the fact that arsenic exists in the commercial superphosphate of lime, at least in certain kinds, coming from the iron pyrites used in the manufacture of the sulphuric acid employed in the production of the superphosphate, which arsenic, if plants may accumulate it in their tissues, would be conveyed

* Vol. xxviii, p. 443, 1859.

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to the flesh of animals fed with turnips manured with such superphosphate, and so conveyed to the human system,—if not in quantity sufficient to poison, yet enough to account for the presence of arsenic in cases of death from supposed poisoning. Mr. Ogston now considers the question as to how much arsenic an agricultural crop (say of turnips) can obtain from an ordinary dressing of the superphosphate so prepared. "Take a very bad sample of pyrites to contain ·30 per cent of arsenic, and consider, as is the case, that in the manufacture of oil of vitriol, one-half of this is stopped by condensation in the flues; ·15 per cent will remain in relation to the pyrites, or about ·10 in relation to the manufactured oil of vitriol. Now suppose the superphosphate made from this acid to contain 20 per cent of it as a constituent, and that 3 cwt. are used as a dressing per acre, there will be added to this acre ·07 of a pound of arsenic, and this is to be distributed among from 20 to 25 tons of roots, giving a percentage infinitely small, and in my opinion relieving us from the necessity of the smallest anxiety on the subject. If, however, even this quantity is objectionable, the use of the Belgian pyrites, in which I have never found a trace of arsenic, would obviate all difficulty." A. G.

8. *Botanical Necrology for the year* 1859.—The following are the principal names upon this obituary record:—Some of them we have named before more briefly.

C. A. Agardh, Professor of Botany in the University of Lund, Sweden, from 1812 to 1834, when he became bishop of Carlstad; a voluminous writer upon botanical and other subjects, especially upon *Algæ*, and a distinguished and remarkable man. His earliest publication, a thesis upon the *Carices* of Scania, was published in the year 1806. He died on the 28th of January, 1859 at the age of 75 years. He was succeeded in his professorship by his son C. A. Agardh, the distinguished algologist.

Arthur Henfrey, Professor of Botany in King's College, London. The death, after a short illness, on the 7th of September last, of this amiable man and excellent vegetable anatomist, at the early age of 39 years, has has already been recorded in this Journal, (vol. 28, p. 443). In his field of research and in his knowledge of the literature of the subject, especially that of the Germans, he had no rival in Great Britain, and his death is deeply felt.

Dr. Thomas Horsfield, born in Pennsylvania, after completing his medical studies in Philadelphia he passed sixteen years in Java and the adjacent islands, in the service of the government, devoting much of his time to botanical and zoological researches; and the long remainder in a responsible position at the India House, in London. A selection only of his botanical collections was published by Mr. Brown and Mr. Bennett, under the title of *Plantæ Javanicæ Rariores*, etc.—a most important work. Dr. Horsfield died, on the 14th of July last, in the 86th year of his age.

A. L. S. Lejeune, a venerable Belgian botanist, died at Verviers, at the very close of the year 1858, in the 80th year of his age.

Thomas Nuttall, born at Settle, in the West Riding of Yorkshire, in the year 1784, may yet be reckoned as one of our own American botanists, since he came to the United States when only 22 years of age, and

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made here his whole scientific career. He returned indeed to his native land in the year 1842, and took possession of a handsome estate bequeathed by his uncle, where he indulged his fondness for horticulture; but his only botanical publication in these later years was made at Philadelphia, and elaborated during a visit to this country in 1852. His writings are intimately connected with the history of North American Botany, and his personal biography is very interesting. A full account of these is given by Mr. Durand, in an excellent address delivered to the Philadelphia Academy of Natural Sciences, and in an article by Mr. Meehan in his Gardener's Monthly for January, 1860. A critical estimate of Mr. Nuttall's contributions to science must be deferred to another occasion. He died at his residence, Nutgrove, near Preston, Lancashire, on the 10th of September last, aged 75 years. A. G.

Zoological Notices.—

1. *A trip to Beaufort, N. Carolina;* by Wm. Stimpson, M.D.—The vicinity of Cape Hatteras, the most projecting point of our coast south of New York, has a peculiar interest for the student of zoology. This cape, which divides the Areniferous region* into two nearly equal parts, the Virginian and Carolinian provinces, is remarkable for the exhibition of a fauna more tropical in character than that of either of these provinces, as will be shown below. This is an evident result of its proximity to the Gulf Stream, the warm waters of which are even said to be deflected directly upon the Cape after violent southeast gales.

Beaufort, N. C., which lies several miles WSW from the Cape, in latitude 35° N. is the only convenient point of departure for explorations in the waters of the vicinity. Some account of the zoological richness of this locality was kindly communicated to me by Capt. J. D. Kurtz, U. S. A., and influenced by a desire of completing a catalogue of the shells of our Atlantic shores, and particularly of procuring and examining a species of *Lingula* said to be found on the southern coast, I undertook its exploration in the month of March last, in company with my friend Mr. Theodore Gill. The harbor of Beaufort is situated at one of the southern outlets of Pamlico Sound, where it joins Bogue Sound. It is shallow and much obstructed by extensive shoals. Centre-board boats only can be used, except in the deeper channels, which are mostly narrow. The bottom is generally sandy, but that of the deep channels is shelly, and that of the shallower channels often muddy. Outside the harbor and off the coast, the depth never exceeds eight fathoms within a few miles of the land, with a variable bottom, sometimes "sticky" or clayey. These bottoms were all pretty thoroughly raked with the dredges. The beaches were also examined for those bivalve shells which perforate the sand to a

* The eastern coast of the United States may be conveniently divided into three regions, viz.—the Rupiferous, Areniferous, and Coralliferous, named from the character of the shores. The Rupiferous or rocky region extends from our northern limit to Cape Cod, or Long Island. The Areniferous or sandy region,—in which there are no rocks whatever and scarce even a pebble except where human agency has been at work—reaches from Long Island to North Florida. The Coralliferous region is characterised by the presence of reef-coral, and includes the peninsular of Florida. Each of these regions has its peculiar fauna, the distinctness of which, enhanced by the two great causes of difference of latitude and diversity of ground, is too well known to require further comment here.

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depth beyond the reach of the dredge and are only exposed by the eroding action of the breakers during heavy storms. In following these beaches we observed a decided increase of the tropical character in the shells as we proceeded eastward toward the Cape.

In order to show the character of the fauna and the results of our explorations I have given below a catalogue of the Mollusca and higher Crustacea which occurred during our short stay, which seems to be the most concise and satisfactory method. A few prefatory remarks of a general character will not be out of place. Geologists will be interested to notice the occrrence of several species hitherto known only as Tertiary fossils, such as species of *Axinaea*, *Lucina*, *Astarte*, *Amphidesma*, *Tellidora*, *Myalina*, *Panopaea*, *Entalis*, and *Columbella*. These were found either alive or in such condition as showed them to be recent shells, which would doubtless have been found alive upon further search. The occurrence of *Myalina subovata* is interesting, although our specimens of this species, as of *Amphimesma constricta*, are not certainly recent, being only single valves. Of the beautiful *Tellidora lunulata* we obtained several living examples, some attaining a length of nearly two inches. Among the shells of a tropical character several species will be noticed which have not hitherto been found north of the West Indies, and do not exist upon the South Carolina coast. The large *Cassis* to which we have applied the name *C. cameo* is identical with the common cameo-shell of the Bahamas, which usually figures in collections under the name of *C. madagascarensis*. Of this we obtained several fine specimens on the beaches, none living however.

In the following catalogues all species of which the identifications is at all doubtful are indicated by the mark of interrogation.

Mollusca.

Cynthia vittata.
Molgula sp.
Ascidia sp.
Lingula pyramidata.
Anomia ephippium.
Ostrea virginiana.
" equestris.
Plicatula depressa.
" sp.
Lima scabra?
" sp.
Pecten dislocatus.
" concentricus.
" nodosus.
Axinæa charlestonensis.
" sp.
Arca Holmesii.
" americana.
" cælata.
" lienosa.
" noæ?
" ponderosa.
" incongrua.
Nucula proxima.
Yoldia limatula.
Leda acuta.
Pinna squamosissima.
" carolinensis.
Avicula atlantica.
Modiolaria lateralis.
Modiola plicatula.
" americana.
" tulipa?
Mytilus edulis.
" cubitus.
Chama arcinella.
" macrophylla.
Cardium magnum.
" isocardia.
" muricatum.
Liocardium serratum.
" Mortoni.
Lucina crenulata.
" cribraria?
" edentula.
" strigilla.
Felania sp.
Diplodonta? punctata.
Lepton lepidum.
Astarte lunulata.
" undulata?
Cardita tridentata.
Mercenaria violacea.
" Mortoni.
" notata.
Trigona sp.
Venus rugosa?
Chione cancellata.
" pygmæa?
Callista gigantea.
" maculata.
Dosinia discus.
Lucinopsis sp.
" n. sp.
Petricola pholadiformis.
Raëta canaliculata.
" lineata.
Mactra oblonga.
" similis.
" lateralis.
Donax variabilis.
Cumingia tellinoides.
Semele orbiculata.
" reticulata?
Amphidesma constricta.
Abra æqualis.
Tellina alternata.
" fausta?

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Tellina polita.
" tenera.
" versicolor?
" tenta.
" iris.
" constricta.
" sp.
" n. sp.
Strigilla carnaria.
" flexuosa.
Tellidora lunulata.
Solen ensis?
" viridis.
Siliquaria gibba.
" bidens.
Solenomya velum.
Mya arenaria.
Corbula contracta.
Myalina subovata, *Con.*
Panopæa americana.
Saxicava distorta.
Gastrochæna sp.
Lyonsia hyalina.
" sp.
Pandora trilineata.
Pholas costata.
" truncata.
Pholadidea cuneiformis.
Teredo sp.
Polycera sp.
Actinodoris? sp.
Utriculus canaliculatus.
Bulla solitaria.
Tornatella puncto-striata.

Chiton apiculatus.
Entalis pliocena.
Dentalium sp.
Crepidula unguiformis.
" fornicata.
" convexa.
Clypidella pustula.
Fissurella alternata.
Zizyphinus sp.
Turbo crenulatus.
Littorina irrorata.
Scalaria Humphreysii.
" turbinata.
" lineata.
" novangliæ?
" multistriata.
Solarium granulatum.
Vermetus radicula.
Cerithium sp.
Bittium nigrum?
" Greenii.
" sp.
Triforis nigrocinctus.
Odostomia seminuda.
" impressa.
Turbonilla interrupta.
" sp.
" sp.
Obeliscus crenulatus.
Eulima oleacea.
Catinus perspectivus.
Natica pusilla.
Neverita duplicata.
Volva uniplicata.

Pleurotoma cerina.
Mangelia rubella.
" filiformis, *Holmes.*
Oliva litterata.
Olivella mutica.
Columbella avara.
" ornata.
" lunata.
" n. sp.
Dolium galea.
Semicassis granulosa.
Cassis cameo, *Stm.*
Purpura floridana.
Murex spinicostatus.
Nassa obsoleta.
" trivittata.
" vibex.
" ambigua?
Cerithiopsis terebralis.
" ? n. sp.
Acus dislocatus.
" concavus.
Busycon pyrum.
" canaliculatum.
" carica.
" perversum.
Cancellaria reticulata.
Fasciolaria gigantea.
" tulipa.
" distans.
Ranella caudata.
Strombus alatus.

Crustacea Decapoda.

Libinia canaliculata.
Pelia mutica.
Leptopodia calcarata.
Cryptopodia granulata.
Cancer irroratus.
Menippe mercenaria.
Panopeus Herbstii.
Pilumnus aculeatus.
Platyonichus ocellatus.
Lupa hastata.
" Gibbesii.
" spinimana.
Gelasimus pugilator.

Ocypoda arenaria.
Pinnotheres ostreum.
" maculatus.
Pinnixa cylindrica.
" sayana.
" chætopterana.
Persephona punctata.
Lithadia cariosa.
Hepatus decorus.
Calappa marmorata.
Porcellana ocellata.
" sociata.
Euceramus prælongus.

Hippa talpoida.
Lepidops scutellata.
Eupagurus pollicaris.
" longicarpus.
" annulipes.
Callianassa major.
Crangon septemspinosus.
Alpheus intermedius.
Virbius pleuracanthus.
Palæmonopsis carolinus.
Penæus brasiliensis.
" constrictus.

In the collection made at Beaufort and now deposited in the Smithsonian Institution a considerable number of new genera and species occur. We add descriptions of two of the most interesting.

LINGULA PYRAMIDATA. Shell greenish-white, elongated-ovate, convex, regularly tapering from the middle to the summit with an outline very slightly convex; also a little tapering toward the extremity, which is less than two-thirds as wide as the middle, and subtruncate with broadly rounded corners. Surface smooth and glossy; lines of increment inconspicuous, but sufficiently distinct near the margins; two or three of them however at irregular intervals sometimes projecting more strongly, indi-

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cating epochs in the growth of the shell. Marginal setæ of the mantle well developed, those on either side at the extremity longer than the rest, equalling in length one-third the width of the shell. There are two black spots on the margin of the mantle at the extremity. Peduncle in life three times as long as the shell, thick, (one-third width of the shell,) at its point of attachment, but rapidly tapering and becoming very slender and hyaline, with an opaque axis or central cord; extremity glutinous and covered with adhering sand. Length of the animal 3·5; length of shell, 0·92; width of shell at the middle 0·35; at extremity, 0·21; half way between middle and summit, 0·26 inch.

This animal was found imbedded in the weedy sand at low water mark, on the occasion of one of the extraordinarily low tides which occur at the equinox. It lives in a perpendicular position with its peduncle deeply penetrating the sand, and its shell scarcely projecting above the surface at its extremity. When drawn out and placed in a vessel of sea-water it showed its uneasiness by snake-like gyrations of the peduncle, which, far from being a simple stem for attachment, is a powerful muscular organ, filling the function of the foot of the Lamellibranchiata. Mr. Peale informs me that this part in the *Lingula anatina* forms a favorite article of food among the Fiji Islanders. Our Lingula appears to be not uncommon near Beaufort, as several specimens were found during a single retreat of the tide. It is interesting as being the first species* of this most ancient genus described from the Atlantic ocean. The other recent species, ten in number, are all inhabitants of the Pacific.

Euceramus, nov. gen. fam. *Porcellanidae.* Body subcylindrical. Carapax elongate-subrectangular, twice as long as broad; sides parallel. Front prominent, tridentate. Eyes minute, longitudinal, projecting a little beyond the orbits which are very incomplete, consisting only of the concave superior margin. Antennulæ placed immediately beneath the eyes; peduncle anteriorly bidentate. Antennæ large; mobile part nearly as long as the carapax and arising from the inner or superior side of the small coxal joint, thus being in contact with the eye at base. The outer maxillipeds are of the form usual in Porcellana, but the sternal piece to which they are attached is very large, nearly as long as broad, triangular in front, and truncate behind. Chelipeds small, subcylindrical, much shorter than the carapax; hands weak. Ambulatory feet subcylindrical; dactyli curved, setose, nearly as long as penult joint. Abdomen narrow, particularly in the males; appendages as in Porcellana.

This aberrant type should be referred to the Porcellanidea, notwithstanding its greatly elongated form, which would lead one to refer it at first sight to the Hippidæ or Raninidea.

Euceramus prælongus. Carapax regularly curved like a segment of a cylinder, above glabrous, and minutely striated transversely; striæ curved forward at the sides. Inter-orbital front one-third the width of the carapax, tridendate, teeth slender, pointed, median longest. Hands externally scabrous, or small-tuberculose and setose; fingers as long as the palm and not gaping. Ambulatory feet of the second and third pairs as long as the chelipeds; those of the first pair smaller. Length about three-fourths of an inch. Dredged on shelly ground in 4 to 8 fathoms.

* We understand that there is a specimen of Lingula from the coast of South Carolina in possession of Prof. Agassiz. Not having access to this specimen, we are unable to say whether it be identical with ours or not.

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IV. METEOROLOGY AND ASTRONOMY.

1. *Abstract of Meteorological Observations, made during the year 1859 —with the average of seven years—at Sacramento, Cal.*, lat. 38° 34′ 41″, long. 121° 29′ 44″; by THOMAS M. LOGAN, M.D.

1859.	January.	Feb'y.	March.	April.	May.	June.	July.	August.	Sept.	Oct.	Nov.	Dec.	Mean.	7 years average
Barometer.	In.	In.	In.	In.	In.	In.	In.	In.	In.	In.	In.	In.	In.	In.
Maxima,	30·491	30·332	30·385	30·371	30·171	30 211	30·066	30·080	30·126	30·270	30·526	30·868	30 266	30·243
Minima,	29·438	29·626	29·771	29·698	29·744	29·605	29·747	29·731	29·725	29 694	29·791	29·788	29 697	29·706
Mean,	30·211	30·026	30·151	30·004	29 976	29·836	29·917	29·892	29·927	29·943	30 040	30 167	30·008	30·004
Thermometer.	°	°	°	°	°	°	°	°	°	°	°	°	°	°
Maxima,	57·00	62·00	64·00	76·00	80·00	96·00	87·00	85·00	82 00	83·00	68·00	53·00	74·42	75·80
Minima,	34·00	39·00	40·00	40·00	53·00	61·00	60·00	58·00	56·00	49·00	42·00	34·00	47·17	46·70
Mean,	44·87	50·49	51·47	57·11	63·03	74·85	69·07	67·16	65 89	63·28	54·05	43 52	58·73	59·36
Thermometrograph.	°	°	°	°	°	°	°	°	°	°	°	°	°	°
Maxima,	58·00	63·00	66·00	78·00	84·00	102 00	92 00	89·00	85·00	87·00	70·00	54·00	77·33	77·99
Minima,	30·00	34·00	36·00	36·00	45·00	51·00	51·00	52·00	50·00	43·00	37·00	28·00	41·08	41·46
Range,	28·00	29·00	30·00	42·00	39·00	51·00	41·00	37·00	35 00	44·00	33 00	26 00	36·25	36·53
Force of Vapor.	In.	In	In.	In.	In.	In.	In.	In.	In.	In.	In.	In.	In.	In.
Maxima,	·362	·436	·410	·524	·522	·942	·677	·564	·581	·524	·529	·334	·534	·529
Minima,	·120	·173	·078	·133	·136	·296	·338	·350	·226	·186	·189	·087	·193	·197
Mean,	·231	·289	·257	·321	·363	·503	·469	·451	·410	·367	·347	·230	·353	·357
Relative Humidity.	Per ct.	Per ct.	Per ct.	Per ct.	Per ct.	Per ct.	Per ct.	Per ct.	Per ct.	Per ct.	Per ct.	Per ct.	Per ct.	Per ct.
Maxima,	93·00	94·00	92·00	87·00	82·00	82·00	77·00	82·00	84·00	85·00	94·00	92·00	87·00	87·81
Minima,	32·00	60 00	19·00	30·00	19·00	18·00	36 00	29·00	25·00	32·00	51·00	30·00	31·75	31·26
Mean,	78·05	78·73	68·32	68·90	64·81	59·51	66·95	68·68	65·61	64·41	82 32	80·30	70·55	69·95
Number of clear days,	7	3½	11	10½	14½	20½	22½	25½	16½	16½	4	10½	162½	176 7–15
No. cloudy and foggy d'ys	17	8½	6	14½	12½	9½	7½	5½	11½	14½	10	15½	132½	126 1–15
No. rainy days,	7	16	14	5	4	0	1	....	2	....	16	5	70	63 1–15
Quantity of clouds,	6·3	5·5	3·7	2·9	2·2	0·5	0·7	0·4	1·4	1·3	5·7	4·1	2·9	3·3
Quantity of rain and fog,	0·964	3·906	1·637	0·981	1·037	0·000	0·030	0·000	0·025	0 000	6·485	1·834	16·899	16·927
1st dys & 2d force N. wind	11½	2·3	4½	1·8	8½	3·2	5½	2·5	6	2·9	8	1·0	1	0·6	1½	0·6	7½	2·2	10½	1·4	7½	1·4	14½	1·4	81½	1·7	
" " " N.E. "	6	2·0	1	1·7	1	1·2	2½	2 0	1½	1·1	1½	0·4	0	0·0	½	0·3	0	0·0	2	1·2	1½	1·2	5	1·7	21½	1·1	
" " " E. "	1½	1·3	½	1·0	1	0·8	1	2·0	½	1·7	½	0·3	½	0·7	0	0·0	1½	0·5	2½	1·4	2	1·7	2	1·8	13½	1·1	
" " " S.E. "	6½	2·8	8	3·1	5	3·2	3½	1·9	4	1·8	2½	1·1	4½	2·2	5½	2·3	4	0·8	3½	1·6	8½	2·7	7	2·1	63	2·1	
" " " S. "	2	3·2	8½	3·1	4	2 6	10½	2·9	9½	2·6	11	2·2	18	2·5	15	2·5	7½	2·5	5½	2·0	4	3·0	1	2·0	95½	2·6	
" " " S.W. "	1½	2·5	3½	1·8	4½	3·2	3	3·0	3½	2·3	6½	1·7	4	2·3	4½	2·2	5½	2·1	2½	1·0	2	1·7	0	0 0	41	1·9	
" " " W. "	0	0·0	2	1·8	4	2·1	3	2·7	2½	1·5	21½	1·3	3	2·6	3	1·6	3	1·5	1	0·8	1½	1·0	½	0·7	26	1·4	
" " " N.W. "	2½	3·1	0	0·0	2½	3·6	1½	3 0	3½	2·6	2	0·7	½	0·7	1	0·7	1½	1·8	3½	3·3	3	2·1	1	2·3	22½	1·8	

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General Remark.—The climatic feature of predominant interest in California lies in the rains of winter, which, although they fell in the early part of the present season in ample abundance for agricultural purposes, nevertheless, in their subsequent diminution, confirm the opinion expressed by us in former remarks, that the cultivator of the earth cannot depend with any certainty upon them alone, but must be prepared to supply their deficiency, whenever it occurs, by irrigation—for which expedient no other country, perhaps, is better adapted, both as regards soil and climate, as well as facilities of commanding water.

2. *Daylight Meteor of Nov. 15th, 1859.*—Of this remarkable body Professor Loomis has already published an interesting account at p. 137 and 298 of this volume. The meteor being one of the most brilliant on record deserves the fullest possible investigation, and we are glad to find in the Journal of the Franklin Institute of Philadelphia, for March and April, a valuable paper thereon by Mr. Benj. V. Marsh of that city, giving an extensive series of observations which he has collected, together with his deductions therefrom.

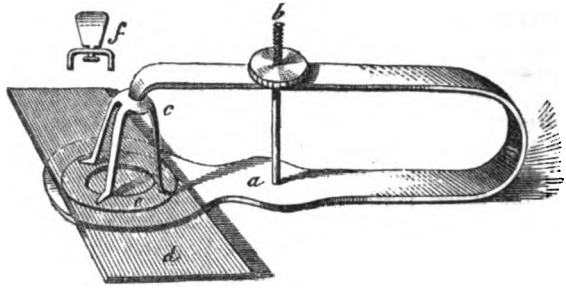
The meteor was seen in full sunshine, as a large ball of fire, from Salem, Mass., to Petersburg, Va. Its path was probably inclined to the vertical about 35°, and the direction of its motion was nearly west. Its velocity was very great, perhaps full 30 miles per second, and the meteor appears to have become luminous when more than 100 miles above the earth. During its brief passage of two or three seconds, it exploded several times, with reports which were loud and violent. These reports or detonations made two series, the whole occupying only half a second of time, the individual sounds being distinguishable because of the different distances they had to travel to reach the ear. The column of smoke resulting from the explosions was nearly a thousand feet in diameter, and its base was vertical about four miles north of Dennisville, N. J. The immense volume of smoke or substance of the meteor, dissipated by its excessive heat, shows that the body was of very considerable magnitude. The meteor's path would strike the earth near Hughesville, on the northwestern boundary of Cape May County, N. J., in which vicinity, or a few miles further west, it is probable that fragments may yet be found. Will nobody look for them?

V. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *Probable Origin of Flint Nodules in Chalk.*—Dr. G. C. Wallich, Surgeon in the Indian army, has published (in the Quart. Jour. Microscopical Sci., No. xxx, p. 36) an interesting paper on the siliceous organisms found in the digestive cavities of Salpæ—embracing under this head the whole molluscoid tribes that frequent the open sea in shoals and live upon the microscopic organisms it contains. These creatures are in their turn the food of whales. In the digestive cavities of the Salpæ the siliceous shields of Diatomaceæ, &c., are freed of all or nearly all their soft portions, and these minute organisms aggregate into masses which in the whales are further aggregated and in the form of coprolites fall in vast numbers upon the pulpy cretaceous strata of Foraminiferæ, &c., now known to form the bottom of the ocean in many places, imbedding themselves there as nodules similar to, certainly, if not identical with, the flint nodules in the Chalk.

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2. *New form of Compressor for use with the Microscope*, (in a letter to Prof. G. C. Schæffer).—*My Dear Sir*,—At your request I enclose a sketch of the little instrument that I use as a substitute for the ordinary compressor, in mounting objects for the microscope.



The frame *a*, is made from one piece of hard brass, possessing sufficient elasticity for the purpose,—held in place and adjusted by the milled nut on the screw *b*. This screw is firmly riveted in the frame at *a*, and plays freely in a slot at *b*. The swinging tripod, (also cut from one piece,) is loosely riveted to the bent end of the frame as shown in section at *f*, having sufficient play in the collar to adjust itself to any inequality in the slide or the cover.

The centre of the frame, at *c*, under the tripod, is pierced with a hole of sufficient size to see the object to be mounted, so that the pressure from the screw can be adjusted without injury to the object. The sketch shows a slide, *d*, and cover, *e*, in place.

For mounting objects dry, or for covering cells I find it useful as it enables me to hold the cover securely while I have access to its entire edge, and can turn it in every direction to apply the cement. For mounting objects in balsam which require very thin covers, say ·01 to ·005 of an inch, such as the silicious epidermis of plants and other test objects, I add to the pierced hole in the frame a circular shield of glass, a little less in diameter than the cover, and of the thickness of an ordinary slide, imbedding but a small portion of its thickness in the brass. Then, after placing the cover on the balsam, and spreading it by heat, I put the slide cover down in the frame, and apply a pressure to drive off the superfluous balsam. The raised surface of the glass shield, keeps the exuding balsam from the frame, and at the same time prevents any bulging of the thin glass at the centre. Dried in this way, under pressure, it is easily taken from the frame and cleaned. I have found it a very simple and satisfactory way of perfecting the mounting of difficult test objects.

Other modifications of this little instrument will readily sugest themselves to you. It is, to me, a great convenience to have a dozen of them at hand, of various sizes, as I can thus get my slides thoroughly seasoned under a perfectly uniform pressure—and I can have twenty of them made for the cost of one English compressor.

My friend, Mr. McAllister, Optician, 728 Chestnut St., Philadelphia, made them for me from a drawing, very neatly and accurately, at seven dollars and fifty cents per dozen ($7\frac{50}{100}$).

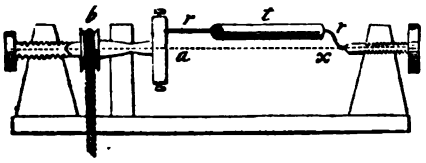
Very truly, your friend, S. Morton Clark.

Washington, Jan. 4th, 1860.

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3. *On Contraction of the Muscles, induced by Contact with Bodies in Vibration;* by O. N. Rood, Professor of Chemistry in Troy University.—Some time since, when grinding a slide for microscopic purposes, as the strip from time to time accidentally came into vibration, I experienced, in the hand holding it, a numbness, and, at times, an absolute inability to relax the grasp. It seemed as though an involuntary contraction of the muscles had been effected by the vibratory action.

For the examination of this matter, the apparatus seen in the woodcut



was devised: *b* is made to revolve at a rate of from 50 to 60 revolutions per second; *r r* is a rod of iron placed excentrically and so that the distance *r a* is equal to $\frac{1}{8}$th of an inch. To protect the hand from blisters, the brass tube *t* encloses the rod, fitting it very loosely.

When the hand is laid on this sheath, the rate of rotation being between 40 and 60 revolutions per second, a feeling of numbness is first perceived; the muscles involuntarily contract with considerable force, and the hand grasps the sheath tightly. As long as this rate of revolution is kept up, it is almost impossible, by an effort of the will, to relax the grasp, just as is the case with the electro-magnetic machine employed for medical purposes. The sensations, indeed, resemble those occasioned by the use of this apparatus, and usually extended as high as the elbows. At the termination of these experiments no particular inconvenience was experienced, although the sensations produced by the higher rates of vibration were painful.

Experiments were made on different parts of the arm and hand. The results obtained differed in degree rather than in kind.

The resemblance of the symptoms and sensations produced by electricity and mechanical vibration, is at least singular, and may eventually throw some light on the method in which electricity causes contractions in the muscles.

Troy University, Feb. 3d, 1860.

4. *Large Object-Glass.*—Messrs. Alvan Clark & Sons, of Boston, have completed on their own account, an object-glass with a focal distance of sixteen feet, and clear aperture of *twelve inches*. It has a nice defining power, and Mimas, the nearer satellite of Saturn, was seen with it Feb. 14th and March 2d and 4th.

5. *Boyden Premium.*—Uriah A. Boyden, Esq., of Boston, Mass., has deposited with the Franklin Institute, Philad., the sum of one thousand dollars, to be awarded as a Premium to "any resident of North America, who shall determine by experiment whether all rays of light, and other physical rays, are, or are not transmitted with the same velocity."

The conditions are given in an advertisement at the end of this Number.

6. *Geological Survey of California.*—We learn that a bill for securing the geological survey of California is now under consideration and if not so already, is likely soon to become a Law. So important an act must meet with the approval of every one interested in the material prosperity of the golden state: while science has much to expect every way from the proper discharge of such a commission.

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Book Notices.—

7. *Elements of Chemical Physics;* by Josiah P. Cooke, Jr., Erving Professor of Chemistry and Mineralogy in Harvard University. 8vo. pp. 739. Boston, Little, Brown and Company, 1860.—We cannot state the nature and scope of this important contribution to our scientific literature better than by quoting the following paragraphs from the Author's preface.

"The history of Chemistry as an exact science may be said to date from Lavoisier, who first used the balance in investigating chemical phenomena, and the progress of the science since his time has been owing, in a great measure, to the improvements which have been made in the processes of weighing and measuring small quantities of matter. These processes are now the chief instruments in the hands of the chemical investigator, and it is evidently essential that he should be familiar with the causes of error to which they are liable, and should be able to determine the degree of accuracy of which they are capable. All this, however, requires a theoretical knowledge of the principles which the processes involve, and a chemical investigator who, without it, relies on mere empirical rules, will be exposed to constant error. This volume is intended to furnish a full development of these principles, and it is hoped that it will serve to advance the study of chemistry in the colleges of this country. In order to adapt the work to the purposes of instruction, it has been prepared on a strictly inductive method throughout; and a student who has acquired an elementary knowledge of mathematics will be able to follow the course of reasoning without difficulty. So much of the subject-matter of mechanics has been given at the beginning of this volume as was necessary to secure this object; and for the same reason, each chapter is followed by a large number of problems, which are calculated, not only to test the knowledge of the student, but also to extend and apply the principles discussed in the work. Regarding a knowledge of methods and principles as the primary object in a course of scientific instruction, the author has developed several of the subjects to a greater extent than is usual in elementary works, solely for the purpose of illustrating the processes and the logic of physical research. Thus, the means of measuring temperature and the defects of the mercurial thermometer have been described at length, in order to show how rapidly the difficulties multiply when we attempt to push scientific observations beyond a limited degree of accuracy; so also the history of Mariotte's law has been given in detail, for the purpose of illustrating the nature of a physical law, and the limitations to which all laws are more or less liable; the condition of salts when in solution, and the nature of supersaturated solutions, have in like manner been fully discussed as examples of scientific theories; and, lastly, the method of representing physical phenomena by empirical formulas and curves, which are the preliminary substitutes for laws, has been illustrated in connection with Regnault's experiments on the tension of aqueous vapor."

After advising the student to study the details of science from original memoirs rather than from digests, and enumerating the chief sources from which he has drawn his facts and illustrations, the Author announces his design of following the present volume with two others.

"Although the present volume is a complete treatise in itself of the principles involved in the processes of weighing and measuring, it is also intended as the first volume of an extended work on the Philosophy of Chemistry. The arrangement of the chapters and sections has been adopted with this view, and the inductive method begun in this volume will be extended through the whole work. The second volume will treat of the theory of Light in its relation to Crystallography (including Mathematical Crystallography), and also of

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Electricity in its relations to Chemistry. The third and last volume will be on Stoichiometry and the principles of Chemical Classification. This volume is now in preparation, and will be published next."

We have read, or studied, the larger portion of Prof. Cooke's present volume, with care, and, are happy to add, with much satisfaction. It is a more elaborate and thorough discussion of the subjects on which it treats than has before appeared in any text-book. All the important propositions are mathematically demonstrated in a simple but thorough manner.

The volume demands and must receive exact and searching study, and any chemical teacher who intends to employ it as his class book will find it capable of the same treatment which he has been accustomed to regard as peculiar to mathematical text-books. The French units of weight and measure are employed exclusively, and a collection of tables (21 in number) is added for scientific reference and for the convenience of the student and teacher in solving the problems (420 in number) which are appended at the close of each principal subdivision of the subject. The whole subject matter of the volume is treated under five chapters, viz., I. Introduction, II. General Properties of Matter, III. The Three States of Matter, IV. Heat, and, V. Weighing and Measuring.

The mechanical execution of the work is beautiful, and the press seems to have been very carefully supervised.

One circumstance in connection with this work cannot fail to attract the attention of all teachers of Chemistry in American Colleges, namely, that a revision of our whole scientific curriculum is demanded in most of our higher Institutions in order to admit of the expansion demanded by the introduction of such a treatise as Cooke's Chemical Physics into the course of study. Such a change Prof. Cooke has been able to effect since his appointment at Cambridge, and now his chemical teachings fill a course of recitations and lectures commencing in the Sophomore year and covering two or three years. This is a great change in the policy of a college where this subject was formerly a by-word, and it offers every encouragement for efforts to secure a similar change in other leading colleges. For this reform, as for the high scientific character of his present work, Prof. Cooke will receive the hearty thanks and esteem of all teachers in this department of science.

8. *Smithsonian Miscellaneous Collections.*—Catalogue of the Publications of Societies and of the periodical works in the Library of the Smithsonian Institution, July 1, 1858. *Foreign works.* Washington, 1859, pp. 259, 8vo, with a Supplement.—The arrangement of this catalogue is geographical, commencing with Scandinavia, and ending in Europe with Great Britain. It is followed by an alphabetical Index to Learned Societies, and another of miscellaneous publications, chiefly Journals. The Smithsonian Library possesses, as appears from this list, the Transactions in full or in part of 501 Institutions and Learned Societies, and series, more or less complete, of 254 Scientific Journals exclusively foreign. The domestic publications form the subject of another catalogue. It is a most valuable aid to the student in ferreting out the often enigmatical references constantly found in books of science, and for determining the probability of being able to verify such references by a visit to Washington, or by correspondence. Only those who have undertaken researches can appreciate the value of such an aid.

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9. *The New American Cyclopædia.* Vol. IX. New York, D. Appleton and Co., 1860.—This volume contains some fifty articles on zoölogical subjects. Some of them are of considerable length and show marks of much care and labor. The sections on Herpetology and Ichthyology are especially to be commended for the plan on which they are written. Instead of entering into trivial details the author has confined himself to giving the ways in which the best authors have arranged the divisions of Reptiles and of Fishes. The subject is thus properly kept within scientific limits. The more popular treatment is reserved for particular animals or for the minor zoölogical divisions. Thus, there is a very interesting and acceptable article on the Horse, and another on Insects. Some of the definitions are not quite precise, and terms are, in one or two instances, somewhat loosely applied: *e. g.*, speaking of the nervous system of an insect as a "brain and spinal cord." But these are of little consequence compared with the general good judgment and accuracy displayed. Nor is it too much to say that these contributions are far better than those commonly met with in Cyclopædias. It is understood that the zoölogical articles are from the pen of Dr. Kneeland, Secretary of the Boston Society of Natural History.

Other articles on scientific topics in this volume worthy of particular mention are Iron and the Iron manufacture by Mr. Hodge, and that on Isomerism by Dr. F. H. Storer, who is also the author of an excellent article on Chemistry in a former volume.

Among the living scientists of whom biographies are given we notice Herschel, T. S. Hunt, and C. T. Jackson.

10. *Cavendish Socy's. Ed. of Gmelin's Hand Book of Chemistry*, vol. XIII.—This is vol. VII of Organic Chemistry embracing organic compounds containing sixteen and eighteen atoms of carbon. The Cavendish Society announce that they will give only one volume for 1857, '58, and '59. This action of the Society is not well calculated to content their subscribers or to invite new recruits. But we can well imagine that the Council have found Gmelin a heavy load and that they are anxious to close it before undertaking any new publication; meanwhile let us hope that "Rose's Analytical Chemistry," long since undertaken, will not be unnecessarily delayed.

11. *Lieber: Geology of South Carolina.* Report IV. 1859. 8vo, pp. 194. Columbia, S. C. Apr. 2, 1859.—We have already announced (this vol., p. 287) the unfortunate discontinuance of the South Carolina Survey from an unwise withholding of the requisite appropriation of money. Dr. Leiber in this Report sums up his results and presents some general discussions on Metamorphism, a subject certainly of much importance in both a scientific and economic view in South Carolina. The Geognostic maps of Anderson and Abbeville districts and the "Industrial map" of the State, are very neatly printed in colors by Colton of New York.

Some of the most important subjects of this Report have already been laid before our readers, *e. g.*, the evidence of a change of level on the coast of South Carolina and the mineralogical details.

12. *Fundamental Ideas of Mechanics and Experimental Data*, by A. Morin. Revised, translated and reduced to English Measure, by Joseph Bennett, Civil Engineer. New York, Appleton and Co., 1860.—This

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work is designed to illustrate the practical application of mechanics to the construction of machinery, draught vehicles, ship building, &c., and contains experimental data of great value in calculating the resistance to be overcome in the form of friction, vibration and wear and tear of machinery and other structures. The work is of great value to practical engineers and machinists and we rejoice to see it presented to the public in an English dress.

13. *Gangstudien, oder Beiträge zur Kenntniss der Erzgänge*, Dritter Band, drittes und viertes Heft.—The continuation of this valuable work, edited by Prof. Cotta and Herrmann Müller, contains a very important paper by Müller, on the relation between *mineral springs* and *mineral veins* in Northern Bohemia and Saxony; also, an extended article on *itacolumite*, its associates, and the metal bearing characters of the same, by Oscar M. Leiber, late State Geologist of South Carolina.

Ferrel, Wm., A M.: The motions of fluids and solids relative to the Earth's surface; comprising applications to the winds and currents of the Ocean. From the Math. Monthly, Vols. I. and II. New York, Ivison and Phinney. 1860. pp. 72. [A very important discussion an abstract of which, prepared by the Author, we shall present at an early day].

Wilkes, Capt. Chas, U.S.N.: On the circulation of the Oceans. Phila., 1859. pp. 24, 8vo.

Hennessy, Henry, F.R.S., M.R.I.A.: A Discourse on the Study of Science and its relations to individuals and to society. 2d edition, Dublin, 1859. 8vo., pp. 54.

Kirkby, J. W.: On Permian Entomostraca from the shell limestone of Durham, with notes by T. R. Jones, F.G.S. XI plates. 8vo, pp. 17.

Parker, W. K. and T. R. Jones: On the Nomenclature of the Foraminifera, (from Ann. and Mag. Nat. Hist., Nov., 1859). 8vo, pp. 17.

Bellardi Luigi: Saggio di Ditterologia Messicana. Parte 1ª, Torino Della Stamperia Reale. 1859. 4te, pp. 77. *Tav* II.

Logan, Thos. M., M.D.: Report on the Medical Topography and Epidemics of California. Phila., 1860. pp. 58, with maps.

Stevenson, D. and T.: Answer to Sir David Brewster's Reply to Messrs. Stevenson, on Sir D. B.'s memorial to the Treasury. Wm. Blackwood and Sons. 1859. 8vo, pp. 16.

Forget, A.M., M.D., etc.: Dental Anomalies and their influence upon the production of diseases of the maxillary bones. (Prize essay), translated from the French. Jones and White. Philadelphia, 1860, pp. 34, with VI plates.

Durand, Elias: Memoir of the late Thomas Nuttall, (from the proceedings of the Am. Phil. Soc.). 8vo, pp. 19. 1860, Phila.

Royal Society, London, Proceedings, Nos. 35, 36 and 37, from May 26, 1859, to January 12, 1860. 8vo, pp. 1–234.

Hall, James: Contributions to the Palæontology of New York, being some of the results of investigations made during the years 1855–1858, being part of the 12th Annual Report of the Regents of the University of the State of New York. March 15th, 1859. Albany, 8vo, pp. 110.

Harris, Elijah P.: The Chemical Constitution and Chronological Arrangement of Meteorites (an inaugural dissertation). Göttingen, University Press. 1859. pp. 131.

Isaac Lea: Observations on the genus Unio, &c., Vol. 7, Part II, with 25 plates, 4to, read before the Acad. Nat. Science. Philadelphia, 1859.

Haidinger (Wm.): Ansprache gehalten am Schlusse des Ersten decenniums der K. K. Geolg. Reichsanstalt in Wein, 22 Nov., 1859. 8vo, pp. 38. 3 plates.

(The same). *Die Rutilkrystalle von* Graves Mount in Georgia. 5 January, 1860.

J. B. Schneider und Ernst Hartig: Untersuchungen über dir Heizkraft der Steinkohlen sachsens: Leipzig, W. Engelmann. 1860. 4to, pp. 509. Taf. IV. This is the third and concluding part of the great government research on the Coals of Saxony. Dr. Geinitz prepared the volume on their Geology and Prof. W. Stein

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is the author of the Chemical volume. The three parts form by far the most complete research on this important subject hitherto published.

Dr. J. Lamont: Magnetische unterschungen in Nord-Deutschland, Belgien, Holland, Dänemark, München. 1859, 4to, pp. xlv, Tab ix.

(The same.) Untersuchungen über die Richtung und Stärke des Erdmagnetismus, &c., München, 1858, 4to, pp. cxv.

Ludwig Seidel: Die Theorie der Lichterscheisung des Saturn München, 1859, 4to, pp. 102, 1 chart.

Von Martius (Dr. Carl): Eine Rede zur feifer des Akademischen Saccularfestes am, 29 März, 1859. München. 4to pp. 74.

Feistmantel (Carl): Die Porphyre im Silurgebirge von Mittelböhmen, 4to, pp. 42. 2 Tafeln.

Daguerin; P. A.: Traité de Physique, tome III, 8vo, pp. 1007, avec Table alphabetique et analytique. Toulouse et Paris, 1860. This volume contains the close of Electricity, and Optics.

Proceedings Boston Soc. Nat. Hist., 1859.—p. 135. On the recent eruption of Mauna Loa; *H. M. Lyman.*—p. 138, Descriptions of new shells brought home by the North Pacific Expedition; *A. A. Gould*—p. 142, List of Plants collected by E. Samuels in California; *A. Gray.*—p. 145, List of Plants collected by J. Xantus at Fort Tejon (5 new species); *A. Gray.*—p. 150, Natural History, etc., of Mohawk, Herkimer Co., N. Y.; *Whittemore.*—p. 153, On vibrating dams; *W. Edwards.*—p. 155, Experiments with the Japan Wax-plant, Rhus succedaneum; *E. S. Rand.*—p. 159, Habits of Esox fasciatus; *G. Curtis.*—p. 160. Green feldspar from Mt. Desert, Me.; *C. T. Jackson.*—p. 161, New shells of the North Pacific Expedition (of the genera Patella, Acmæa, Scutellina, Emarginula, Rimula, Chiton, and Dentalium); *A. A. Gould*—p. 166, On reversed bivalve shells; *L. Agassiz.*—p. 167, On Odontaspis griseus; *D. H. Storer*—On the shedding of the antlers of the American red deer; *J. Wyman.*—p. 168, On the Carboniferous rocks of Nova Scotia and the United States; *W. B. Rogers*—p. 174, On larvæ vomited from the human stomach, and on a new Gordius, G. trifurcatus; *White.*—p. 175. On a great meteorite recently discovered in Oregon; *Evans* and *Jackson.*—p 176, On the geology of the shores of the Bay of Fundy; *W. B. Rogers.*—p. 176, On the meteor of August 11th, 1859; *D. A. Wells.*—p. 179. On the transformations of the Cecidomyiæ, a posthumous paper by the late *Dr. T. W. Harris.*—p. 184. On Diatomaceæ prepared by Mr. Samuels; *Stodder.*—p. 185, On Cicindela Hentzii; *Harris.*—p. 189. Synonymy of three North American butterflies; *Harris.*—p. 191, On the Arrangement of Zoological Museums; *L. Agassiz.*—p. 192, Experiments on frogs; *B. J. Jeffries.*—p. 193, Descriptions of new Ophiuridæ belonging to the Smithsonian Institution and to the Museum at Cambridge; *T. Lyman.*—1860. p. 208, On flattened pebbles found in the conglomerate of Vermont; *E. Hitchcock, Jr.*—p. 209, On a mineral substance found in the medullary cavity of trees at the Sandwich Islands by Dr. Winslow; *A. A. Hayes.*—p. 211, On the Gorilla; *J. Wyman.*—p. 213, Chronological Index to the Entomological papers of T. W. Harris, M.D.—p. 222, On the lower jaw and a tooth of Physeter macrocephalus; *J. C. White.* s.

Proceedings Philadelphia Acad. Nat. Sci., 1860.—p. 3, Number of species of American Unionidæ; *I. Lea*—Mosasaurus bones from New Jersey; *J. H. Slack.*—p. 4, Contributions to North American Lepidopterology, No. 3; *B. Clemens.*—p. 15, Appendix to paper on new genera and species of North American Tipulidæ with short palpi, etc.; *R. Osten Sacken.*—p. 17, Catalogue of the Mollusks of Mohawk, N. Y.; *J. Lewis.*—p. 19, Notes on the nomenclature of North American fishes; *T. Gill.*—p. 22, Prodromus descriptionis animalium evertebratorum, etc. Pars VIII, Crustacea Macrura; *W. Stimpson.*—p. 47, Mexican Humming birds; *R. M. de Oca.*—p. 49, Geographical distribution of Coleoptera; *J. L. Leconte.*—Geographical distribution of the Helices of North America; *W. G. Binney.*—p. 51, Reversed Unionidæ; *I. Lea.*—p. 55, Illustrations of fossils; *Conrad* and *Gabb.*—p. 55, Descriptions of new species of American fluviatile gasteropods; *J. G. Anthony.*—p. 72, Supplement to Catalogue of Venomous Serpents; *E. D. Cope.*—p. 74, Catalogue of the Calamarinæ in the Museum of the Academy; *E. D. Cope.*—p. 80, Descriptions of new species of Cyrena and Corbicula; *T. Prime.*—Mexican Humming-birds; *R. M. de Oca.* s.

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NOTICE.

At the close of Volume XXX, Nov. 1860, an alphabetical and analytical Index will be prepared to the TEN Volumes of this Journal completed by that number. This Index will be issued to all subscribers with the number for January, 1861, and may also be ordered separately.

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THE

AMERICAN JOURNAL

OF

SCIENCE AND ARTS.

LAWRENCE SCIENTIFIC SCHOOL CHEMICAL DEPARTMENT

PROFESSORS B. SILLIMAN, B. SILLIMAN, JR.,

AND

JAMES D. DANA,

IN CONNECTION WITH

PROF. ASA GRAY, OF CAMBRIDGE,
PROF. LOUIS AGASSIZ, OF CAMBRIDGE,
DR. WOLCOTT GIBBS, OF NEW YORK.

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ERRATA.

P. 162, l. 16 from bottom, for "many" *read* "some."
" 167, l. 23 from top, for "best," *read* "test."
" 174, l. 18 from top, for "point," *read* "part."

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THE

AMERICAN

JOURNAL OF SCIENCE AND ARTS.

[SECOND SERIES.]

ART. I.—*On the Origin of Species;* by THEOPHILUS PARSONS, Dane Professor of Law in Harvard University, Cambridge, Massachusetts.

IT has frequently occurred in the history of science that some startling theory, which, when first announced, was regarded as the antagonist of received opinions, and became at once the subject of earnest hostility as well as unqualified approbation, has, after much discussion been importantly qualified and modified, and thus reconciled with views which it seemed to contradict; and when thus shorn of its excess and moderated in its demands, has been generally adopted as an important addition to knowledge. It may yet be so with Mr. Darwin's views.

His theory, stated very briefly, is, that all organisms tend to reproduce themselves in a geometrical ratio, and with such exuberance of life, that each one would speedily fill the earth, if not repressed by constant and powerful causes of destruction. Hence but a very small proportion of seeds or ova which are impregnated are able to mature and reproduce. Therefore there must be a competition, or as he phrases it, a "struggle for life," among all these impregnated germs of life; and if one in a hundred only lives there must be a reason why that one lives rather than the ninety and nine which perish. This reason must again be frequently, or at least sometimes, that it had some advantage in this "struggle for life," by a structural or functional difference. That is, it varied from its kindred, in such wise, that it was somewhat easier for it to live, to grow, to mature, and to reproduce, than for them. This difference or variation it must, as a general rule, impart to its offspring. When it be-

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came established, the same law of struggle, of advantage, of life, and of death, would operate upon this new and improved variety, and would cause another and a farther improvement. As this law is universal, and must always have operated upon all organisms from the beginning, not only are varieties established in this way, but so likewise varieties become species, species become genera, and so also orders, classes, families are formed, and thus finally we may suppose that all the organisms of the earth, living and extinct, animal and vegetable, have proceeded from the simplest original form of life.

While much interested in Darwin's work and in the discussions and controversies to which it has given rise, it occurred to me to consider whether one of the limitations which he seems to have imposed upon himself, was necessary. He assumes, and reasons exclusively upon the assumption, that the successive changes by which these great results have been brought about have always been minute and slow, and have only become sufficient to reach their consummation, by an indefinite accumulation of effects, through the indefinite periods of time which geology affords them. It seemed to me that this assumption was quite unnecessary, and therefore unphilosophical; and supposing that these changes may sometimes have been much greater, I then inquired what would be the effect of this supposition upon the general theory, that the succession of organized being has from the beginning been produced by generative development. This paper is intended to suggest—and only to suggest—some of the results to which I have come. Upon the question whether I have not departed so widely from the theory of Darwin, that I have no right to use his name, I have nothing to say. I wish only that these suggestions may pass for what they are worth whatever that may be.

To say that it is the tendency of all organisms to reproduce their like, but with some difference, would be merely to utter a truism, for there is almost or quite always some family resemblance between offspring of the same parents, and always so much of difference that no two of the offspring are ever undistinguishable from each other. We may say, however, that one certain law of this difference, or variation, is this; that while a slight difference is universal, great difference is less common, and the greater the difference the more rare it is, and therefore the less to be expected in any given instance. The question then arises, how far this difference may go; or to say the same thing in other words, what limit is there to the possible immediate variation of offspring from their parents and kindred?

The law of variation is itself variable; and while we have little knowledge of the causes of variation, we have none whatever of the limits to which it may be carried. Indeed, if we assume that there must be some limit to the possible extent of

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variation, we may infer that it must be a very broad one, from the instances of extreme monstrosity which science has recorded. Let us say, then, that we will assume that there may be as much variation or aberration as these records prove that there has been, and no more.

Perhaps abnormality always seems to us a mischief, and by monstrosity we always understand aberration in a wrong direction; and facts would justify the inference that extreme aberration is usually a degradation. But we have no sufficient reason for saying that this is a *law;* or, in other words for asserting, that there can never be monstrosity in a right direction; or in yet other words, that the aberrance can never be an improvement and a help. As this seems to me an important principle I restate it. We know that it is the tendency of all organisms to reproduce their kind, but with some difference. We know, for all the improvement in our domestic animals proves it, that this difference may be improvement. We know that this difference may be carried to an enormous extent, as a mischief, because the records of monstrosity prove it; and we do not know that this difference may not be carried to an equal extent in the opposite direction of improvement.

My position therefore is precisely this. It is always possible that offspring may be born, differing as much from their parents and kindred in the way of gain, of advantage, and of improvement, as we know that offspring have differed in the way of loss, of hindrance and of degradation; and therefore when I speak of extreme aberration I shall mean by it variation carried to this extent.

Admitting this principle as possible, let us proceed with it to consider what may be called the system of Agassiz; using his name only because he has given to it great development and full illustration.

Take first his assertion that there must have been in each geological age many new creatures; say if you please an hundred or a thousand, and consider this as proved and admitted. Still it leaves wholly untouched the question how these new creatures were created. And be the answer what it may, that answer so far as it is only an answer to this question, leaves the assertion of Agassiz untouched. But if we bring to the question, how were these creatures created? the possibility of aberrant variation of offspring in the direction of improvement, we bring to it one answer. For example: suppose the time to have come when there is to be a new creation, and it is to be a dog, or rather two dogs, which will be the parents of all dogs. How shall they be created? We may say of this either of five things. One is, that we do not know, and never can know, and had better not inquire. This does not seem any answer. A second is, that they will be created "by chance." This also seems to me no answer, because chance is a word only and not a thing. A

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third is, that they will be created at once and out of nothing, by the absolute will of a creator. This answer does not satisfy me much better. The fourth is, that they will be so created by absolute fiat, out of a proper quantity of earth and water, with the necessary chemical elements in due proportion, which had been summoned to meet together in a proper place by the will of the Creator for that purpose. But this answer does not recommend itself to my reason much more than the others. The fifth is, he will be created by some influence of variation acting upon the ovum (before or at conception or during its uterine nutriment) of some animal nearest akin—a wolf, a fox, a hyena, or a jackal; and the brood will come forth puppies and grow up dogs to produce dogs. Now the question is not whether this last answer offers a probability *per se*, but whether it is not after all less improbable than either of the other suppositions; less unphilosophical than either of the other answers, and therefore to be accepted on that ground: and I may say in passing, that if the present favorite theory for accounting for the diversities of our domestic dogs, by referring them to four different origins, be adopted, we may then conjecture that each of the four animals above named brought forth its own puppies, to be the progenitors of their respective families.

Let this doctrine of the new creation of new species, by generative development through variation be accepted, and we have Darwin's theory of the origin of species by successive generation; and instead of opposing the theory of Agassiz, it confirms it; because it adopts and reasserts the principle of new creations, and offers some explanation of the way in which they were made.

Let us glance—and only glance—at some facts in geology and zoology, to see what would be the effect of this principle; and I shall carefully limit myself to the most general suggestions, on a topic which would fill more than a volume.

At the beginning of the fossil records of life, in the Silurian formation, we find trilobites of various forms; and recently a Limulus or something akin to a Limulus has been found there. There are other Crustacea; but with these two only, is it not possible to account for all the Crustacea which have ever existed or now exist, without overstepping the rationally possible limits of extreme variation in offspring, simply by arranging those which we already know in a chain of affinity?

But how shall we get to the vertebrates? These same trilobites ran up through all the palæozoic rocks, through the Silurian, the Old Red-sandstone, the Carboniferous and the Devonian, and are lost at last in the Permian. Near their end, when they are already thinning out, we have, in the old red-sandstone formation, the "buckler head,"—or, to use the Greek name given by Agassiz, the Cephalaspis. And we have also the fossil flying fish, or using again the Greek name, Pterichthys. The first of these was long regarded as a trilobite of the genus Asaphus, until Agassiz

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at length determined it to be a fish. Of the second, Murchison says in a letter to Miller, "if not fishes they approach more closely to crustaceans than to any other class. I conceive, however, that Agassiz will pronounce them to be fishes, which together with the curious genus Cephalaspis *form the connecting links* between crustaceans and fishes." Now, is it too much to infer from these facts, and always within the reasonable limits of generative aberration, that either of these animals, if a crustacean was so nearly a fish that some of its ova may have become fishes; or if itself a fish was so nearly a crustacean, that it may have been born from the ovum of a crustacean? We may add indeed, that the Eurypterus, now called a crustacean, was regarded at one time, by Agassiz, as a fish.

If fishes may thus have begun to be, and we may suppose that, having begun, they could be so arranged by their affinity and gradual difference as to account in this way for the successive new creation of their kinds, we may then pass to the question of reptiles.

Here also we have Lyell's Dendrerpeton, Owen's Placodus, and the Archegosaurus of von Meyer, all of which were held, and somewhat firmly held, by the highest living authority on this point—Agassiz again—as fishes; and all of them after further and final investigation, have been lifted out of the water by the same strong hand, and placed upon dry land, as reptiles. I know the explanation of this; but does not the fact itself suggest irresistibly that we have here what Murchison calls "connecting links." Links, that is to say, through which, by generative variation, the fish passed into the reptile, and so the family of reptiles began. So too, possibly, the Pterichthys, or fossil flying fish, the Pterodactyle, or huge winged fossil reptile, may suggest the possibility of a similar origin for birds.

As to that difference between vegetables and animals, which some have regarded as the greatest difficulty, I would say only what every one who owns or uses a microscope knows, that the line which separates the protophyta from the protozoa is constantly changing and always uncertain; and that if the organisms which lie along this line, should have offspring which are certainly vegetable, or those which are certainly animal, in neither case would the offspring differ much from the parent.

Nor let it be said that the geological records exhibit numerous instances where a race which succeeds another, does not come into existence until a certain period after the kindred race from whom they might have come has utterly perished. It is not quite so. On the contrary, in most cases, the great classes of animals lap over, as in the instances given, of crustaceous trilobites and the fish found with them, and again the fish and the earliest reptiles, in a way which has always suggested, of itself, this idea of succession by generative reproduction. There are eminent naturalists who read in the records of geology the plain declaration

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that there have been some—perhaps many—cataclysmic destructions of whole orders of being, followed by periods characterized by the absence of organic life. If this were *proved*, there must have been not only many new *creations*, but many new *beginnings* of organic life. It must be remembered however, that the geologic record is assuredly not yet wholly unrolled; and that we are not sure that we read aright all that is seen. I have some doubts whether there be an instance in which such an interval of absolute nothingness unquestionably occurs; or one, even in the present state of our knowledge, in which among the races passing away there are not found, and far within the limits of extreme aberration, some who *may* have been their offspring, and the parents of succeeding races.

But I must forbear following these suggestions further. The difficulty of admitting the transformation is, I know, great; and still greater difficulties must be encountered in other parts of this supposed chain of reproduction. A very great one to my own mind arises from those beds below the Silurian, which, on the one hand, are wholly free from traces of life, and on the other, from evidence of destructive alteration by heat. They seem to me to lead strongly to the conclusion of Murchison and others, that the earth had only then become cool enough to make life possible, and consequently that life must have begun there; and there certainly we find it already very various. But, not to insist that with farther knowledge, wider discovery of "connecting links," or transitional forms, and better examination, all these difficulties may be materially lessened, I say at once that I should accept them all unhesitatingly, rather than the notion that the first horse, or dog, or eagle, or whale, flashed into being out of nothingness, or out of a mass of inorganic elements which had been drawn together in due proportion for that purpose.

This last supposition is inevitable if we reject the first.

The one thing I would be understood to assert, is, that science must now *elect* between two hypotheses, which together fill the whole ground, and cannot *both* be rejected. One is, that the animals and vegetables of the world have been formed, by absolute fiat, out of a mass of inorganic materials. The other, that they have come into being successively, by generative production, of some kind and in some way. When Milton tells us that

> * * * * The earth obeyed, and straight
> Opening her fertile womb, teemed at a birth
> Innumerous living creatures, perfect forms
> Limbed and full grown. Out of the ground arose
> As from his lair, the wild beast where he dwells
> In forest wilds, in thicket, brake or den.
> The grassy clods now calved; now half appeared
> The tawny lion, pawing to set free
> His hinder parts: * * * *

he adopts and adorns the first hypothesis; but while Milton was a great poet, he was not so great a zoologist.

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I do not now assert that no creature *can* be made out of nothing, or out of the dust of the earth, nor do I speak of the first beginning of creation; nor of anything but the existing and extinct floræ and faunæ. In reference to the various species of these, I say only that this is the last conclusion which we should adopt, and only when driven to it. Perhaps I may illustrate my meaning thus. If a pair of undescribed mammals, about as large, we will say, as a fox, with young or preparing for them, were found to-day in some district in England which has been thoroughly explored, and of which the fauna and flora were perfectly well known, and these animals differed in some specific essentials from any known animal, there would be a vast amount of speculation about their origin. One writer would say that they had escaped from a menagerie or from some ship; another that they had always been overlooked and undescribed until now; another that they were hybrids, and there would be much discussion as to what animals could have produced them, like that which Gilbert White tells of about the bird which he thought a cross between a pheasant and a hen. There would be no limit to the extent or variety of the discussion,—excepting this. No naturalist would, I think, explain their appearance at that time and place, by supposing that they had been made out of nothing, or out of the dust, suddenly, where they were found. If any one ventured upon this hypothesis, I do not believe that it would be generally adopted. I do but apply the same way of thinking to past times. When the new species appears first in the geological strata, I say that its creation from nothing or from the dust should not be held, until all other possibilities of production are exhausted and rejected. For creation from nothing is just as possible now as it ever was; and we have no reason for saying that it would not be as natural *now*, as likely to occur, and as worthy of admission and belief.

What do we gain by the use, in this connection, of the word miracle in the sense of an exceptional interference by omnipotence? When one of the wheels of Babbage's calculating machine turns up its numbers in a certain unbroken series for a million of times, and then a new element is suddenly introduced, and an old one goes out, this apparently disturbing thing is just as much a part of the machine and its operation as all the rest. The illustration fails so far as this. Babbage calculates his machine and sets it going, and leaves its working to the natural laws which he finds in operation. God never leaves his machine, for if he did it would instantly perish, because it is always his present activity which gives force and efficacy to the laws by which He works.

But what shall we do with that other principle of Agassiz, that all this successive production or creation of new creatures has happened by the will of a creating God; or, to use his own

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phrase, that each new creature has come into being by the fiat of the Almighty? What I do with it, is to accept it readily and entirely. For when the voice of God issues the fiat and says let this thing be, is it not as perfectly obeyed although that thing comes into being by generative development, as if it sprang forth from nothing or from the dust?

And again what shall we do with the principle of Agassiz, that in all these new creatures there is no chance and nothing arbitrary, but a coherence and coördination of parts, and a unity of purpose and of place, which prove the whole to be the work of one directing mind and one causative power. Again I answer, admit this also freely and gladly; thankful for every argument and illustration which enforce it. For what is there in the supposition that God has his own laws of divine order, and operates through these laws, and by the means which He has provided, (no matter how universal these laws or how far back the chain of influences or causes extends,) to prevent our recognition of Him and of his wisdom in his works.

But what shall we do on the other hand with Darwin's "struggle for life," and consequent "natural selection," which plays so great a part in his theory? Again I say, if farther investigation renders it probable, as I think it will, admit this also with perfect readiness to play whatever part sufficient evidence may assign to it, be that more or less. The fact *to some extent* is obvious and certain. And may not God act as well through this "struggle for life" as through any other of his laws? Must it be regarded as a blot, an imperfection, which he could not help, and bears with as he may? If we regard it as an instrument, by means of which he works out universal, inevitable, and never ending improvement, incorporating this law with the nature and essence of every thing that lives, or can live, may we not see in this also, at once his infinite love and his infinite wisdom?

Then as to hybridism. Darwin admits the vast preponderance of authority against the continued fertility of hybrids, but still thinks that there are some qualifications. Even since his book was published, Isidore St. Hilaire, who has made hybridism a special study, has published a work in which he asserts, and goes far to prove, that hybrids are sometimes at least just as fertile as their parents. Out of this uncertainty, let us draw one certainty; and it is that nothing is certainly known about it. And also one probability—that offspring may differ from their parents and brethren so very much that there can be no sexual intercourse between them. They may differ less and then there may be intercourse but it will not be productive. They may differ still less, and it may be productive, but the offspring will not reproduce. Still less and they will reproduce, but only for a few steps. Still less, and they will be as fertile as their parents or brethren. Scientific men may give to these degrees of differ-

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ence the names of classes, of genera, or species, or what else they will. For here I will venture to remark that much of the criticism and discussion to which Darwin's work has given rise, both in England and in this country, seems to me verbal only. That is, it relates not to the origin and nature of certain existences, but to the language we should employ in speaking of them. What do we gain in real knowledge, when we insist that the word "species" *must* mean this or that, when it *may* mean anything, and very few persons use it in the same sense, or in any definite sense. And as to the question of difference or identity, do we know enough about it to be very positive on any point, except our ignorance? For how many years has the Tertiary formation been arranged into four classes—the Eocene with its one shell in twenty-five now living, the Miocene one in six, the Pleiocene one in two and a half, and the Pleistocene nine out every ten. DesHayes, a great man, has devoted himself to their examination, and has reasserted this with the most emphatic distinctness and the most abundant illustration: and Agassiz now comes and declares it to be all a great mistake. He doubts whether any one shell of the 4 per cent, the 17, the 40, or the 90, has ever been looked upon alive by man.

Far be it from me to undertake to decide between such men. But again let me draw one conclusion, which seems certain; and it is that there is no sure, unerring, and unmistakeable test of specific identity or difference.

If we admit with the qualification and in the way above stated the theory of the production of all things by generative development, and the active operation of this principle of the "struggle for life," and admit also Agassiz's requirement of new creations, and of the orderly succession and coördination of these, we have a theory composed of elements which certainly do not now oppose and destroy each other, but coëxist in harmony, and in mutual support and illustration.

How far shall we carry it? Not to the creation of all things from one beginning, unless farther investigations should remove the immense difficulties which this theory must now encounter, and sustain its probability. But let not the investigation be clouded, obstructed and defeated by the assertion that any theory which calls into being all existing and extinct organisms by some method of successive generative development, cannot be true, and must needs be false and dangerous.

The great difficulty to most minds would be, after all, that which relates to man himself. Man, from a monad! Yet let it not be forgotten, that this is the natural history of every man that has ever been born of woman. At first a nucleated cell, (call it a monad if you like,) not distinguishable from other nucleated cells, which, by segmentation, gives rise to that germinal membrane, from the outer portion of which are formed the or-

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gans of animal life, and from the inner those of organic (or vegetative) life; and then, in its uterine development, exhibiting successively resemblances, more or less close, to the lower animals; the human embryo, for example, having, about the twenty-fifth day, the branchial openings and elongated body of a fish, at a later period the imperfect limbs of a seal, and still later the bent limbs of a quadruped. These, and many analogous particulars in the history of the human embryo, make this one of the most inexplicable and yet suggestive wonders of existence. One might well imagine that the "monad" retraces his footsteps along that immeasurable pathway from primeval being, and as it repeats, records them.

While all this is nothing like *proof* that man is also a product of this law of generative devlopment through variation, it may have some tendency to lead the mind in that direction. And how much there is elsewhere in the metamorphoses of nature, to exert upon the mind a similar influence. Tell one who is eating a ripe peach, and after enjoying all the pulp, breaks his teeth against the stone, and being curiously inclined opens that, and finds the solid meat, and opens that again, and puts the infolded plumule under a lens, and sees there the promise of a future tree,—tell him that skin, and stone, and seed and plumule, all are but *changed peach leaves*, will he not be at least as much surprised as if you carried him to a menagerie, and pointing to a hyena, said to him, there stands the father of the "yaller dog" of New England? *

But this notion of man being born from an animal stands in

* I allude, of course, to the January number of the Atlantic Monthly, wherein this strange animal is presented with that wonderful power of word-painting, which is a true daguerreotyping by the sunlight of genius.

But I write this note rather to refer to an article in the North American Review for July, 1857, in which Dr. Holmes, before the controversy about "Darwinism" began, treats many of the topics to which it has given rise, and exhibits his own views of an ever *immanent* God.

No one can admit more cordially than I do, the principle which has been recently so much considered, that God must have had at and from the very beginning of his action, laws, to which he and his universe have always, and, I am willing to say, necessarily conformed. So too, I admit, as cordially, that other principle, that all science, philosophy and reason, lead concurrently to the conclusion, that the "Causa causans" must be always and incessantly a present cause, as *present* at one period of duration as at another, and always directly and universally operative. But why regard these principles as antagonistic? To me they seem not only harmonious, but complementary, and necessary each to the other. If I believe that God is ever present, active and operative, it is because I believe that the laws of order which arise from his own divine nature, permit and require this. If I believe that these laws exist, that he has ever conformed to them and must ever do so, it is because I believe that they are the eternal instruments of his ever active love and wisdom. In the words in which Dr. Holmes sums up the whole matter at the close of his article in the N. A. Review, "whatever part may be assigned to the physical forces in the production and phenomena of life, all being is not the less one perpetual miracle, in which the Infinite Creator, acting through what we often call secondary causes, is himself the moving principle of the universe he first framed and never ceases to sustain."

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the way of positive revelation! In my own mind it does not. I look upon the Bible as the word of God: but I do not believe that the first chapters of Genesis teach or were ever intended to teach natural scientific truth; nor does this denial lessen my reverence for what I consider as the moral, spiritual, and religious truth which I believe they do teach directly, or under the form of parable and symbol. And upon the question of the original and physical creation of man, I think that we know no more and no less, and are at equal liberty to think, to argue, and to conclude, as if these chapters had never been written. To me, they do not say one word about it.

But does not this notion stand in utter opposition to all religious belief? Again, I can only say that in my own mind it does not. I believe, most unreservedly and undoubtingly, that man is superior, not in kind only but in degree, to all animals, and is immortal, which they are not. But this belief would not be either shaken or troubled, if science should, upon evidence discovered hereafter, teach, that the Gorilla, which Owen says is most like to man, or the Chimpanzee, which Professor Wyman, with better reason, places higher,—if either or both had given birth, when the fit time had come, to a babe, whose brain and nervous system, with all the residue of its frame, were so organized that the breath of life, of spiritual and immortal life, could be breathed into him, and bear with it all the attributes of human nature,—all those attributes which divide, as by an unfathomable abyss, the man from the beasts that perish, and lift him infinitely above them. At present, science possesses not only no facts which would lead to this as a certain conclusion, but none which would declare it to be a probability. But neither has it sufficient reason for asserting it to be an impossibility. Nor, does it seem to me, that religion would receive a blow, if science should be led by additional discovery and more thorough investigation, to go not only thus far, but so much farther, as to account for the various kinds of men by asserting that the brown oran-outang that lives among the brown Malays was their progenitor; the black gorilla the father of the black races, among which he is still found; other simiæ the parents of other human families; and some one fairer than the rest, the remote ancestor of the Circassians, whose superiority over their progenitors was so great that they had rooted him out from the earth!

But let us consider the general relation of this hypothesis to religion. I am perfectly willing to confess that the theory propounded by Darwin, as it rests upon excessively minute changes, and those produced by what he calls "accident," (of which word, however, and of his use of it, he offers much explanation) seemed to me to have a tendency to obscure the thought of providential causation and government; and that I was first led to reason out, as well as I could, the probability and effect of more salient changes in the offspring, by its appearing to open the door

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to this thought somewhat more widely. But aside from this, and indeed from any reference to this or any special question or theory, may it not be well to remember, that natural science belongs, mainly at least, to the intelligence of man, and to his outer life, while religion belongs, mainly again, to his affections, his motives and his inner life. Hence, entirely different faculties and functions of our common nature are brought into exercise in reference to science, from those which are invoked by religion. It is a good and wholesome thing for a man to become religious because he chooses to be so, and loves to be so; and it is good for him to compel himself to make this choice. He cannot indeed become religious on any other ground or in any other way. And therefore Divine Providence has mercifully guarded him, not only from the external compulsion, which, as all men see, cannot reach the heart, but from the compulsion of his own intelligence, which might be equally injurious.

In investigating the claims of science, he must call upon his intellect to look sharply at the facts, the logic, the arguments and the conclusions; and this is all, or nearly all. But he must choose and hold his faith, not by means which logic disdains and denies, but by asking of logic to do all that it can do and and the best that it can do, as the instrument of something higher than itself, which can take up and complete the work which mere logic must leave unfinished.

How easily could God have written his word and his truth in fire upon the sky, and in gold upon every leaf or stone, if all he had desired was the intellectual advancement of man. We may infer from his course of providence, that he desires this, only as a means to an end; and as an instrument of that moral and affectional improvement, which must be man's own coöperative work. Therefore it is, that religion never has been, and I think never will be fortified by the demonstrations which belong to ascertained science; and hence it is also that no science, and no mere truth has ever yet been suffered to arise on the world, and none I think ever will be, that does not leave man free to be irreligious if he will; although all true science offers him much to feed upon and to rejoice in, if he loves to look upon the truth he learns as aliment for his religion.

To every creature is given a tendency and a capacity to seek and find and appropriate that food which agrees with its own nature. When a willow tree sends a root far in one direction to a ditch where it may drink its fill, and a neighboring grape vine sends its root as far in an opposite direction and finds a heap of buried bones, we have but the operation of the same law, by virtue of which if ten men read a book, it may be to them ten books; for each will read the same words, and then translate them in his own way. It is an old saying, that what one brings home from foreign travel, depends upon what he car-

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ries with him. So it is in the journeyings of the mind. Let that go where it will it carries itself, and uses itself as the organ for giving form and effect to all that it receives.

The poet may say that the undevout astronomer is mad; but astronomy, and every science cultivated among men, has those who are devoted to it with the most faithful assiduity, and who extend its borders and enlighten its dark places, and who are, nevertheless, utter unbelievers as to God and religion; and find in their science support for their unbelief. To minister to religion is the highest, the consummating work of science; but science cannot render this service where there is no religion to accept it. So will it be with the theory of the creation of all things by successive generative and variant production, if it be established in any form whatever.

This man will read it to whom the idea of God is an offense and a pain. His unbelief holds him in subjection; and when he reads any book, or studies any subject, he reads with clouded eye and mind all that favors religious truth, but brightens at once when he gets a fact or an argument for his unbelief, and dwells on that as a choice morsel. He will study this new theory, and find in it new evidence that God is a mere superfluity; and he will say exultingly, now we have proof that the laws of the world and their own necessity are all that a truly rational mind can ask. And he will deny, or forget, that there is no possible conception which so imperatively demands a lawgiver, as law; and none which so requires a cause to set it in action, as an active necessity.

Another man who loves to believe that God forms and fills and *is* the universe, and that there is no other God, will find here abundant support for his opinion, and will rejoice in the evidence this theory affords of the universality of law and the connection of all things by gradation into unity. And he will forget, or will not know, that all this implies design, and purpose, and will, and therefore personality.

And a third man will see in this theory new proof of the eternal working of the personal God in whom he believes. He will rejoice at the evidence it offers that God loves to bless every entity of his creation by using it as his own instrument and as the means for farther creation; that preservation is continual creation; and that he forever puts forth the same power, born of the same love and guided by the same wisdom, that in the beginning laid the foundation of the universe deep in that infinite which no plummet of human imagination ever can sound. To such a mind it will be a new proof, that from God's own nature, there came forth laws of order, in which, through which, and by which, he has ever worked, from a beginning, which when we try to think of it, recedes faster than thought can follow.

Cambridge, May, 1860.

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ART. II.—*Notes on the Habits of the Common Cane,* (Arundinaria macrosperma, *Michx.*); by HUGH M. NEISLER, Corresponding Member of the Essex Institute, Salem, Mass.

THE common cane, as it springs from the seed, bears no slight resemblance to some of our coarser and more reedy Panicums; rising, during the season of its growth, according to the more or less favorable circumstances of its locality, from the height of a few inches to that of several feet, forming a straight unbranched culm, with a bud at every node. The culm itself completes its entire growth the first season, and though it endure for years, even to the time of its flowering and fruiting, never afterwards increases in height, only becoming tougher and stronger by the gradual deposition of siliceous matter on its surface, and ligneous matter in its substance.

The second year, circumstances being favorable, its lateral buds are developed—forming slender, upright branches—whose growth in length likewise terminates with the season, and which rarely grow much higher than the summit of the culm.

The third year, the buds of these lateral branches are developed, and they, in their turn, form other lateral branches with their buds, and thus the plant continues to grow until it flowers and fruits, until its existence is terminated by accident, or it becomes so much crowded that there is no longer room for its growth in this manner. However, the buds of one season do not invariably put forth the next; but, if circumstances are unfavorable to their development, they may remain dormant for a time, and then put forth or eventually perish.

The first year, besides the growth already described, the plant throws off one or more subterranean culms, popularly termed "chain roots," differing from the others only in being white, with very short internodes, and these clothed with imperfect sheaths, or naked, the sheaths becoming obsolete or, as is more commonly the case, reduced to a circle of rootlets around the node. From the buds of these subterranean culms springs the second year's growth of cane. This is subject to the same laws and grows in the same manner as that of the first year, likewise throwing off its subterranean culms, from the buds of which springs the cane of the next season, and so on it proceeds for years, new cane coming up each successive season, until it is so crowded there is no room for more to grow. The buds of these subterranean culms may, like the others, be developed the year after they are formed, or remain dormant until circumstances favor their growth.

The cane of each succeeding season becomes stouter and taller than that of the preceding; and as the cane of one year is overtopped and shaded by that which grows up the next, it dies out gradually and gives place to the larger cane of some future season; hence arises the great uniformity in size, observable generally in the plants of a cane-brake.

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It may not be amiss, in this place, to observe, that the cane is described by authors as "branching towards the summit"; this is correct as applied to plants growing in a crowded cane-brake, where the development of the lateral buds is prevented; but where it is uncrowded and free to grow, it presents itself, clothed from the ground upwards, with numerous erect, closely appressed branches, as the seedling plant is described near the beginning of this article.

After the growth of a number of years, and when the cane has reached as great a size as the circumstances of the locality will admit of, it springs up only in such places as may yet be unoccupied, until the whole brake becomes as thickly crowded as possible. After arriving at this point, its growth altogether ceases, excepting from the few buds that may be formed at the summit, and may chance to be freely exposed to the light and air. In this condition it will remain for a series of years, until the time of flowering arrives. If, however, whilst in this condition, any or all of it by chance be destroyed during the winter, its place will be supplied by cane of equal size the following season, from the buds of the subterranean culms; but if an accident of this kind happens during the growing season, it never grows up again, the whole plant dying; though if any escape the accident, the extension of their "chain roots" may gradually fill up the vacancies left by the plants that have perished.

The size which the cane ultimately attains is as various as the soil in which it grows. In places peculiarly unfavorable, it may not be higher than six inches; in the "Piney-woods" swamps, in my immediate vicinity, ten or twelve feet is about the average height. On Flint river it is common to find it as high as twenty-five and thirty feet; whilst I have seen occasional specimens cut from the swamps of the Uchee in Alabama, which, though not actually measured, I should judge, could not have fallen far short of forty feet. In the same cane-brake, too, if covering ground which presents striking diversities of soil, it will be found to vary greatly in size.

We have above remarked that when cane reaches its full size, it remains almost unchanged until the time of flowering: how long this may be I have not the means of determining. It is evidently many years. A piece of land, many acres in extent, was once pointed out to me in the low grounds of Flint river, by a gentleman residing in the vicinity, who informed me that on his removal to that country, it was covered by a dense growth of large and full-grown cane, and that it flowered and fruited fifteen years thereafter. If in this instance we suppose it to have required ten years to bring it to the condition in which he first observed it, its time of flowering would be brought to twenty-five years—the age at which, in this region, it is pop-

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ularly supposed to flower and fruit. This, however, is but a supposition.

When the flowering once begins, it takes place in every plant in the cane-brake at the same time, whatever may be its age, its puberty depending not on the actual age of the cane itself, but upon the time that has elapsed since the germination of the seed from which it has either directly or indirectly sprung. When the time comes, it flowers, whether it has grown up from the ground ten or twenty or more years before, or whether it has yet to complete the first season's growth. In the spring of 1857 I found a small field from which the cane had been cut the preceding winter. The husbandman had been so careless in clearing out the young cane that had come up amongst his corn, that great numbers of them were left growing about. They were generally about three-fourths of an inch in diameter, but only some five feet high, and still so tender and soft that they could be easily crushed between the finger and thumb to within one and two joints of the ground; yet every plant was in full bloom. There the time of flowering had overtaken it when little more than two months old. It is, however, no uncommon thing to find individual plants in bloom, sometimes several years before the general flowering commences. This is, however, only an exception, an occasional precocity brought about by some extraordinary cause.

With the ripening of the seed ends the life of the plant. It then perishes root and branch, as entirely as an annual grass; and if the same ground be ever occupied by cane again, it must be produced anew from seed. Authors, I judge, have been unaware of this fact, from their describing the plant as fruiting at stated periods, or at more or less distant intervals; and this error perhaps may have originated from their noting the times of the flowering of the plant, as it occurred in different localities; or, if observed in the same place, the intervals have been so long between the times of flowering, that the fact of its having been in the meantime reproduced from seed, may have escaped recollection.

The question here presents itself, Whether there are two species or varieties, or but one? Within the comparatively narrow limits to which my own researches have been confined, I have had opportunities of gathering the flowers of both large and small canes; but specimens have yet to be collected that present a difference. As to the question in general, if the reader bears in mind, that the cane in the earlier years after the germination of the seed is much smaller than that which follows at a more distant day, he will readily see, that if both should chance to survive for years, they might appear so different as to be regarded as distinct varieties, if not species, especially as the flowers are rarely procurable to prove their identity. The same may be said of the differences produced by differences in the character of the soil in which they grow.

Taylor Co., Geo., 1860.

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ART. III.—*Experiments on the forms of Elongated Projectiles;* by OGDEN N. ROOD, Prof. of Chemistry in the Troy University.

ALTHOUGH during the last few years great attention has been paid in Europe to the improvement of rifle projectiles, yet much obscurity seems still to prevail on this subject, as is sufficiently evinced, by the different and opposite courses pursued by the several governments; the variety in the shapes of the balls employed at the present time being almost as great as that in the patterns furnished by a kaleidoscope.

The inquirer in this department meets with very *general* answers, and is too often told in substance, that the accuracy of performance is directly proportional to the excellence of the ball's model; it being in the meanwhile by no means particularly apparent in what such excellence consists.

An explanation is found in the fact, that there is perhaps no other field of investigation, in which so great a number of experiments is essential to the establishment of single and even isolated facts—wind and weather, slight changes of temperature in the tools employed, or in the form and fit of the projectiles, as well as other and more obscure causes, all largely influencing the results, contribute and combine vastly to complicate what at first glance might seem a moderately simple problem.

A series of experiments lately instituted by me, had for their object the examination of a few of the more obvious considerations relative to the accurate flight of elongated projectiles, such as length, the form of the base, and other points.

The rifles employed, were, with the exception of No. 5, made by Nelson Lewis, of Troy, N. Y., and were of the model sometimes called "Kentucky," or more properly, "Improved American."*

Our marksmen know the quality of Mr. Lewis's work, although it may be well to state, once for all, that rifles of this model as manufactured by him, and a few other makers in the United States, hold much the same relation to the English or Continental arms that the Oertling balance does to the scales of the apothecary; a fact which has not escaped the notice of the author of the article on Gun Making, in the Encyclopædia Britannica, 1856, vol. viii, p. 101, where, after a description of this rifle as made by James, of Utica, N. Y., and an account of its performance at 220 yds., he concludes: "The whole of the ten shots would have gone into a small sized playing-card. A feat of this kind is probably unparalleled in Great Britain, and it may draw

* For some account of these rifles see an excellent article published in the Atlantic Monthly for October, 1859.

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the attention of our own makers to the propriety of diminishing the calibre, and increasing the speed of the ball."

Dimensions of the rifles employed.

	Length of Barrel.	Weight.	Calibre.	Twist of grooves gaining, ending in one turn in inches.
		lbs. oz.		
No. 1.	30·5 in.	16 0	0·46 in.	
No. 2.	31·0 "	9 0	0·34 "	42·35
No. 3.	30·4 "	7 4	0·42 "	
No. 4.	12·0 "	2 12	0·36 "	47·37
No. 5.	5·5 "	0 8	0·20 "	12·85

They were all provided with the sights known as the "Globe and Bead," as well as with set locks. Nos. 1 and 2 were supplied with the false or loading muzzle, and all with the exception of No. 5 had "guide-starters," which ensured the accurate placing of the ball in the barrel. Linen patches were in every case employed, not greased, but moistened, it being now pretty well understood that the latter afford the more accurate practice. As a general thing, after each shot the barrel was wiped out with a slightly moistened rag, dampness being guarded against by a repetition of the process with one that was dry.

Length of the Projectile.—As has been intimated, great variety of opinion seems to exist as to the proportion which the length of the ball should bear to its diameter measured at the base, balls being used in Europe whose length ranges from 1·36 to 3 diameters. The advantages of length are generally known and there are writers who advocate progress in this direction, imagining naturally enough that a ball modeled after the lines of a ship would experience less resistance in passing through the air than its shorter and blunter rival. Some months ago, filled with this idea, I constructed a ball of the model shown in Fig. 1, sharp both at bow and stern, and having of course its centre of gravity farther forward than is usually the case with solid projectiles. Its penetrating power at short distances was great, but its complete and total failure otherwise, resulting from the impossibility of communicating a sufficiently high rotary motion, led to an examination of the rates at which it is necessary for balls of different lengths to revolve in order to secure accuracy of flight.

1.



Diam. ·36 in. Length 1·27. 3·53 Diameters long. Weight 222 grs.

Now it is evident that if the velocity with which any ball leaves the rifle-barrel be known, and likewise the twist of the grooves, the rate of revolution of such ball per second is easily found; thus, if the initial velocity should be 1000 feet per second and the twist one

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turn in 5 feet, it is plain that at the moment of leaving the muzzle, the ball will be revolving at the rate of 200 times per second.

To ascertain then the initial velocity, I constructed two ballistic pendulums, similar to those used by Robins. The total weight of the larger pendulum was 21·75 lb., and it happened, by chance, that 120 of its small swings were performed in two minutes; consequently its centre of oscillation was distant 39·13 inches from its axis of suspension: the centre of the pendulum was 39·7 inches from the axis, while the centre of gravity was 37·4 inches distant from the same point: therefore we have,

$$\left.\begin{matrix} 39{\cdot}7 : 39{\cdot}139 \\ 39{\cdot}7 : 37{\cdot}4 \end{matrix}\right\} :: 21{\cdot}75 : 20{\cdot}2 \text{ lb.}$$

or the pendulum resisted the impact of a body in motion as though its weight had been only 20·2 lb. The axis was made of a bar of steel, two sides of which were ground to an edge. Instead of the ribbon used by Robins to measure the chords, a narrow strip of paper was employed, which was fastened to one side of the pendulum on a line with its centre, that is 39·7 inches from the axis.

The smaller pendulum was similar in construction: its weight was 7833 grs., it made 93 oscillations in one minute, consequently its centre of oscillation was 16·29 inches distant from the axis; the centre of the pendulum and its centre of gravity were distant from the axis 16·6 and 15·2 inches; it resisted therefore as though its weight had been 7038·4 grains.

For barrel No. 4 I succeeded in constructing, after many trials a ball or picket, on the American model, Fig. 2, whose flight was accurate up to 500 yards; its dimensions are given below. This ball was fired with different initial velocities at targets of *pasteboard*, placed at certain distances from the rifle; an examination of the holes made, at once indicated whether the rate of rotation sufficed to compel it to fly truly and point foremost; for if this were not the case, the picket of course, made *oval* holes or struck the target sideways.

2.



Ball No. 4. 1·67 Diam. long.

Ball No. 4.

Length ·62 in. Diameter ·37 in. Length in diameters, 1·67. Weight 113·34 grains.

Initial velocity.	No. revolutions per sec	Distance.	Point foremost.	Oval.	Sidewise.
520 ft.	131·7	20 ft.	1	4	6
520 "		36 "		2	2
691 "	175	20 "	3	1	1
691 "		36 "		2	2
691 "		165 "			8
849 "	215	75 "	1	1	2
849 "		165 "	2	1	2
965 "	244	165 "	8	6	3
1128 "	285·8	1500 "	Flight accurate.		

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It will be seen from these experiments that a ball of the above model, and 1·67 diameters in length, must make 280 revolutions per second to ensure its accurate flight, when discharged with an initial velocity of 1100 feet per second; what rate of revolution would be necessary to sustain it point foremost, were the initial velocity different, is another question, and one which will presently be noticed.

Let us examine the case of a ball slightly *longer* than the above. Rifle No. 2 was now employed, its ball was slightly blunter than No. 4; it had often made excellent practice at 220 and 500 yards.

Ball No. 2.

1·77 diameters in length. Weight 105·01 grains.

Initial Velocity.	No. Rev.	Distance.	Point foremost.	Sidewise.
1051	298	165 ft.		all
1199	340	660 "	all	

By a comparison with the last table, it will be seen that this slight increase of the length has carried with it the necessity of 50 additional revolutions per second. The lowest initial velocity I have ever actually employed in this rifle for target practice was 1774 feet, the highest 1917 feet, so that the number of revolutions made by the ball in actual practice was either 503 or 543.

Balls 2 and 2·5 diameters in length.

A longer ball for barrel No. 4 was now constructed: its weight was 141·5 grains, diameter ·36 in., length ·74, so that it was in fact 2·05 diameters in length. The following results were then obtained:

Ball 2·05 diameters long.

Initial Velocity.	No. of Rev.	Distance.	
1063	269	18 ft. 42 ft.	all sidewise.

The above shows that with this model 269 revolutions are altogether insufficient, if the initial velocity be as high as 1063 ft.

The length of the ball used in the Swiss federal rifle is 1·0039 inches, its diameter 0·41 inches, or it is 2·44 diameters in length, weight 257 grains, weight of the charge 62 grains. Making use of the British empirical formula $V = 1600\sqrt{\frac{ap}{w}}$, we have the initial velocity $= 1600\sqrt{\frac{3{\cdot}62}{257}} = 1361$ ft., and as the twist is one turn in three feet the ball leaves the barrel making 453 revolutions per second.

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The initial velocity in Jacobs' rifle, judging from his statements, must be about 986 feet: as the twist is one turn in 28 inches, the ball makes 422 revolutions per second.

Now why not, it is often asked, project either of these balls with an initial velocity of 1600 or 1700 feet per second? Simply because the above rates of revolution then become wholly insufficient. To test this point, I constructed for barrel No. 2 a ball weighing 136 grains and only 2·08 diameters in length, consequently not requiring as rapid a rotary motion as the Swiss ball.

Ball 2·08 *diameters in length.*

Initial Velocity.	No. of Rev.	Distance.	
1715 ft.	486	165 ft.	All sidewise.

It would probably require a rate of at least 600 revolutions per second, and the Swiss ball being again much longer would hardly fail to need a rate of from 700 to 800.

The twist of Whitworth's rifle (ball 3 diameters long) makes one turn in 20 inches: its initial velocity is not given, though it is understood to be greater than that of the Enfield: *if* it be as high as 1600 feet, the rate of revolution of the projectile will be 960 times in a second; if 1700 feet, 1020!

The above mentioned experiment will perhaps suffice to establish the fact that for any given projectile, the necessary rate of revolution increases rapidly with the augmentation of the initial velocity.

The inability to use larger charges of powder prevented me from pursuing the investigation farther in this direction, but it still remained possible to invert the order of proceeding and to examine whether at very *low* velocities the necessary rates of revolution for projectiles 2 and 2½ diameters long rapidly decreased. An elongated ball for barrel No. 5 was now constructed: its length was ·38 inches, diameter ·2, *i. e.* 1·9 diameters in length; weight 20·8 grains. The initial velocity was determined with the smaller pendulum and found to be 232·6 feet: the velocity was determined also from the time of its flight: a distance of 15·41 feet gave it 233: the average of 14 shots at 44·2 feet made it 216.

Ball 1·9 *diameters in length, weight* 20·8 *grains.*

Initial velocity.	No. of Rev.	Distance.	
232	216	75 ft.	Flight accurate.

Another ball was now constructed for this barrel; the results were as follows:

Ball 2·3 *diameters in length, weight* 26·5 *grains.*

Initial velocity.	No. of Rev.	Distance.	
202	188	75 ft.	Flight accurate.

A third ball, 3 diameters in length, was finally made for the same

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barrel: it was cast with inclined bands on it, extending for two-thirds of its length and corresponding to the grooves of the barrel:

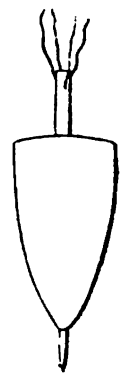
Ball 3 diameters in length, weight 42·3 grains.

Initial velocity.	No. of Rev.	Distance.	
157·9 ft.	147·5	30 ft.	Flight accurate.

Patches were not used with these three balls.

As a termination to this portion of these experiments, I constructed the model seen in Fig. 3, 2 diameters in length and provided with a steel axis: it was caused to rotate by the thread on which it was suspended: small discs of paper divided into green and red sectors were attached, it having been previously ascertained by a revolving machine, with what rate of revolution each disc assumed a neutral tint. The model being made to rotate was allowed to fall from a height and it was found that six revolutions per second enabled it to remain point foremost in a fall of five feet, while 20 per second sufficed for a fall of 45 feet.

Fig. 3.



The only question that now remains is, if 700 or 800 revolutions per second are necessary for the flight of a projectile 2½ diameters in length at a high velocity, why should this rate of rotation not be communicated to it? The answer is found in the practical difficulties; the friction and recoil become enormous, the act of discharge twists the rifle over sidewise and out of the line of sight. In the American Rifle, with a gaining twist, where the friction, twisting &c., are reduced to a minimum, it has been found that the maximum number of revolutions per second that can properly be communicated to a ball is from 500 to 550, and many will be ready to deny that even this rate can be employed with other than very heavy rifles of very small calibre without greatly impairing the accuracy. According to Chapman, a rifle having a regular twist of only one turn in four feet, calibre 80 round balls to the pound, charge of powder 2 inches to the bore, [initial velocity about 1680 feet, rate of rotation 420 times in a second] will, "when fired, twist over sidewise in spite of all you can do, and also kick or recoil very severely.* Surely if such recoil and twisting can be felt and seen, the tendency of the bullets to scatter and strike the target in a circle and not in a straight line is easily accounted for."

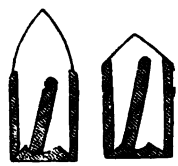
* It appears that European projectiles moving with a low velocity are subject in the longer ranges to a horizontal drift independent of the wind. This is attributed to the action of the grooves in the rifle, and has been found to correspond with their direction. No such deviation was observed by Gen. Jacobs (page 27 of Rifle Practice,) and he attributes it to the heavy recoil of the rifles used. As far as I know, it has never been observed in the American rifle where the recoil is very small, and the *trajectory very much flattened*, though it is the custom of our markamen on calm days to practise at targets placed indifferently at 110 and 500 yards.

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But where accuracy is aimed at, it is highly desirable not to employ a rate of revolution such as is able barely to cause the projectile to fly point foremost. A very notable excess above this quantity is essential for good practice.

One point remains to be noticed, viz.: the disfiguration which balls longer than 1·9 diameters suffer, when discharged with more than a very moderate velocity, in their passage through the necessarily strongly inclined grooves of the rifle. Gen. Jacobs states (page 23 Rifle Practice) that his projectile 2½ diameters in length performed admirably with a charge equal to one-fifteenth of the weight of the ball, but adds that he found, "A very slight increase of the charge caused the lead to change its shape under the pressure of the gunpowder so that a ball of the shape of Fig. 4 came out like Fig. 5. The lead was found totally incapable of preserving any resemblance to its original form under the pressure of a charge of powder equal to one-eighth the weight of the ball" From this dilemma the General escaped by making the forward part of the ball out of zinc; Whitworth provides for this difficulty by the use of pewter, which again necessitates a polygonal bore and a regular twist.

4 and 5.



Finally, during the last few years many of our best gunsmiths and marksmen have endeavored to increase slightly the length of the balls of their rifles above the standard dimensions given below, but in spite of the heavy charges used (3½ inches to the bore sometimes,) it does not appear that their efforts have been crowned with success, every fifth or sixth shot straying off some inches, or even, in the worst cases, striking the target sidewise at 220 yards.

It is perhaps hardly necessary to dwell farther on this matter of length, for while all the foregoing tends to explain why it has been found necessary in Europe to use low initial velocities with these *highly* elongated projectiles, it at the same time gives us little reason to expect that they will ever be able to compete with their shorter and more manageable rivals.

Below are the dimensions and weights of a number of balls belonging to first-class American rifles, each of which has been distinguished for its accurate practice:

Diameter of the Base.	Length.	Length in Diameters.	Weight in Grains
0·36 in.	0·62 in.	1·72	112·7
0·35 "	0·62 "	1·77	105·
0·46 "	0·82 "	1·78	243·6
0·47 "	0·87 "	1·83	249·
0·45 "	0·86 "	1·91	265·8
0·44 "	0·85 "	1·93	225·

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But assuming the length of the projectile to be within the above standard American limits, it is still possible to increase its weight by adding matter about the point, which of course becomes blunter. A ball was constructed of the same length as No. 4, but weighing 121·1 instead of 113·6 grains, the $7\frac{5}{8}$ additional grains of lead being disposed about its point. As has before been seen, the old ball with an initial velocity of 1128 feet, and with 286 revolutions, had been accurate in its flight, but with the same charge of powder the new ball flew sidewise: the charge was increased; still with an initial velocity of 1443 feet, and making 365·5 revolutions per second, the new ball flew untruly, making oval holes in the target at 165 feet.

At present it seems to be the opinion of our best marksmen that a projectile constructed with a very moderately heavy point affords proportionately better practice at 500 than at 220 yards.

The Curve of the Projectile.—The bounding curve of the projectile has been largely experimented on in the United States, and has been slowly perfected by a class of men whose stock of mathematical knowledge was, and is, exceedingly limited: the results of their experimental labors are on this very account the more interesting.

I selected balls belonging to three excellent and well-tried rifles (by different makers) and placed them under the compound microscope, which was arranged so as to magnify only about five diameters: a camera lucida being attached, by its help the magnified images were transferred to paper. The curves were examined and found to be portions of ellipses, the main difference being in the points of the balls, which were slightly sharper. In two of the balls, with this exception, the coincidence was almost perfect: the curve of the first formed a portion of an ellipse whose diameters were in the proportion of 5 to 0·9; in the second the diameters were in the proportion of 5 to 1, in the third of 5 to 0·86. The agreement was pretty close with the third ball, but its bearings had been made more nearly parallel with its axis than was the case in the others, consequently here it differed slightly from an ellipse.

Form of the base of the Projectile.—The base of most balls now used is either flat, slightly convex as in the American model, or more or less deeply concave as in all those constructed on the expansion principle. It is generally admitted that balls having a flat base and moving with a velocity greater than 1200 feet per second, leave behind them a perfect vacuum, which, in addition to other sources of resistance, retards their progress by a pressure in front of 15 pounds to the square inch.

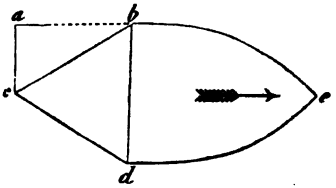
As the air is assumed to rush into a vacuum at the rate of 1150 feet per second, it now becomes a question whether the

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base of the ball could be so shaped that in its passage through the air this vacuum might in great part be prevented by air rushing in at a certain, but lower rate than 1150 feet. It is evident that the lower this rate is made, the more do we subtract from the pressure in front, that is, diminish the resistance to the flight.

Suppose the base constructed like *b c d*, Fig. 6, and the velocity of translation 1100 feet per second, then it is evident that while *a b* is the measure of the velocity with which the point *b* moves from *a* to *b*, viz: 1100 feet, *a c* will be the measure of the velocity with which the air must rush in to prevent a vacuum, and by construction *a c* is only 635 feet.

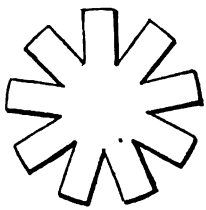
6.



$bd = cd$
$bd \times 1.9 = ce$

We should be led to expect that a projectile of this shape would experience less resistance than one provided with a flat base, until the velocity of translation was increased so that *a c* became equal to 1150 feet, which in the model used by me takes place of course when *a b* or the velocity of translation is 1992 feet per second. Every effort was made to give these double cones as great a length as possible; many models were tried, and the length was slowly diminished until the new projectiles flew truly; finally, moulds for two balls of this model were perfected for rifles No. 2 and No. 4, and after some slight alterations the new double cones rivaled in accuracy of performance the pickets with flat bases. To accomplish this, it was found necessary to cut the patch as seen in Fig. 7, which ensured accurate loading.

7.



Patch for double cones.

Below are the results of a number of experiments on the time of flight of the old flat-ended balls and of the new double cones, together with their average velocities from 110 to 500 yards.

Single Cones. (Weight 113·34 grains.)

Rifle No. 4. Initial Velocity 1128 ft.

Distance.	Time of Flight.	Average Velocity.
110 yards,	·3328 sec.	993 feet.
220 "	·6947 "	950 "
500 "	1·885 "	795 "

Double Cones. (Weight 109 grains.)

Rifle No. 4. Initial Velocity 1138 ft.

Distance.	Time of Flight.	Average Velocity.
110 yards,	·32795 sec.	1006 feet.
220 "	·71725 "	920 "
500 "	2·05680 "	729 "

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Single Cones. (Weight 105·01 grains.)

Rifle No. 2. Initial Velocity 1917 ft.

Distance.	Time of Flight.	Average Velocity.
220 yards,	·4175	1580 feet.
500 "	1·3080	1146 "

Double Cones. (Weight 100·8 grains.)

Rifle No. 2. Initial Velocity 1867 ft.

Distance.	Time of Flight.	Average Velocity.
220 yards,	·4552	1449 feet.
500 "	1·4120	1062 "

It will be seen from these tables that the flight of the double is *slower* than that of the single cone, particularly at high velocities: this disproportion diminishes as we use lower velocities, but even here the new seems to possess no advantage over the old form—the sharpness of the stern not at all compensating for the necessary bluntness of the bows. It occured to me that this inferiority in the flight of the new balls might be owing to a disfiguration in their shape produced by the explosion of the powder: a number of them were accordingly fired into a bank of snow distant 500 yards: when dug out they were found to be as perfect in their proportions as at the time of loading.

It would seem that if the conical has any advantage over the flat base, it is only at velocities as low as 400 or 500 feet per second. These experiments also tend to show that the vacuum behind a projectile does not suddenly cease at a velocity of 1150 feet, but that its diminution is very gradual: they farther indicate to some extent the importance of making the forward part of the ball *sharp* when a high velocity is desired. Indeed, the air struck by the forward part of the ball seems to be thrown from it with such force, at high velocities, that a vacuum is produced behind it, whatever its form may be; and the vacuum is more complete the blunter the point is made. This may account fully for the disadvantage of the double cone.

It is well understood that the *weight* of the projectile exercises much influence on the time of its flight at the longer ranges: the time of flight of a ball weighing 105·01 grains and starting with a velocity 1917 feet has already been given: below are the results obtained with a heavier ball.

Rifle No. 1. *Weight of Ball* 243·6 *grains.*

Initial Velocity 1602 ft.*

Although the difference in the initial velocities was 315 feet, yet at 220 yards the difference in the average velocities was

* The initial velocity was *calculated* from determinations made with rifles Nos. 2 and 3. The determination from No. 2 when reduced gave it 1582·5 ft., that from No. 3, 1621·5 ft., the difference being only 39 ft.: the mean of these numbers is given above.

Distance.	Time of Flight	Average Velocity.
220 yards.	·477 sec.	1480 ft.
500 "	1·345 "	1115 "

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only 100 feet, and at 500 yards it was reduced to 31 feet; showing that after a flight of some 600 yards the lighter projectile is overtaken by the heavier. It may be remarked here that the average velocity of these two rifles during a flight of from 500 to 600 yards, is as high or higher than the *initial* velocities of many European guns! Thus the French Tige Rifle has an initial velocity of 1023 feet, the Enfield Rifle 1115 feet, the Belgian chasseur carbine 1007 feet, United States new rifle musket 963 feet and the altered Harpers Ferry rifle 914 feet per second. Certainly Maj. Barnard is justified in his animadversions on the evils which have attended the introduction of the "shot-gun principle" into modern rifles; and with much reason he exclaims—"a decided step has been made *backwards* in losing that most essential element to range and accuracy, initial velocity."*

Position of the Centre of gravity.—In many European projectiles, every effort is made by hollowing out the base to throw the centre of gravity "well forward," in order that the disposition of the ball to fly point-foremost may be encouraged as much as possible, and for the furtherance of the same desirable end, grooves are usually made about its cylindrical portion That the first proceeding exercises a notable influence (at low velocities,) is generally admitted; that it virtually lowers the specific gravity of the ball and therefore retards the flight is no less certain; and if the rudder-like action of the grooves is admitted, their presence also entails a farther loss of velocity.

I have not as yet found time to experiment with either hollow or grooved balls, but the results obtained with solid projectiles, seem to show that it is of small moment whether *their* centre of gravity be situated a little before the middle of the longer diameter, or a little behind it; thus, in the double cone of which mention has been made the centre of gravity was forward of the middle of the axis, but in spite of this, it was found to require almost (if not quite) as many revolutions per second as ball No. 4. Balls were also constructed like Fig. 8 and fired with an initial velocity of 1682 feet when with a rate of 477 revolutions per second; they struck the target sidewise at 165 feet: a velocity of 1060 feet was then tried, when 268 revolutions proved wholly insufficient to project them point-foremost a distance of 12 feet.

8.



It may not be amiss in closing this article to offer a few remarks on the accuracy of the *American* rifle as compared with those now used in Europe.

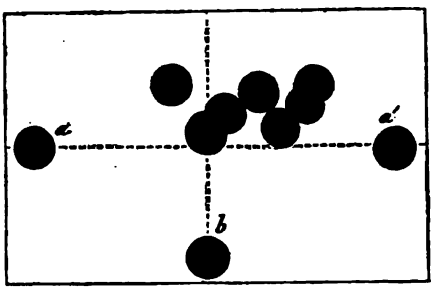
In England it is admitted that the best practice has been obtained by Mr. Whitworth of Manchester with his hexagonal projectile 3 diameters in length, in a covered gallery 500 yards

* This Journal, vol. xxix, p. 197.

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long. Mr. Whitworth states, or is reported to state, that he succeeds in projecting ball after ball into a circle but little more than two inches in diameter, and that he will not rest satisfied till he has fired a ball from one of his rifles down the barrel of another, placed at that distance! This excessive hopefulness of Mr. Whitworth might cause some to hesitate at the story of the two inch circle; assuming it however to be true, it may possibly be shown that the American rifle under like circumstances will do as well or better.

9.



One-half of full size.

The anexed target was made by Lewis with a rifle similar to No. 1, (provided only with globe and bend sights), at a distance of 220 yards, in the presence of over a hundred persons at the yearly trial of skill held at Waltham, Mass.—of course in the open air. The average distance of the shots from the centre is 1·038 inches. Any marksman upon inspecting it will at once see that shots *a* and *a'* were carried to the right and left merely by the wind, and that in a properly constructed gallery all the shots except *b*, would have been included in a circle one inch in diameter. It will also be acknowledged that in shots fired in a covered gallery the deviation with a good rifle is proportional to the distance traversed, though in the open air it is always somewhat greater owing to the wind producing more effect proportionally on the flight of the ball as its velocity becomes lowered.* Therefore as 220 yards : 500 yards :: 1 in. : 2·27, or nine out of the ten shots, at 500 yards, would have been in a circle 2·27 inches in diameter —practice as good as reported by Whitworth.

But what was the nature of the manipulation in each case? In Mr. Whitworth's by the help of "certain appliances with reference to the recoil, guarantees were obtained that each shot should be taken under similar circumstances. The gun was fitted accurately into a frame resting upon a perfectly level plane, and the recoil was compelled to take place in a line precisely

* The anterior probability of this statement may perhaps be rendered stronger by a comparison of the actual with this theoretic deviation. Rifle No. 1 when fired in the open air by only moderately skilled markamen, gave—

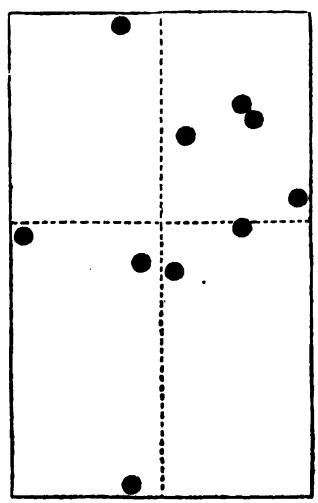
Distance.	Theoretical Deviation.		Actual Deviation.
110 yd.	....	....	0·7 in.
220 "	1·4	+0·2	1·6 "
500 "	3·69	+3·02	6·71 "

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parallel to its axis and could be calculated to a nicety. Again, when the object was to prevent any recoil, there was no difficulty in doing so."* All of the above mentioned precautions being no doubt essential to counteract the evil effects of the heavy recoil and of the *twisting* of the piece. With the American rifle no such artificial bolstering was employed, the barrel was rested on a common shooting bench and the stock held honestly to the shoulder of the marksman, the friction and recoil being so insignificant as not to require other contrivance.

But the Whitworth-gun was tried in the open air at Hythe, April, 1857, in competition with the Enfield rifle, to which it appears to have been greatly superior. We are not informed whether "the firing machine" was transported and used on this occasion; be that as it may, the statement made as to the results obtained is, "that when both had a range of 500 yards the superiority of the Whitworth was in the proportion of three to one." As the absolute deviation of the Enfield rifle at that distance is 28 inches, the deviation of the Whitworth rifle must have been about 9·3 inches, or the shots averaged that distance from the centre of the target. Chapman states that the absolute deviation of the American rifle at 550 yards is 11 inches; his work was published in 1848 since which time very considerable improvement has been effected by our best makers. Knowing this to be the case, I instituted some experiments at 500 yards with rifle No. 1: after it had been sighted for that distance ten shots were fired by a friend, who was but a moderate marksman; a reduced cut of the target is given. The distance of each shot from the centre was measured, the sum of the distances was 67·1 inches: the absolute deviation therefore in this experiment was $6\frac{71}{100}$ inches or $2\frac{1}{2}$ inches less than that of the Whitworth rifle.

10.



One-tenth of full size.

This may serve to show how greatly the American rifle, as made by our best gunsmiths, must, and in fact does surpass in accuracy of fire the various rifles used at the present day in Europe, which are confessedly in this respect inferior to the Whitworth. By reference to the tables below it will be seen that the performance of Rifle No. 1 in the above target made at 500 yards, was superior to that of the Swiss rifle at 200 yards.

* "The Rifle," by Hans Busk, M. A., page 94.

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No less demonstrable is it, that the American rifle-*pistol*, with a barrel only 12 inches in length, but constructed on the same principle as the larger arm, surpasses in the accuracy of its fire, up to a range of 500 yards, most of the rifles now used in Europe.* This may seem incredible; it is highly *significant* as pointing out, that the principles on which rifled-guns should be constructed, have been better apprehended by our countrymen, than thus far at least, by the ordnance boards of European governments.

Absolute Deviation.

	200 yd.	500 yd.
Enfield,	13·3	28·0 in.
Rifle à tige,	11·0	25·2 "
Swiss,	6·8	
American Government,	6·58	17·3 "

Absolute Deviation of 12 *in. Pistol according to Chapman. Weight of Ball* 100 *to* 115 *grains.*

110 yd.	220 yd.	330 yd.	440 yd.	550 yd.
in.	in.	in.	in.	in.
1·5 to 2	2 to 4	6 to 8	10 to 12	14 to 16

Diameters of Circles containing the best half of the shots.†

	164 yd	328 yd.	492 yd.
Swiss Federal,	5·4 in.	11·40 in.	23
Swiss Chasseur,	8·4 "	22·40 "	40
French Rifle à tige,	12·9 "	26·4 "	40
Austrian Rifle,	26·0 "	40 "	90!
Sardinian Rifle,	22·8 "	38 "	83

Diameters of Circles containing a certain proportion of the shots from the American 12*-inch Pistol.*

220 yd.	275 yd.	440 yd.	500 yd.
5·04 in.	9 inches.	30 in.	18 in.
8 out of 10 shots.‡	8 out of 10.§	All the shots.§	4 out of 10.‖

* Although Gen. Jacob has printed no particulars relative to his rifle practice at distances under 1000 yards, still that it cannot have been remarkably accurate, is patent from the table of the time of the flight of his projectiles given on page 35 of his "Rifle Practice," which is copied without comment by Lieut. Busk, page 165 of his work, and again by Lieut. Wilcox, page 210. By reference to either of these books it will be seen that the time of flight for 100 yards is 325 sec., for 200 yards exactly twice as much, for 300 yards just three times as much, and so on up to 600 yards; which is equivalent to saying that the velocity remained undiminished during a flight of 500 yards! A manifest absurdity, which can only be explained by the surmise that the path of the balls was irregular, and therefore allowed no determination of the time of flight to be made with accuracy. I was first unable to comprehend the meaning of this table, until in experimenting with a ball whose flight was known to be irregular, similar results were obtained. Gen. Jacob himself remarks that the table is a little curious.

† According to Lieut. Wilcox.
‡ Chapman, plate VI.
§ Trials by Mr. Lewis.
‖ An experiment of my own.

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Strange indeed is it, that Lieut. Wilcox in his recent treatise on "Rifles and Rifle Practice," while describing each minute variation in the faulty construction of the European arm, should wholly ignore the existence of this most remarkable product of the experimental skill and mechanical ingenuity of his countrymen.

Inasmuch therefore as our own rifle for years has stood without a rival, how happens it that for the use of our army we have been induced to import an inferior arm from France? Were it not better policy to furnish our soldier with the weapon which has became so famous in the hands of our hunters? If greater accuracy or power be required, might it not be well to institute a minute investigation into the causes to which our home-product owes its success, rather than to spend time and incur expense in the study of the inferior rifles of Europe, which although owing their existence to the labors of boards of ordnance, composed of highly educated men, still have never approached in perfection the weapon devised by the *experimental skill* of our American backwoodsmen.

Below are a few of the initial velocities obtained in these experiments. Instead of the average merely, I have given each determination by itself. The powder employed was of very moderate strength, such indeed as is for the most part used in these rifles by our best marksmen:

Rifle No. 2.

Weight of ball 105·01 grains.

Charge.	Initial Vel.
50 grains about 2·15 inches of the bore,	1785
	1799
	1739
	1774
62 grains, about 2·66 in. the bore.	1890
	1925
	1925
	1928
	1917
75 grains, about 3·22 in. to the bore,	2082
	2086
	2084

Rifle No. 3.

Weight of ball 163·04 grains.

Charge.	Initial Vel.
69·4 grs. about 2·3 in. to the bore,	1759
	1782
	1816
	1785

Rifle No. 4.

Weight of ball 113·35.

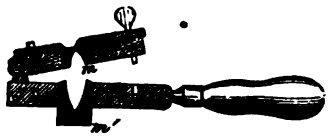
31·5 grs. about 135 in. to the bore,	1141
	1136
	1177
	1057
	1136
	1129

Fig. 11 is a section of an universal bullet mould which is per-

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haps new. These experiments were greatly facilitated by its use. The portions *m* and *m'* are removable at pleasure. To construct a mould for a new ball, it is merely necessary to make the parts *m* and *m'*, all other labor being spared: an important consideration, where new forms of balls must be made by the dozen.

11.



Troy, February, 1860.

ART. IV.—*On the Conservation of Force;* by Prof. JOSEPH HENRY, Smithsonian Institution.*

[THE following remarks upon the conservation of force, particularly in relation to organic matter, by Professor Henry, Secretary of the Smithsonian Institution, will be interesting to those who have given attention to articles on the same subject, which have already appeared in this Journal.

They are extracted from the Agricultural Report of the Patent Office for 1857.]

Organic Molecules.—"The groups of atoms which we have thus far been considering, are principally those which have been formed under the influence of what is called the chemical force, and result from the ordinary attraction of the atoms. These are comparatively simple groups; but there is another class of groups of atoms of a much more complex character, and which are formed of new combinations of the ordinary atoms under the influence or, we may say, direction of that mysterious principle called the *vital force.* We are able to construct a crystal of alum from its elements by combining sulphur, oxygen, hydrogen, potassium, and aluminum; but the chemist has not yet been able to make an atom of sugar from the elements of which it is composed. He can readily decompose it into its constituents, but it is impossible so to arrange the atoms artificially, as in the ordinary cases of chemical manipulation, to produce a substance in any respect similar to sugar. When the attempt is made, the atoms arrange themselves spontaneously into a greater number of simpler and smaller groups or molecules than is found in sugar, which is composed of molecules of high order, each containing no less than 34 atoms of carbon, oxygen, and hydrogen.

The organic molecules, or atoms, as they are called, are built up under the influence of the vital principle of inferior groups of simple elements. These organic molecules are first produced in the leaves of the plant under the influence of light, and subsequently go through various changes in connection with the vital process. After they are once formed in this way, they may be

* Communicated by Prof. Henry.

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combined and recombined by different processes in the laboratory, and a great variety of new compounds artificially produced from them.

But what is this vital principle, which thus transcends the sagacity of the chemist and produces groups of atoms of a complexity far exceeding his present skill? It is generally known under the name of the vital *force;* but since the compounds which are produced under its influence are subject to the same laws, though differing in complexity, as those produced by the ordinary chemical forces; and since in passing from an unstable to a more stable condition in the form of smaller groups, they exhibit, as will be rendered highly probable hereafter, an energy just equivalent to the power exerted by the sunbeam, under whose influence they are produced, it is more rational to suppose that they are the result of the ordinary chemical forces acting under the *direction* of what we prefer to call the vital *principle.* This is certainly not a *force*, in the ordinary acceptation of the term, or in that in which we confine this expression to the attractions and repulsions with which material atoms appear to be primarily endowed. It does not act in accordance with the restricted and uniform laws which govern the forces of inert matter, but with forethought, making provision far in advance of the present condition for the future development of organs of sight, of hearing, of reproduction, and of all the varied parts which constitute the ingenious machinery of a living being. Matter without the vital influence may be compared in its condition to steam which, undirected, is suffered to expend its power in producing mechanical effects on the air and other adjacent bodies, marked with no special indications of design; while matter under its influence may be likened to steam under the directing superintendence of an engineer, which is made to construct complex machinery and to perform other work indicative of a directing intelligence. *Vitality*, thus viewed, gives startling evidence of the immediate presence of a direct, divine and spiritual essence, operating with the ordinary forces of Nature, but being in itself entirely distinct from them.

This view of the subject is absolutely necessary in carrying out the mechanical theory of the equivalency of heat and the correlation of the ordinary physical forces. Among the latter, vitality has no place, and knows no subjection to the laws by which they are governed.

All the constituents of organic bodies are formed of organic molecules, and, as we have said, these are of great complexity, and are readily disturbed and resolved into a greater number of lesser groups. Thus, the constitution of cane sugar is represented by $C_{12} H_{11} O_{11}$, making in all 34 atoms. Organic bodies are, therefore, in what may be called a state of power, or of tottering

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equilibrium, like a stone poised on a pillar, which the slightest jar will overturn; they are ready to rush into closer union with the least disturbing force. In this simple fact is the explanation of the whole phenomena of fermentation, and of the effect produced by yeast and other bodies, which being themselves in a state of change, overturn the unstable equilibrium of the organic molecules, and resolve them into other and more stable compounds. Fermentation, then, consists simply generally in the running down from one stage to another of organic molecules, changing their constitution, and at last arriving at a neutral state. There is, however, one fact in connection with the running down of the organic molecules which deserves particular attention, namely, that it must always be accompanied with the exhibition of power or energy, with a disturbance of the ethereal equilibrium in the form of heat, sometimes even of light, or perhaps of the chemical force, or of that of the nervous energy, in whatever form of motion the lattter may consist. It is a general truth of the highest importance in the study of the phenomena of Nature, that whenever two atoms enter into more intimate union, heat, or some form of motive power, is always generated. It may, however, be again immediately expended in effecting a change in the surrounding matter, or it may be exhibited in the form of one of the radiant emenations.

Balance of Nature.—The term balance of organic nature was first applied, we think, by Dumas, to express the relations between matter forming animals and vegetables, and the same matter in an inert condition. We shall apply the term "balance of nature," in a more extended sense, and include within it the balance of power, as well as the transformations of matter. The amount of matter in the visible universe is supposed to remain the same, though it is subject to various transformations, and appears under various forms—now built up into organic molecules, and now again resolved into the simple inorganic compounds. The carbon and other materials absorbed from the air by the plant is given back to the atmosphere by the decaying organisms, and thus what may be called a constant balance is preserved. But this balance, if we may so call it, does not alone pertain to the matter, but also to the energy which is employed in producing these changes. It may disappear for a while, or may be locked up in the plant or the animal, but is again destined to appear in another form, and to exert its effects, perhaps in distant parts of celestial space.

To give precision to our thoughts on this subject, let us suppose that all the vegetable and animal matter which now forms a thin pellicle at the surface of the earth were removed—that nothing remained but the germs of future organisms buried in the soil and ready to be developed when the proper influences

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were brought to bear upon them. Let us further suppose the sun to cease giving emanations of any kind into space. The radiation from the earth, uncompensated by impulses from the sun, would soon reduce the temperature of every part of the surface to at least 60° below zero; all the matter and liquid substances capable of being frozen would be reduced to a solid state; the air would cease to move, and universal stillness and silence would prevail.

Let us now suppose that the sun were to give forth rays of heat alone; these would radiate in every direction from the celestial orb, and an exceedingly small portion of them, in comparison with the whole, would impinge against the surface of our distant planet, would melt the ice first on the equator, then on the more northern and southern parts of the globe, and, finally, their genial influence would be felt at the poles. The air would be unequally rarefied in the different zones, the winds would again be called forth, vapor would rise from the ocean, clouds would be formed, rain would descend, and storms and tempests would resume their sway.

If the sun should again intermit its radiation, all these motions would gradually diminish, and after a time entirely cease; the heat given to the earth would, in part, be retained for awhile, but in time would be expended; the water would slowly give out its latent caloric and be again converted into ice. Something of this kind takes place in the northern and southern parts of the earth during the different periods of summer and winter. Since the mean temperature of the earth does not vary from year to year, it follows that all the excess of heat of summer received from the sun is given off in winter and hence the impulses from this luminary which constitute all the energy, producing the changes on the surface of the earth, merely lingering for awhile, are again sent forth into celestial space, changed, it may be, in form, but not in the amount of their power. The solar vibrations have lost none of their energy, for the water has returned to the state of ice, and the surface of the earth is again in the same condition in which it was before it received the solar impulse. The energy of the solar vibrations communicated to the ice overcomes its cohesion, converting it into the liquid state, and the ice again becoming solid gives out the same amount of heat in a less energetic form. Even the motive power of the wind is expended by the friction of its particles in producing an amount of heat equivalent to that which gave rise to its motion, and this also is radiated into celestial space.

But the most interesting part of our inquiry relates to the effects which the radiation alone of heat from the sun would have on the vegetable germs buried in the soil. If these germs were enclosed in sacs filled with starch and other organic ingre-

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dients, stored away for the future use of the young plant, as in the case of the tuber of the potato, or the fleshy part of the bean, as soon as the sun penetrated beneath the surface in sufficient degree to give mobility to the complex organic molecules of which these materials consist, the proper degree of moisture also supposed to be present, germination would commence. The young plant would begin to be developed, would strike a rootlet downward into the earth, and elevate a stem towards the surface furnished with incipient leaves. The growth would continue until all the organic matter in the tuber or sac was exhausted; the further development of the plant would then cease, and in a short time decay would commence.

But let us dwell a few minutes longer on the condition of the plant and the tuber before the downward action becomes the subject of consideration. If we examine the condition of the potato which was buried in the earth, we shall find remaining of it nothing but the skin, which will probably contain a portion of water. What has become of the starch and other matter which originally filled this large sac? If we examine the soil which surrounded the potato, we do not find that the starch has been absorbed by it; and the answer which will, therefore, naturally be suggested is, that it has been transformed into the material of the new plant, and it was for this purpose originally stored away. But this, though in part correct, is not the whole truth; for if we weigh a potato prior to germination, and weigh the young plant afterwards, we shall find that the amount of organic matter contained in the latter is but a fraction of that which was originally contained in the former. We can account in this way for the disappearance of a *part* of the contents of the sac, which has evidently formed the pabulum of the young plant. But here we may stop to ask another question: By what power was the young plant built up of the molecules of starch? The answer would probably be, by the exertion of the vital force; but we have endeavered to show that vitality is a *directing principle*, and not a mechanical power, the expenditure of which does work. The conclusion to which we would arrive will probably now be anticipated. The portion of the organic molecules of the starch, &c., of the tuber, as yet unaccounted for, has run down into inorganic matter, or has entered again into combination with the oxygen of the air, and in this running down, and union with the oxygen, has evolved the power necessary to the organization of the new plant.

The oxygen of the atmosphere penetrates into the interior of the potatoe, to enter into combination with the gluten and starch;—or, in other words, to burn it by a slow combustion; and the carbonic acid and water produced find their way, in turn, back to the atmosphere. We see from this view that the

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starch and nitrogenous materials, in which the germs of plants are imbedded, have two functions to fulfill—the one to supply the pabulum of the new plant, and the other to furnish the power by which the transformation is effected, the latter being as essential as the former. In the erection of a house, the application of mechanical power is required as much as a supply of ponderable materials.

To return to our first supposition. We have said (and the assertion is in accordance with accurate observation) that the plant would cease to increase in weight under the mere influence of heat, however long continued, after the tuber was exhausted. Some slight changes might, indeed, take place; a small portion of pabulum might be absorbed from the earth; or one part of the plant might commence to decay, and thus furnish nourishment to the remaining parts; but changes of this kind would be minute, and the plant, under the influence of heat alone, would, in a short time, cease to exist.

Let us next suppose the sun to commence emitting rays of *light*, in addition to those of heat. These, impinging against the earth, would probably produce some effects of a physical character; but what these effects would be we are unable, at the present time, fully to say. We infer, however, that the light, not immediately reflected into space, would be annihilated; but this could not take place without communicating motion to other matter. It would probably be transformed into waves of heat of feeble intensity.

Let us now suppose, in addition to heat and light, the chemical rays to be sent forth from the sun. These would also produce various physical changes, the most remarkable of which would be in regard to the plant.

The carbonic acid of the atmosphere, in contact with the expanding surface of the young leaves, would be absorbed by the water in their pores, and in this condition would be decomposed by the vibrating impulses which constitute the chemical emanation. The atoms of carbon and oxygen, of which the carbonic acid is composed, would be forcibly separated; the atoms of oxygen would be liberated in the form of gas, and the carbon be absorbed to build up, under the directing influence of vitality, the woody structure of the plant. In this condition the pabulum of the plant is principally furnished by the carbonic acid of the air, while the impulses of the chemical ray furnish the primary power by which the decomposition and the other changes are effected. This is the general form of the process, leaving out of view minute changes, actions and reactions, which must take place in the course of organization.

In the decomposition of the carbonic acid by the chemical ray, a definite amount of power is expended, and this remains, as it

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were, locked up in the plant so long as it continues to grow; but when it has reached its term of months or years, and some condition has been introduced which interferes with the balance of forces, then a reverse process commences, the plant begins to decay, the complex organic molecules begin to run down into simpler groups, and then again into carbonic acid and water. The materials of the plant fall back into the same combinations from which they were originally drawn, and the solid carbon is returned in the form of a gas to the atmosphere, whence it was taken. Now, the power which is given out in the whole descent, is, according to the dynamic theory, just equivalent to the power expended by the impulse from the sun in elevating the atoms to the unstable condition of the organic molecules. If this power is given out in the form of vibrations of the ethereal medium constituting heat, it will not be appreciable in the ordinary decay, say of a tree, extending, as it may, through several years; but if the process be rapid, as in the case of combustion of wood, then the same amount of power will be given out in the energetic form of heat of high intensity. This heat will again radiate from the earth; and in this case, as in that we have previously considered, the impulse from the sun merely lingers for a while upon the earth, and is then given back to celestial space, changed in form, but undiminished in quantity. It may continue its radiating course through stellar space, until it meets planets of other systems; but to attempt to trace it further would be to transcend the limits of inductive reason, and to enter those of unbridled fancy.

In the process we have described, the carbon, hydrogen, and other substances which are absorbed from the atmosphere, are returned to this great reservoir to be used again, and, it may be, to undergo the same changes many times in succession. The earthy materials are again returned to the earth, and all the conditions, as far as the individual plant which we are considering is concerned, are the same as they were at the beginning. The absorption of power in the decomposition of the carbonic acid gas, and its evolution again when the recomposition is produced of the same atoms, is precisely analogous to that which takes place in forcibly separating the poles of two magnets, retaining them apart for a certain time, and suffering them to return by their attractive force to their former union. The energy developed in the approach of the magnets towards each other is just equal to the force expended in their separation.

By extending this reasoning to the vast beds of coal which are stored away in the earth, we are brought irresistibly to the conclusion that the power which is evolved in the combustion of this material, now so valuable an agent in the processes of manufacture and locomotion, is merely the equivalent of the force which was expended in decomposing the carbonic acid which

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furnished the carbon of the primeval forests of the globe; and that the power thus stored away millions of years before the existence of man, like other preordinations of Divine Intelligence, is now employed in adding to the comforts and advancing the physical and intellectual well-being of our race.

In the germination of the plant a part of the organized molecules runs down into carbonic acid to furnish power for the new arrangement of the other portion. In this process no extraneous force is required; the seed contains within itself the power and the material for the growth of the new plant up to a certain stage of its development. Germination can, therefore, be carried on in the dark, and, indeed, the chemical ray which accompanies light retards rather than accelerates the process. Its office is to separate the atoms of carbon from those of oxygen in the decomposition of the carbonic acid, while that of the power within the plant results from the combination of these same elements. The forces are therefore antagonistic, and hence germination is more rapid when light is excluded; an inference borne out by actual experiment.

Animal Organism.—Besides plants, there is another great class of organized beings, viz: animals; and as we commenced with the consideration of the seed in the first case, let us begin in this with the egg. This, as is well known, consists of a sack or shell containing a mass of organized molecules formed of the same elements of which the plant is composed, viz: carbon, hydrogen, oxygen, and nitrogen, with a minute portion of sulphur and other substances. Without attempting to describe the various transformations which take place among these organized molecules, a task which far transcends our knowledge or even that of the science of the day, we shall merely consider the general changes which occur of a physical character.

As in the case of the seed of the plant, we presume that the germ of the future animal pre-exists in the egg, and that by subjecting the mass to a degree of temperature sufficient perhaps to give greater mobility to the molecules, a process similar in its general effect to that of the germination of the seed commences. Oxygen is absorbed through some of the minute holes in the shell, and carbonic acid constantly exhaled from others. A portion then of the organic molecules begins to run down, and is converted into carbonic acid, and, possibly, water. During this process power is evolved within the shell—we cannot say, in the present state of science, under what particular form; but we are irresistibly constrained to believe that it is expended under the direction, again, of the vital principle, in rearranging the organic molecules, in building up the complex machinery of the future animal, or developing a still higher organization, connected with which are the mysterious manifestations of thought and volition.

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In this case, as in that of the potato, the young animal, as it escapes from the shell, weighs less than the material of the egg previous to the process of iucubation. The lost material in this case, as in the other, has run down into an inorganic condition by combining with oxygen, and in its descent has developed the power to effect the transformation we have just described.

We have seen, in the case of the young plant, that after it escapes from the seed, and expands its leaves to the air, it receives the means of its future growth principally from the carbon derived from the decomposition of the carbonic acid of the atmosphere, and its power to effect all its changes from the direct vibratory impulses of the sun. The young animal, however, is in an entirely different condition; exposure to the light of the sun is not necessary to its growth or existence; the chemical ray, by impinging on the surface of its body, does not decompose the carbonic acid which may surround it, the conditions necessaary for this decomposition not being present. It has no means by itself to elaborate organic molecules, and is indebted for these entirely to its food. It is necessary, therefore, that it should be supplied with food consisting of organized materials, that is, of complex molecules in a state of unstable equilibrium, or of power. These molecules have two offices to perform: one portion of them, by their transformations, is expended in building up the body of the animal, and the other in furnishing the power required to produce these transformations, and, also, in furnishing the energy constantly expended in the breathing, the pulsations, and the various other mechanical motions of the living animal. We may infer from this that the animal, in proportion to its weight before it has acquired its growth, will require more food than the adult, unless all its voluntary motions be prevented; and secondly, that more food will be required for sustaining and renewing the body when the animal is suffered to expend its muscular energy in labor or other active exercise.

The power of the living animal is immediately derived from the running down of the complex organized molecules, of which the body is formed, into their ultimate combination with oxygen, in the form of carbon, water and ammonia. Hence, oxygen is constantly drawn into the lungs, and carbon is constantly evolved. In the adult animal, when a dynamic equilibrium has been attained, the nourishment which is absorbed into the system is entirely expended in producing the power to carry on the various functions of life, and to supply the energy necessary to perform all the acts pertaining to a living, sentient, and, it may be, thinking being. In this case, as in that of the plant, the power may be traced back to the original impulse from the sun, which is retained through a second stage, and finally given back again to celestial space, whence it emanated. All animals are constantly radiating heat, though in different degrees, the amount

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in all cases being in proportion to the oxygen inhaled and the carbon exhaled. The animal is a curiously contrived arrangement for burning carbon and hydrogen, and the evolution and application of power. In this respect it is precisely analogous to the locomotive, the carbon burnt in the food and in the wood performing the same office in each. The fact has long been established, that power cannot be generated by any combination of machinery. A machine is an instrument for the application of power, and not for its creation. The animal body is a structure of this character. It is admirably contrived, when we consider all the offices it has to perform, for the purpose to which it is applied, but it can do nothing without power, and that, as in the case of the locomotive, must be supplied from without. Nay, more, a comparison has been made between the work which can be done by burning a given amount of carbon in the machine, man, and an equal amount in the machine, locomotive. The result derived from an analysis of the food in one case, and the weight of the fuel in the other, and these compared with the quantity of water raised by each to a known elevation, gives the relative working value of the two machines. From this comparison, made from experiments on soldiers in Germany and France, it is found that the human machine, in consuming the same amount of carbon, does four and a half times the amount of work of the best Cornish engine. The body has been called "the house we live in," but it may be more truly denominated the machine we employ, which, furnished with power, and all the appliances for its use, enables us to execute the intentions of our intelligence, to gratify our moral natures, and to commune with our fellow beings.

This view of the nature of the body is the furthest removed possible from materialism; it requires a separate thinking principle. To illustrate this, let us suppose a locomotive engine, equipped with steam, water, fuel—in short, with the potential energy necessary to the exhibition of immense mechanical power; the whole remains in a state of dynamic equilibrium, without motion or signs of life, or intelligence. Let the engineer now open a valve which is so poised as to move with the slightest touch, and almost with volition, to let on the power to the piston; the machine now awakes, as it were, into life. It rushes forward with tremendous power, it stops instantly, it returns again, it may be, at the command of the master of the train; in short, it exhibits signs of life and intelligence. Its power is now controlled by mind—it has, as it were, a soul within it. The engine may be considered as an appendage or a further development of the body of the engineer, in which the boiler and the furnace are an additional capacious stomach for the evolution of power; and the wheels, the cranks and levers, the bones, the sinews, and the muscles, by which this power is applied.

Washington, D. C., 1860.

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ART. V.—*On a mode of employing Instantaneous Photography as a means for the Accurate Determination of the Path and Velocity of a Shooting Star, with a view to the Determination of its Orbit;* by JONATHAN H. LANE.

A tolerably accurate knowledge of the orbits of those meteors, or shooting stars, which may enter our atmosphere, would be of very high value in the settlement of certain questions as to their origin. Hitherto this knowledge has appeared unattainable by reason of the difficulty of effecting sufficiently precise observations of the meteor in the transient period of its visible flight, especially considering how this difficulty is aggravated on account of retardation of its motion by the resistance of the atmosphere. Recently, however, a method has occurred to me of applying instantaneous photography, so as to show accurately, not only the track of the meteor, but the division of its track into many equal and known fractions of time. If this can be successfully accomplished, we should have the data for ascertaining the velocity of the meteor at each point of the recorded part of its track, and the rate or law of variation of the velocity, and thence, with probably a good degree of accuracy, the velocity it had beyond the limits of the atmosphere, and the like remark may be made concerning the direction of the motion, should that be found subject to change.

The basis of the proposed process, as already intimated, is the extraordinary advances that have been made within a few years in the preparation of the sensitized surfaces of photographic plates, whereby artists are enabled to produce good pictures by an exposure of a very small fraction of a second—so small as to afford a tolerable definition of objects in motion, such as sailing vessels. This holds out encouragement for a hope, at least, that a passing meteor would leave a visible trace on a plate so prepared, or, even if that degree of sensitiveness has not yet been reached, that it will be hereafter. I need therefore make no apology for placing the suggestion on record previous to direct experiment on this point.

In the first place, simple exposure in a camera, at a given station, would give the apparent track of a meteor as seen by the observer at that station, and a pair of such records made in two cameras at two stations, would give the track in absolute space. In the second place, if one of the two cameras were furnished with a mechanism by which equidistant points of time should be marked upon the trace made in that camera, these points could be referred to the real path in space, and if both cameras were in like manner furnished, the two records would, to that extent be a check upon each other, and serve to reduce the limits of probable error. The

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device for marking time is an application of the revolving glass prism, very similar to that described in my paper on a visual method of comparing time between distant stations, published in the January number of this Journal.* Immediately in front of the object glass of the camera, a glass prism of small angle and sufficient area to cover the entire aperture, is made to rotate at an accurately measured rate of say twenty-five revolutions per second. The prism may be replaced by an excentric lens, or the object glass itself may revolve on a slightly excentric axis. The consequence will be that the image of a fixed star in any part of the field of view will traverse the circumference of a circle every twenty-fifth of a second, and the image of a shooting star will combine this motion with its motion of translation. If the photographic surface retain a visible impression of the looped curve or the waved curve which will thus be produced, then, neglecting for the present the small effects of optical distortion, the line drawn midway between the two straight or regularly curved lines between which the looped or waved curve oscillates, will represent the apparent track of the meteor, and the points where it intersects the looped or waved curve, if they be translated along this middle line through a space equal to the optical displacement of the meteoric image, will show the apparent place occupied by the meteor at points of time separated by the equal intervals of one fiftieth of a second. If the period be made too brief, the impression left by the head of the meteor in one sweep of the looped or waved curve, might possibly be obliterated by the impression of the closely following parts of its train, while the head is traversing the subsequent sweeps of the curve. But there is no reason to anticipate from this cause any difficulty in obtaining a sufficiently short period to determine the law of variation of the velocity or direction.

In the above statement I have supposed only a single camera, but it will probably be impossible in this way to command a sufficient extent of the heavens. A system of many cameras may, however, be formed, so arranged that their several optic axes shall cross in a common point *in front* of the object glasses. The object glasses may thus be approximated as closely as we can desire, and the several revolving prisms, or excentric lenses, may have a common geared connection, and the backs of the cameras will be readily accessible for the renewal of plates. When the track of a meteor, by reason of its extent or situation, is obtained in parts from different cameras of such a system, it is geometrically impossible, on account of the spherical excess, that the exact interval of one fiftieth of a second between the times

* Vol. xxix, 43.

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marked upon the meteor's track, should, in general, be preserved in the transition from one plate to another in all situations of the track, or in other words, that every two adjacent cameras in the system shall be capable of marking, in the manner described, the same common point of time upon the track of a meteor, but the exact difference in time can always be known.

In the execution of such a plan as this, two stations are to be selected at a suitable distance, and a system of cameras established at each, of such range that the two may cover in common a sufficient extent of the upper regions of the atmosphere to afford a fair chance for the occurrence of meteors. Each station will require at least an observer and a photographer. The photographer will renew the plates as often as their surfaces, either from time or exposure, become impaired, and will perform the manipulations required in fixing the impression when taken. The observer, after having made the necessary instrumental adjustments and determinations will be charged with the sole duty of watching for meteors in the region covered by his system of cameras, and at the appearance of a meteor will touch a spring so contrived as to cause the instant unveiling of all the cameras of the system, and on the extinction of the meteor will promptly replace the screen.

The expense and trouble of this process will certainly be great, but will not be disproportioned to the importance of the object in view. Only let us have a photographic surface that will give a visible trace of the meteor's path, in the face of exposure to the light of the sky during the time of the meteor's visible flight, and then success, as regards the attainments of an accurate record, will be nearly certain, and we should not hesitate at the expense and trouble.

Nor does it seem to me that our success would be much less certain in respect to the reliable determination of the direction and velocity which the meteor had before entering the atmosphere, and consequently of the orbit in which it had moved. A very simple calculation based upon the mechanical theory of heat, leads us to the conclusion that any body of a nature to become readily incandescent by heat, of such a thickness as half an inch, and not possessing a greater power of conducting heat through its mass than any we are acquainted with, must, on entering our atmosphere with planetary velocity, become self luminous by the time that velocity has been reduced by some such fractional part as one thousandth. The *vis viva* of a body moving with a velocity of twenty miles a second is equivalent to the heat that would raise the temperature of an equal weight of water about 224,000° Fah. With such a velocity, so many times exceeding that of sound, the masses of air lying in the

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path of the body must be driven before it, and receive a velocity equal to that of the body, or at least to a large fractional part of it. The mass of air which the body must have encountered in losing the thousandth part of its velocity, will, therefore, be of the order of a thousandth part of that of the body. With the loss of a thousandth part of the velocity, the loss of the body's own *vis viva* will correspond to the quantity of heat that would raise the temperature of its weight of water 448° Fah. If after making allowance for the motion communicated to the displaced air—approximately one half—and for the quantity of generated heat which this air retains and carries off, we assume that a twentieth part of the above 448° enters into the body itself, and by reason of the rapidity of its production is collected in a superficial coating of a hundreth part of its mass, and give this a specific heat within that of water, we should find an elevation of temperature of 2240° or upwards. The inference we would draw from these considerations seems confirmed by what we know of the great length of the visible flight of meteors, and of the great elevation of the region of atmosphere in which it occurs.

If, therefore, upon suitable trials made upon the fixed stars, and upon shooting stars themselves, we shall find ourselves in possession of sufficient photographic power, there is no reason why an organized system of observations should not be instituted. If the fact in regard to the retardation of a meteor's motion be as the foregoing considerations lead us to anticipate, the discussion of a collection of such records as we should obtain, of a large number of meteors, will be likely to afford us complete assurance on the subject, by pointing out certain laws of the resistances at different altitudes. A moderate degree of accuracy in the absolute determination of the orbits, except, when they make a near approach to the parabola, will be sufficient to answer all the questions of interest that will be likely to arise upon which a knowledge of the orbits would have any bearing. Whether the November meteors, for instance, move through regions that would identify them with the Zodiacal light, according to the theory of the late Prof. Olmsted, is a question that would receive an absolute determination.

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ART. VI.—*The True Figure of the Earth.*—Notice by Mädler in Prof. Heis' Wochenschrift für Astronomie, Meteorologie und Geographie, No. 51 and 52. Dec. 21 and 28, 1859.*

Essai d'une détermination de la véritable figure de la terro. Par T. F. DE SCHUBERT. St. Petersbourg, 1859.

THIS brief but very important paper treats of a question which has engaged mankind for ages, and treats it in such a way as to convince us that an essential step forward has been taken towards its final determination.

It is not intended to recapitulate the history of opinions and notions which antiquity, as well as the middle ages, had formed on this topic so generally interesting, as it does not belong to the plan of the work to be discussed.

Since the establishment of Newton's theory, that the earth is flattened at its poles, and the confirmation of this theoretical result by the first measurements of arcs in the eighteenth century, the chief inquiry has been directed to the magnitude of this compression, or, in other words, the difference between the semi-major and the semi-minor axes of the earth. The attempt was made to determine it by comparing arcs measured in different latitudes, the latitudes of their extremities being astronomically and their extent on the earth geodetically determined. In this manner was obtained, between 1735 and 1746, two arcs, the one in Peru, and the other in Lapland, which could be compared with each other, as well as that previously measured by Cassini in France. The result of the comparison was not satisfactory. Although they proved a comparison, the measures did not agree, and the source of this difference was too obscure to favor the supposition that any nearer approach had been made to the object of research.

In the course of the eighteenth century measures of arcs of the meridian were executed or attempted in France, Austria, Italy and Pennsylvania, at the Cape of Good Hope and other places. Their comparison made it apparent that the compression ($\frac{1}{178}$), as at first concluded, was too great, and that it must be reduced one half or even more. Yet the uncertainty remained very considerable.

The measurement of arcs was continued in the nineteenth century over greater extents in different parts of the earth and with greater care and accuracy, and the close agreement of these arcs, as it appears from the comparison of their parts, left but little, further to be desired. Perceptibly different values for the compression were nevertheless obtained, as when the most proba-

* Translated for the American Journal of Science by CHARLES A. SCHOTT, Assistant, and communicated by Prof. A. D. BACHE, Supt. U. S. Coast Survey.

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ble value was determined from them all, there still remained from the separate measures of arcs, deviations which could not be ascribed to errors of observation.

Repeated investigation has shown that the old measurement of a degree by Maupertuis and Outhier was considerably in error. Rosenberg and Swanberg obtained quite different values. In like manner the measures executed by Boscovich, Le Maire, Liesganig, Beccaria and others in the eighteenth century could not bear severe criticism nor stand side by side with the better and approved arcs. A more accurate and reliable coefficient of compression still remains a desideratum.

We insert here for comparison the values of the compression, as they result, for the most part, from the same measures, but according to different methods of reduction, in the first four decades of this century.

	Semi-major axis in toises.	Semi-minor axis in toises.	Compression in toises.	Coeff. of compression.
Walbek, - -	3271742·8	3260989·3	10785·5	$\frac{1}{302\cdot78}$
Schmidt, - -	3271852·3	3260853·7	10998·6	$\frac{1}{297\cdot48}$
Bessel, (I.), -	3271953·9	3261072·9	10881·0	$\frac{1}{300\cdot70}$

As Bessel afterwards discovered an error in Delambre's French triangulation which could not be without influence on the combined result, he repeated his reduction with this correction and found—

Bessel, (II), -	3272077·1	3261139·3	10937·8	$\frac{1}{299\cdot15}$

Still, according to the last reduction, the probable error is three times as great as the difference of Bessel (I) and Bessel (II).

Since in this even the best measures of this sort do not yield sufficiently accordant results, it was attempted to find the compression in two other ways. The observations of the pendulum give, as a consequence of the earth's compression, different lengths of the seconds pendulum in different latitudes, and hence we can deduce, conversely, the compression from these observed lengths or, if we choose, from the number of beats which the same pendulum makes in the course of a day. Observations made with extreme care were used as the foundation of such experiments, yet still the result ($\frac{1}{311\cdot31}$) fluctuated between the limits, $\frac{1}{313}$ and $\frac{1}{301}$.

The moon's path, on which the compression was not without influence, was also resorted to, and gave $\frac{1}{305}$ for the compression, but on account of the smallness of the aggregate effect it is of inferior accuracy.*

We have found it necessary to present this general view in order to define the limit of the results arrived at by preceding

* This result depends, in a measure, on the law of density of the earth in passing from the surface to the centre.

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efforts, and in order to place the merit of the writer in its proper light, especially for those not possessing a special knowledge of the subject in question.

The preceding investigations were all based upon the following assumptions:

(1.) The meridians of the earth are ellipses.*

(2.) The minor axis is also the axis of rotation.

(3.) All the meridians of the earth are equal.

The writer remarks that, in a rigorous sense, no one of these assumptions is proved and that we do not possess the means of proving the first two. We may however add, that these two assumptions, if not absolutely, must be very nearly true.†

In regard to the third assumption we are now prepared to submit it to investigation, and the previous failures compel us to question its applicability. Paucker and Borenius have already attempted to prove that it must be false, but neither has arrived at any definite result. The writer uses as a basis the following measurements of arcs:

(1.) The Russian (or more properly Russo-Scandinavian) executed in 1820–51 by Hansteen, Selander, Struve and Tenner from Fuglenäs (in latitude 70° 40′ 11″ ·3 N) to Staro-Nekrasofka (45° 20′ 02″ ·8 N) the longest arc yet measured.

(2.) The Indian arc, 1802–43, by Lambton and Everest, from Kaliana (29° 30′ 48″ ·9 N) to Punnæ (8° 09′ 32″ ·3 N).

(3.) The French arc, 1792–1806, by Méchain, Delambre, Biot and Arago, from Dunkirk (51° 02′ 08″ ·5 N) to Formentera (38° 39′ 56″ ·1 N).

(4.) The measure at the Cape of Good Hope, by Henderson and Maclear, from 34° 21′ 06″ ·3 S to 29° 44′ 17″ ·7 S.

(5.) The Peruvian arc, 1735–46, by Bouguer and la Condamine, from Tarqui (3° 04′ 32″ ·1 S) to Cotchesqui (0° 02′ 31″ ·4 N).

(6.) The Prussian arc, 1831–34, by Bessel and Bæyer, from Memel (55° 43′ 40″ ·4 N) to Trunz (54° 13′ 11″ ·5 N).

(7.) The British arc, by Roy and Mudge, from Clifton (53° 27′ 31″ ·1 N) to Dunnose (50° 37′ 07″ ·6 N).

(8.) The Pennsylvanian arc, 1764, by Mason and Dixon, from 39° 56′ 22″ ·5 N to 38° 27′ 37″ ·5 N.‡

The writer does not explain why he has not taken in other reliable measures, in particular the Hanoverian by Gauss and the Danish arc by Schumacher; but his principal aim being to prove merely the untenability of the assumption that all meridians are equal and similar, and pronouncing his own work as preliminary, no exception can be taken to this course. In a perfectly rigorous investigation by the method of least squares, where each

* In the recent account of the Ordnance Survey of Great Britain the figure of the meridian, in one of the hypotheses, is not restricted to this condition.

† The truth of the second assumption may be granted.

‡ The reason for taking in this arc will be apparent from what follows.

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measure with its corresponding weight would enter, such omissions could of course not be permitted.

Reducing and comparing with each other, each of the eight arcs, he obtains twenty eight binary combinations, which he communicates in detail; but this can only be for the purpose of showing the impossibility of an agreement by the assumption hitherto made. For instance, the Russian arc compared with the English gives $\frac{1}{299.7}$, the French compared with the English gives $\frac{1}{178.4}$, and the deviations from the general mean are still greater in comparing the Prussian and Pennsylvanian arcs with all the others. It is thence inferred that the meridians of the earth are not equal to each other, and thence that the equator and the parallels are not circles, so that it is generally impossible to draw a great circle on the earth's surface anywhere.

Meridians different in form and length really indicate different polar compressions, a non-agreement of the results found, proving nothing against the accuracy of the measures.

Now since all meridians must converge at the poles they must all have in common one and the same diameter, viz., the smallest (the axis of rotation) which can be obtained from each of the large arcs referred to.

The three greatest measured arcs are—

the Russo-Scandinavian	=	25°	20′	08″·5,
" Indian	=	21	21	16 ·6,
" French	=	12	22	12 ·4,

whilst all the others, including those not used by the writer, are less than 5° in extent.

By dividing each of these three arcs into two equal parts and comparing one part with the other and also each part with the whole, the writer obtains the following mean values:

	Semi-major axis (toises).	Semi-minor axis (toises).
From the Russian	3272610·3	3261428·7
" " Indian	3272650·9	3261547·4
" " French	3273448·2	3260364·7

In the first two, the differences are unimportant, but the last shows a greater deviation. Schubert remarks that this latter deviation can be got rid of by changing the latitude of Carcassonne, the selected point of division of the French arc, by 1″·96, which may not exceed the limits of uncertainty. He considers it safer, however, to obtain the semi-minor axis from the first two arcs alone, the greater arc (the Russian) having twice the weight of that of India. In this manner he deduces the semi-minor axis:

3261467·9 toises.

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By means of this value the semi-major axis can be found for each measure of an arc. It results as follows:

For the meridian of	Kaliana	in	Long. 95° 20′	3272581·3
" "	" Dorpat	"	" 44° 23′ 10″	3272650·1
" "	" Tarqui	"	" 298° 44′	3272382·8

Three radii and their included angles suffice for the determination of the ellipse. The writer finds for the semi-major axis of the equatorial ellipse 3272671·5 toises
and its direction 58° 44′
for the semi-minor axis of the equatorial ellipse 3272303·2
and its direction 148° 44′

Compression of the equator..... = $\frac{1}{\text{▪▪▪▪}}$;
Polar compression of the greatest meridian = $\frac{1}{\text{▪▪▪·▪▪▪}}$
" " " smallest " = $\frac{1}{\text{▪▪▪·▪▪▪}}$,

and the separate arcs calculated with these values have the following deviations:

	in toises.	in arc.
Peru,.....	+ 1·2	0″·077
Pennsylvania,.....	−105·68	6 ·687
England,.....	+ 11·68	0 ·736
France,.....	− 25·45	1 ·607
Cape of Good Hope,.....	− 6·98	0 ·442
Prussia,.....	+ 23·28	1 ·267
Russia,.....	− 20·47	1 ·289
India,.....	+ 25·54	1 ·619

In reference to the arcs showing greater deviations the writer remarks that the Pennsylvanian arc was measured with imperfect means (by the chain only, and without triangulation) and that he has taken it merely for trial.* The other seven deviations are so small that no one exceeds the ten thousandth part of the measured length, and the great accuracy of the Peruvian measure, which had been already shown in former discussions, appears here in its true excellence. Honor is justly due to the memory of the men who, at so early a period, accomplished such a work under the untold difficulties of ten years' separation from all civilized countries.

It has frequently been shown that the geodetic measures, in particular the longitudes, do not harmonize with astronomical observations, but no one has yet succeeded in discovering the cause of this disagreement. The ellipticity of the earth's equator, as discovered by the writer, will call forth new investigations,

* Dr. Maskelyne in his description of Mason and Dixon's base (Phil. Trans., London, 1768) says they employed *rods of fir* frequently compared with a standard brass measure at a fixed temperature. This line was revised in 1849 and 1850 by Lt. Col. Graham, U. S. Top. Eng. See Message of the Gov. of Md., &c., in relation to the intersection of the boundary lines of the States of Maryland, Pennsylvania and Delaware. Washington, 1850.

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since it shows that each geodetic survey, and particularly the longitudinal differences, require correction. The writer shows from two examples how we must proceed and what we may expect.

Between Pulkowa and Dorpat the difference of longitude was found as follows:

Astronomical	3°	36′	16″·77		
Geodetic	3	36	23 ·07′		
Difference			+6″·94	=	57·9 toises.
Correction now found to the geodetic is—			−6″·01	=	50·1 toises.
and the remaining error			= 0″·93	=	7·8 toises.

Between Pulkowa and Warsaw is found—

Astronomical	9°	17′	48″·43		
Geodetic	9	18	01 ·24		
Difference			+12″·81	=	124·7 toises.
Correction to the geodetic			−18 ·85	=	183·4 toises.
remaining error			= − 6 ·04	=	58·7 toises.

The first error is thus completely got rid of and the second is reduced to nearly one half.

The writer concludes with the following remarks:

"This determination of the earth's figure is only an approximation, subject to many improvements, when we have more data, and when we use more rigorous methods of reduction. But it shows that we can obtain an agreement of the measures in which we have not heretofore succeeded. The determination of the general figure of the earth does not exclude local irregularities of its surface.

When this hypothesis of the earth's figure, after a scrutinizing investigation, finds general adoption, all geographical positions determined by geodetic means must have corrections applied, which will refer principally to the differences of longitude."

As much as I may agree with this view and as certainly as the importance of the object requires the application of rigorous methods, it is yet clear that the trouble will be sufficiently rewarded when further reliable data, particularly from the western hemisphere, shall be available to the computer. It may be remarked here that the meridian in the western hemisphere corresponding to the small equatorial axes passes through Newfoundland, and, in the eastern, through the Amoor country and Eastern Siberia. For these countries we have at present not even approximate determinations. The Pennsylvania measure of 1764 is worthless for accurate investigation and the superior Peruvian measure is at too great a distance. Measurements of

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arc in the meridian of Irkutsk and extending through Mantchooria southwards, through the Chinese empire, as well as the Atlantic States of the American Union* will give us the smallest meridian of the earth with the same accuracy, corresponding to the determination of the greatest, which passes through Eastern Europe (Kostromo, Stawropol, Erzerûm). Still, much requires to be done. The continuation of the Russian Survey through Turkey, and if possible to Eastern Africa, would be most important; and Struve has already (in 1857) advocated it in the Paris Academy. The opposite part of this greatest meridian passes over the icy deserts of North Western America and the group of islands known as the Marquesas, and hence afford no possibility of its completion on that side.

After this article was printed, the writer received the account of the New British Survey and notice of the correction of a small error of computation. This induced him to reprint some of the sheets on which the numerical results are given as above.

It will hardly be necessary to add anything in praise of the author. To bring up a question of such importance is sufficiently praiseworthy in itself, but the writer has done more; he has opened the way to a final determination and has given it, at least in its general aspect. We shall rejoice if this interesting subject calls other powers into the field to labor further in this direction. Corrections like those which the writer has made for Dorpat and Warsaw will be required for many other places, and each comparison of this kind furnishes its share to the final determination.

MÄDLER.

ART. VII.—*On the Transit Instrument as a substitute for the Zenith Telescope in determining Latitude, and on the Latitude of New Haven;* by Prof. C. S. LYMAN.

THE method of determining latitude by measuring micrometrically the difference of meridional zenith distances of stars on opposite sides of the zenith, known commonly as Talcott's method, strongly commends itself to observers, both by its beautiful simplicity, and the very great accuracy of its results. It is the method of late years exclusively used in the operations of the United States Coast Survey, and may properly be regarded as one of the many excellent incidental fruits of that great National work. A special instrument, however, the zenith telescope, has been required for making the necessary observations, and sup-

* It may be stated in connection with this subject that the geodetic surveys of two arcs, one in the New England States, the other on the Chesapeake Bay, are already completed and the astronomical part of the former is nearly so, promising an important result derived from the labors of the U. S. Coast Survey at no distant time.

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posed to be indispensable for this purpose. It indeed admirably answers the end for which it was invented, and in respect to simplicity, convenience, and efficiency, leaves almost nothing to be desired. But being expensive, and of limited use, it is likely to be in the hands of but few observers. The suggestion, therefore, of additional instrumental facilities for using this method of latitude, so as to render it more widely available, may not be considered as without value.

At the recent meeting of the American Association, at Springfield, the writer, in a brief communication, pointed out the facility and efficiency with which a transit instrument may be used as a substitute for the zenith telescope, in such observations. The object of the present article is, in part, to exemplify further the same point, and at the same time to place on record the results of a more careful determination, astronomically, of the latitude of New Haven, than, so far as appears, has heretofore been attempted.

The essential parts of the zenith telescope, as an instrument for determining latitude by Talcott's method, are its level and micrometer. And the superiority of the method itself rests mainly on the fact that its results depend on these simple and efficient instrumental means, instead of on graduation. It is obvious, that a transit instrument with a declination micrometer and a suitable level attached to its finding circle, is, at the same time, essentially a zenith telescope, and is capable of performing the whole work of that instrument—as accurately, if of corresponding size and quality, and as conveniently, if furnished with a reversing apparatus. The advantages of such a use of the transit instrument, in a multitude of cases, scarcely need to be pointed out. Where both latitude and longitude are to be determined at the same station, as is usually the case, a single instrument will suffice for both, and thus half the ordinary equipment for the purpose may be dispensed with. The corresponding diminution of expense will be a consideration of weight where pecuniary means are limited, as will also the saving of transportation, in the case of boundary and other surveys in remote and uninhabited districts. The observer who has at command such an instrument, even of very moderate size, will have it in his power to fix his latitude with a degree of precision scarcely surpassed even in first class observatories.

The transit instrument with which the observations were made, the results of which are given in this article, has an object glass (by Fitz) of two and six-tenths inches aperture, and thirty-five and a half inches focal length; a filar-micrometer, so constructed as to be used with equal facility either in the plane of the meridian or perpendicular to it; and a twelve inch circle, (graduated on the new and excellent engine of Messrs. E. & G. W.

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Blunt, of New York,) reading by two verniers to 10″. The level attached to the alidade of the circle has a run of one-thirteenth of an inch for 5″; and this being the smallest division of the scale, single seconds and fractions have to be estimated by the eye—a defect which will be further noticed in another connection.

The graduation of the circle is very accurate. The difference between the readings of the opposite verniers seldom amounts to to 15″, and from its law of variation, it is manifestly due, in the main at least, to a very slight excentricity in centering the circle on the graduating engine, not to imperfection in the engine itself.

The micrometer-screw has 75 threads to the inch, or one revolution of the head equal to 78″·74.

The optical performance of the instrument is very satisfactory. Transits of Polaris are readily taken at midday, and in favorable states of the atmosphere, at that hour, even by reflection in mercury. Transits of Aldebaran have been taken when the star was within 6° of the sun's center. The eye-piece commonly used is a diagonal one of four lenses, with prismatic reflector,—the magnifying power about 190.

The method of observing for latitude with the transit instrument is of course essentially the same as with the zenith telescope. A pair of stars having been selected, which culminate within a few minutes of each other, but on opposite sides of the zenith, and having their zenith distances nearly equal, the instrument is set for the star first to culminate, the level clamped, the bubble brought to zero by the slow-motion screw which turns the whole instrument, the star bisected with the micrometer wire, and the readings of the level and micrometer noted. The axis is then reversed, the bubble brought to zero as before, and the other star observed in the same manner.

The pairs for the observations now to be considered, were taken from the Catalogue of the British Association. The stars are to the sixth magnitude, and mostly within 25° of the zenith, the difference of zenith distances of the stars of a pair being usually less than 25′—averaging about 12′.

The individual results of 92 pairs are exhibited in the following table, which includes, with two exceptions, all the observations made, whether in favorable or unfavorable states of the atmosphere. In a few cases where the B. A. C. differs widely from other catalogues, the B. A. C. results are enclosed in brackets [ ] and are omitted in computing the mean. That the fault is in the B. A. C., is obvious from the accordance of the corresponding results from other catalogues with the general average.

The contents of the table are as follows:

Col. 1 and 2. The numbers from the British Association Catalogue designating the stars of a pair.

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3. The dates of the observations.

4. The mean results for latitude from each pair, using the positions of the B. A. C.; the seconds of the individual results, where the same pair was observed on different evenings, being placed at the left hand side of the column, and connected with their mean by a brace.

5. The differences, Δ, between these mean results of pairs and the mean of the whole.

6. The results for latitude from the same observations, the position of one or both the stars of the pair being taken from some other catalogue than the B. A. C.; the arrangement as in column 4.

7. The names of these other catalogues; G. standing for Greenwich Twelve Year Catalogue; B. that of the British Association; R. the catalogue in Radcliffe Observations for 1856; and A. the English Nautical Almanac. A. G., for example, signifies that the position of the first star of the pair is from the Nautical Almanac, and that of the other from the Twelve Year Catalogue; etc.

8. The differences between the results in column 6 and the mean of the whole.

Number of each Star in B. A. C.		Date of observation.	Lat. by B. A. C.		Δ	Lat. by other catalogues.		Name of catalogue.	Δ
			41° 18′			41° 18′			
121	164	1858, Nov. 8	43″·4	43″·7	+1·3	42″·3	42″·6	B. G.	+0·2
"	"	" " 11	44 ·1			43 ·0			
155	219	" " 10		40·0	−2·4		39·8	G. B.	−2·6
245	334	" " 11		42·1	−0·3		42·0	B. G.	−0·4
267	314	" Dec. 16		45·3	+2·9		45·3	B. G.	+2·9
334	487	" Nov. 10	42·1	42·5	+0·1	41·7	42·2	G. G.	−0·2
"	"	" " 11	43·0			42·7			
1083	1123	1859, Jan. 20		45·0	+2·6		43·0	R. G.	+0·5
1175	1214	" " "		40·7	−1·7		42·1	B. R.	−0·3
1313	1367	" " "		43·5	+1·1		42.0	R. R.	−0·4
3523	3777	" Oct. 21		41.0	−1·4		40·2	A. A.	−2·2
3742	3767	" May 6		42·6	+0·2		42·4	B. G.	0·0
3767	3809	" " "		42·6	+0·2		42·4	G. B.	0·0
3856	3913	" " "		41·8	−0·6		42·2	G. R.	−0·2
3968	3995	" " "		40.8	−1·6		40·5	B. G.	−1·9
4112	4151	" " "		43·9	+1·5		43·0	G. G.	+0·5
4216	4240	" " "		42·7	+0·3				
4351	4371	" " "		42·8	+0·4				
4371	4406	" " "		42·9	+0·5		42·1	B. G.	−0·3
4789	4823	" June 30		42·8	+0·4		45·2	G. R.	+2·8
4980	5036	" " "		44·0	+1·6		43·9	G. R.	+1·5
5131	5181	" July 5		41·5	−0·9				
5245	5285	" June 30	40·5	40·3	−2·1	41·0	40·8	R. A.	−1·6
"	"	" July 5	40·1			40·6			
5285	5309	" June 30		40 9	−1·5		40·8	A. B.	−1·6
5336	5388	" " "		43·1	+0·7		44·2	B. R.	+1·8
5406	5490	" July 5		41·5	−0·9				
5417	5460	" " 6		40·7	−1·7				
5460	5523	" June 30		43·7	+1·3		42·8	B. G.	+0·4

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Number of each star in B. A. C.		Date of observation.	Lat. by B. A. C.		Δ	Lat. by other catalogues.		Name of catalogue.	Δ
			41° 18′			41° 18′			
5449	5604	1859, June 30	43″·4	43″·7	+1·3	43″·6	48″·9	B. G.	+1·5
"	"	" July 5	44″·1			44 ·3			
5525	5560	" " 6		43·8	+1·4		42·6	R. R.	+0·2
5643	5703	" June 30	40·7						
"	"	" July 5	41·9	41·5	−0·9				
"	"	" " 6	41·9						
5703	5752	" June 30		[47·2]	+[4·8]		42·7	B. R.	+0·3
5747	5776	" July 5	40·8			40·5			
"	"	" " 6	39·5	40·2	−2·2	39·2	39·9	G. B.	−2·5
5776	5842	" " 5	41·4						
"	"	" " 6	42·4	41·9	−0·5				
5841	5887	" Aug. 16		40·5	−1·9				
5951	6021	" July 5		42·9	+0·5		42·4	G. A.	0·0
6033	6079	" Aug. 16	[56·1]			43·8			
"	"	" " 29	[57·2]	[56·7]	+[14·3]	44·9	44·4	R. R.	+2·0
"	"	" Sept. 1	[56·7]			44·4			
6150	6185	" Aug. 29		42·3	−0·1		41·9	G. B.	−0·5
6162	6193	" Sept. 1		43·1	+0·7				
6178	6246	" Aug. 16		43·1	+0·7		41·7	G. G.	−0·7
6185	6238	" " 29		41·5	−0·9				
6223	6289	" Sept. 1	40·7			41·5			
"	"	" " 9	40·9	40·8	−1·6	41·7	41·6	G. G.	−0·8
6289	6322	" Aug. 29	43 0			43·2			
"	"	" Sept. 7	44·1	43·5	+1·1	44·3	43·7	G. G.	+1·3
6404	6473	" Aug. 16	41·2						
"	"	" Sept. 9	41·2	41·2	−1·2				
6427	6470	" Aug. 29		43·8	+1·4				
6520	6556	" " 16	43·6			41·9			
"	"	" " 29	45·4	44·4	+2·0	43·7	42·7	R. B.	+0·3
"	"	" Sept. 1	44·3			42·6			
6583	6637	" Aug. 16	42·4			42·3			
"	"	" Sept. 9	44·8	43·6	+1·2	44·7	43·5	G. B.	+1·1
6623	6648	" " 1	41·0			41·9			
"	"	" " 26	43·2	41·7	−0·7	44·0	42·6	G. G.	+0·2
"	"	" Oct. 4	41·0			41·9			
6662	6744	" Sept. 7		43·2	+0·8		43·7	G. B.	+1·3
6674	6712	" Aug. 29	[51·5]			43·7			
"	"	" Sept. 9	[50·1]	[50·8]	+[8·4]	42·3	43·0	G. R.	+0·6
6690	6748	" Aug. 16	42·5			42·0			
"	"	" Sept. 26	42·5	42·5	+0·1	42·0	42·0	G. B.	−0·4
6709	6737	" " 1		41·6	−0·8				
6731	6765	" Oct. 4		44·5	+2·1				
6741	6784	" Aug. 29	[38·5]			43·8			
"	"	" Sept. 9	[37·0]	[37·8]	−[4·6]	42·3	43·0	R. R.	+0·6
6779	6849	" " 26		41·5	−0·9		42·2	G. B.	−0·2
6799	6851	" " 7	42·1						
"	"	" Oct. 7	41·1	41·6	−0·6				
6827	6867	" Aug. 29	43·2			43·4			
"	"	" Sept. 9	43·1			43·3			
"	"	" Oct. 3	43·4	43·1	+0·7	43·5	43·3	B. R.	+0·9
"	"	" " 4	42·8			42·9			
6858	6930	" Sept. 1	42·4						
"	"	" Oct. 6	42·6	42·5	+0·1				
6893	6936	" Sept. 7	42·7			41·2			
"	"	" " 26	42·6	42·7	+0·3	41·2	41·2	G. B.	−1·2
6926	6952	" Aug. 29	41·4			41·6			
"	"	" Oct. 4	41·2	41·3	−1·1	41·4	41·5	G. B.	−0·9
6932	6975	" " 3		42·7	+0·3		42·4	R. B.	0 0
6937	6965	" " 7		45·3	+2·9		43·2	G. G.	+0·8

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Number of each star in B. A. C.		Date of observation.	Lat. by B. A. C.		Δ	Lat. by other catalogues.		Name of catalogue.	Δ
				41° 18′			41° 18′		
6965	6997	1859, Sept. 1	42″·6			42″·2			
"	"	" " 9	44 ·0	43″·5	+1·1	43 ·6	43″·1	G. B.	+0·7
"	"	" Oct. 7	43 ·8			43 ·4			
6959	7029	" Sept. 7		45·4	+3·0				
6963	7022	" " 26		44·4	+2·0		44·6	B. G.	+2·2
6996	7041	" Oct. 3		40·8	−1·6				
7037	7107	" Aug. 29		40·9	−1 5		40·3	B. G.	−2·1
7041	7114	" Oct. 3		39·8	−2·6				
7067	7153	" Sept. 7		40·5	−1·9				
7088	7124	" Oct. 4	40·3			39·8			
"	"	" " 7	39·9	40·1	−2·3	39·4	39·6	G. B.	−2·8
7119	7174	" Sept. 9		40·7	−1·7				
7246	7281	" " 7	42·4			42 5			
"	"	" " 9	43·6	43·0	+0·6	43·7	43·1	B. G.	+0·7
7253	7313	" " 26		42·7	+0·3		42·9	G. B.	+0·5
7377	7437	" " 9	43·1			43·8			
"	"	" Oct. 4	42·1	42·6	+0·2	42·7	43·3	G. B.	+0·9
"	"	" " 7	42·7			43 4			
7437	7476	" Sept. 9	42·0						
"	"	" Oct. 4	42·7	42·6	+0·2				
"	"	" " 7	43 0						
7554	7593	" Sept. 26	40·3						
"	"	" Oct. 3	41·6						
"	"	" " 4	42·5	41·2	−1·2				
"	"	" " 7	40·3						
7607	7642	'58, Nov. 22		40·0	−2·4		40·8	B. R.	−1·6
7627	7683	'59, Sept. 26	41·4			42·7			
"	"	" Oct. 3	40 3	40·7	−1·7	41·5	42·0	A. B.	−0·4
"	"	" " 4	41·1			42·4			
"	"	" " 7	40·0			41·3			
7627	7749	" Sept. 26	40.0			41·5			
"	"	" Oct. 3	40·1			41·6			
"	"	" " 4	40·3	40·0	−2·4	41·7	41·5	A. G.	−0·9
"	"	" " 7	39 8			41·2			
7699	7807	'58, Nov. 22		40·1	−2·3				
7777	7800	" " 18	41·6			41·5			
"	"	'59, Oct. 4	43·1	42·2	−0·2	43 0	42·1	B. G.	−0 3
"	"	" " 7	42·0			41·9			
7843	7888	'58, Nov. 18	[49·0]			43·8			
"	"	" " 22	[46·7]	[48·1]	+[5·7]	41·5	42·9	R. G.	+0·5
"	"	'59, Oct. 4	[48·7]			43·5			
7856	7902	" Sept. 26		41·7	−0·7		42·6	R. G.	+0·2
7961	8032	'58, Nov. 18	43·6			42·7			
"	"	'59, Oct. 4	43·4	43·5	+1·1	42·5	42·6	B. G.	+0·2
7962	8023	" " 18		43·1	+0·7		42·6	G. G.	+0·2
7994	8023	" " 3	43 9			42·9			
"	"	" " 18	43·1	43·5	+1·1	42·1	42 5	B. G.	+0·1
7990	8169	'58, Nov. 8		43 6	+1·2		43·4	G. A.	+1·0
8079	8158	" " 18		44·2	+1·8		42·9	B. R.	+0·5
8177	8238	'59, Oct. 13	43·0			43·3			
"	"	" " 18	41·6	42·3	−0·1	42·0	42·7	G. G.	+0·3
8212	8237	'58, Nov. 18	42·9			43·4			
"	"	" " 22	42·9	42·9	+0·5	43·4	43·4	G. G.	+1·0
8282	8324	" " 18		41·7	−0·7				
8355	8	'59, Dec. 30		45·5	+3·1				
α Cyg.	61¹ Cyg.	" Oct. 3 to Nov. 25, 20 obs.		42·7	+0·3		42·5	A. A.	+0·1
γ Ceph.	ω Pisc.	" Oct. 3 to Dec. 24, 21 obs.		41·8	+0·4		41·7	A. A.	−0·7

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If we divide these 92 pairs into four sets of 23 each, as indicated in the table by the lines after Nos. 5336, 6623, and 7119, the mean of each set respectively, and of the whole, will be as follows: column 1, giving the number of the set; col. 2, the number of results of pairs in the set by B. A. C.; col. 3, the mean of these results; col. 4, the number of results of pairs by other catalogues; col. 5, the mean of these; col. 6, the number of results by both B. A. C. and other catalogues taken together; col. 7, the mean of the same; col. 8, the probable error, *E*, of this mean; col. 9, the probable error, *E'*, of a single result for latitude.

	By B. A. C.		By other Catalogues.		By both B. A. C. and others.		E.	E'.
Set	Prs.	Mean.	Prs	Mean.	Num. of results.	Mean Lat.		
1	23	41°18′42″·46	20	41°18″42·″34	43	41°18′42″·40	±0″·15	±0″·97
2	21	42 ·35	14	42 ·60	35	42 ·45	±0 ·14	±0 ·82
3	21	42 ·32	14	42 ·36	35	42 ·34	±0 ·17	±1 ·02
4	22	42 ·35	18	42 ·53	40	42 ·43	±0 ·12	±0 ·76
All	87	41 18 42 ·37	66	41 18 42 ·45	153	41 18 42 ·41	±0 ·07	±0 ·88
	E	± 0 ·10		± 0 ·10		Mean of the whole.		
	E′	± 0 ·97		± 0 ·80				

We may regard 41° 18′ 42″·41, then, as a close approximation to the latitude of the point in New Haven, where the observations were made, the probable error of this result, as given by the observations, being less than a tenth of a second. In taking the arithmetical mean of the results for the several pairs, we assume, of course, for each of these results equal weights—no discrimination having been attempted between errors of observation and of star-places, or between different catalogues. Such discrimination, however, in so large a number of independent pairs, would not have materially changed the result.

It will be seen, from the above table, that any one of the four sets would have given a latitude almost identical with the mean of the whole—in no case differing from it so much as a tenth of a second, or ten feet on the earth's surface. That this extremely close accordance, however, of results from sets composed of so few pairs, is in a measure accidental, may be inferred from the larger probable errors of these results as given in column *E*. Had there been three sets, instead of four, the differences would have been somewhat greater, though still not exceeding a few tenths of a second.

The probable error of a single result for latitude, as given in the last column, (about nine-tenths of a second on an average), includes, it will be understood, the accidental error of observation, (depending on observer, instrument, state of the atmosphere, etc.), together with the error of the stars' places, as given in the catalogues. The experience of the observers of the Coast

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Survey with the zenith telescope, as stated by the superintendent, shows that usually the latter error very much exceeds the former, or that the catalogues are less reliable than the instruments. The same is apparent from our own results. While the probable error of a single result (including both error of observation and of catalogue) amounts, on an average to ± 0″·88, the probable error of observation, as distinct from that of catalogue, is found by comparing the repetitions on the same pairs in our first table, to be only ±0″·48. A similar value of this error is given by a comparison of the longer series of repetitions on the last two pairs of that table (α and 61¹ Cygni, γ Cephei and ω Piscium) as given below. Twenty observations were taken of the former pair, and twenty-one of the latter. The star-places used are those of the English Nautical Almanac.

Date of Obs.	α Cygni and 61¹ Cygni.	Δ	Date of Obs.	γ Cephei and ω Piscium.	Δ
1859.			'58, Nov. 8	41° 18′ 41″·2	−0″·5
Oct. 3	41° 18′ 42″·9	+0″·4	11	41 ·4	−0 ·3
4	42 ·0	−0 ·5	'59, Oct. 15	42 ·4	+0 ·7
7	41 ·9	−0 ·7	18	42 ·1	+0 ·4
10	41 ·9	−0 ·7	21	41 ·3	−0 ·4
11	43 ·7	+1 ·2	27	43 ·2	+1 ·5
15	41 ·8	−0 ·8	29	40 ·8	−0 ·9
18	42 ·7	+0 ·2	Nov. 1	42 ·0	+0 ·3
21	42 ·3	−0 ·2	3	41 ·4	−1 ·3
23	42 ·9	+0 ·3	14	43 ·3	+1 ·6
25	42 ·2	−0 ·4	15	40 ·6	+1 ·1
28	43 ·6	+1 ·0	23	42 ·4	+0 ·6
29	42 ·8	+0 ·2	28	41 ·7	0 ·0
Nov. 2	42 ·8	+0 ·2	Dec. 8	43 ·0	+1 ·3
3	43 ·4	+0 ·9	10	40 ·2	−1 ·5
4	43 ·0	+0 ·5	12	41 ·6	−0 ·1
9	43 ·2	+0 ·7	15	41 ·0	−0 ·7
14	43 ·0	+0 ·4	16	41 ·9	+0 ·2
17	41 ·7	−0 ·9	21	41 ·2	−0 ·5
19	42 ·0	−0 ·6	23	41 ·6	−0 ·1
25	41 ·3	−1 ·2	24	41 ·9	+0 ·2
Mean	41 18 42·55	± 0″·10	Mean	41 18 41·73	±0″·12
Prob. Er. of single result		± 0 ·47			±0 ·55

We have, then, for the probable accidental error of *observation*, from the repetitions on the same pairs in our first table, ±0″·48; from α and 61¹ Cygni, ±0″·47; from γ Cephei and ω Piscium, 0″·55; from all these combined, ±0″·50. This compared with the average probable error of a single result for latitude, ±0″·88, in which it is included, gives for the probable error depending on the catalogues used, ±0″·72. The relative accuracy, therefore, of instrument and catalogues, in the present case, may be represented approximately by the ratio of 5 to 7. That the Twelve Year and other catalogues used are, on the whole, more accurate than that of the British Association, appears from the values of *E* in the last line of the table on page 58; viz. ±0″·97 for B. A. C., and ±0″·80 for the other catalogues; although in the column for

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the latter, as may be noticed, many of the positions for one of the stars of a pair, are also from B. A. C. If we separate from these values the probable error of observation, $\pm 0''.50$, before stated, we shall have for error depending on the catalogues, $\pm 0''.82$ for B. A. C., and $\pm 0''.63$ for the others. This result accords well with the remark of Prof. Bache, in a paper published in the 12th volume of the Proceedings of the American Association, that "the probable error of the result by a single pair of stars as depending on the catalogue errors of the stars' places, will range from $\pm 0''.60$ to $\pm 1''.00$."

In the same paper it is stated also, that "a probable error in observing of from $\pm 0''.25$ to $\pm 0''.80$ may be expected for one observation, according to the size and quality of the instrument and the ability of the observer." The average value of this error for some 300 pairs of stars, observed with the excellent instruments and by the experienced observers of the Coast Survey, is, according to the same paper, $\pm 0''.47$—ranging from $\pm 0''.25$ to $\pm 1''.14$. The value of the corresponding quantity in our own results, is, as we have seen, $\pm 0''.50$, ranging from $\pm 0''.47$ to $\pm 0''.55$. A considerable portion of this is due, doubtless, to the want of sufficient sensitiveness in the alidade level of the instrument; the run of which, as before stated, is only one thirteenth of an inch for 5″, smaller portions of the scale than this being measured by the eye. This level, we may remark, was intended originally only to be used in the ordinary way, in connection with the graduated circle, and is sufficiently sensitive for that purpose; but not enough so to correspond, in point of accuracy, with the micrometer,* or for the delicacy required in observations for latitude with the zenith telescope. The use of the instrument for such observations was wholly an after thought, and no change was made to adapt it to its new functions. It is of smaller size also than the zenith telescopes of the Coast Survey, and it may be proper to add, that, being of the observer's own make, and a first attempt of the kind, some allowance is due in a comparison of its results with those of more finished instruments in the hands of trained observers.

In order to compare the latitude, thus determined astronomically, with the geodetical determination of the Coast Survey, a survey was made with theodolite and chain connecting the writer's observatory with three points in the city, the positions of which are given in the Coast Survey Report for 1851, viz., "Brewster's Factory Cupola" at the foot of Wooster street, "Episcopal church," which means the west spire of St. Paul's,

* Sixty successive bisections of Polaris, near culmination, with the micrometer wire, gave for probable error of a single bisection, $\pm 0''.30$; which would make the probable error of a single result for latitude (involving two bisections) as depending on the micrometer alone, $\pm 0''.21$.

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and "College Spire," which is that of the Lyceum, or middle spire. The results are as follows.

	Lat. by Talcott's method.	By Coast Survey.	Diff.
Observatory of the writer,	41° 18′ 42″·41		
Factory Cupola, East Street,	3·29	41° 18′ 7″·10	3″·81
St, Paul's Church, W. Spire,	7·52	11·32	3·80
College Observatory, (Athen. Tower),	22.36		
"College Spire," (Lyceum),	23·90	27·74	3·84
Chapel Spire,	25·49		

Mean excess of geodetic over astronomical lat. 3″·82.

It may be added that a series of observations made in 1850, with an excellent Pistor and Martins' Patent Sextant, on northern and southern stars, and also a series of observations made in 1858, in the usual way, with the circle of the Transit instrument before described, on Polaris and other stars, both direct and by reflection, and in reversed positions of the instrument, gave results differing but about half a second from the above determinations, the former minus, the latter plus.

The differences among results for latitude, depending on differences of catalogues, are often considerable, even when the best authorities only are trusted; as will be seen by comparing the mean results, given below, of the observations on α and 61^1 Cygni, and γ Cephei and ω Piscium, according as the declinations are taken from the several catalogues named.

	α and 61^1 Cygni.	γ Cephei, ω Piscium.
English Nautical Almanac,	41° 18′ 42″·5	41° 18′ 41″·7
Twelve Year Catalogue,	42·6	41·4
B. A. C.,	42·7	41·3
Greenwich, 1850,	42·8	41·6
Washington, 1845–1850,	42·7	
Radcliffe, 1856, '57,	41.9	
Bradley,		40·8
Piazzi,		40·7
Argelander,		41·1
Mädler,		41·8
		41·4

It will be noticed, also, in respect to these two pairs, that there is a greater difference between their respective mean results, than we should expect from well determined nautical almanac stars. This difference between the means of the twenty and twenty one observations respectively on the two pairs, using the nautical almanac declinations, amounts to 0″·8, an amount difficult to attribute to any errors of instrument or of observation, and equally difficult, perhaps, to ascribe to errors of stars' places, especially as the several catalogues so well agree with each other. From whatever source it may arise, it seems to be constant, and will receive further investigation.

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ART. VIII.—*On Fixing Magnetic Phantoms;* by Prof. J. NICKLÈS.

THE name phantom was given by M. de Haldat* to the figures which are obtained when iron filings are thrown upon a sheet of paper or a pane of glass placed over a magnet. This physicist fixed these images by producing them upon a sheet of paper coated with starch or prepared with gelatine.

This process certainly enables us to obtain the general form of the phantoms, but all physicists can see that it suppresses the details. I was more particularly struck with this fact on a recent occasion, where I sought to fix the phantoms of some new electro-magnetic combinations; I therefore propose another method, which is here briefly given; it is very simple and succeeds perfectly. The paper upon which the phantoms are to be fixed is "waxed" paper. A sheet of this is placed over the poles of the magnet in question, and kept in a horizontal position by means of a screen placed between the paper and the magnet. Then proceeding in the usual manner, when the image is fully developed, a hot brick is held above it, or the warm lid of a crucible, which is preferable because it is lighter, and easily managed with the tongs. They must not touch the paper, but only be brought within the distance necessary to fuse the wax. As soon as this happens, which is easily perceived by the glistening appearance produced, the brick is withdrawn. Meanwhile the current does not cease its activity, nor the filings lose their arrangement, in which position the whole solidifies so well that the fixed image does not at all differ from the phantom of the magnet in activity.

This result is explained as follows. By capillarity the melted wax penetrates the masses of filings, very much as water penetrates a heap of sand; the heat of the brick facilitates this, by preventing the solidification of the wax, and as the temperature is not sufficiently elevated to sensibly affect the magnetism developed, the phantom, after the solidification, and in the most minute details, preserves the same arrangement which the iron filings had, while they were free to obey the action of the magnet.

A condition indispensable to success, is, that the stratum of wax has a sensible thickness, so that it may suffice for the agglomerations, since these absorb melted fatty matter, even to saturation. That this force of absorption is very energetically exercised, may be perceived after the cooling, since the paper about the agglomerations is deprived of wax, and differs thus in appearance from those parts where capillarity has not been exercised. It is therefore possible to preserve to the phantoms the

* Memoir before the Academy of Stanislas, p. 42, for the year 1839—1840.

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relief which has before been sought in vain, and what will be still more useful, to give permanence to the sort of molecular arrangement which the filings take, when exposed to magnetic influence.

Instruction can hardly fail to be derived from the use of these means, by aid of which it will be possible to study the figures more advantageously, which are, in some sense, the visible expression of the force animating bodies endued with polarity developed by magnetism.*

Nancy, March, 1860.

ART. IX.—*On some Questions concerning the Coal Formations of North America;* by LEO LESQUEREUX. Continued from Vol. xxviii, p. 21.

Geographical Distribution of the Coal Flora.

To follow the plan exposed in the first part of this memoir (Silliman's Journal, No. 82, July, 1859, p. 21) I should have to examine now the nature of the coal-flora, that is, the anatomical and chemical constitution of the coal plants, first comparing between themselves the groups of species of plants from which the matter of the coal is a compound, and then examining how this vegetation is related to the plants living at this epoch. The distribution of the coal plants, either geographical or stratigraphical, is a question accessory to the former. Nevertheless, it has become now of a greater importance, since it touches upon a problem which is at present discussed by the authority of the highest scientific names. I allude to the theory *of the origin of species* by Mr. Darwin. It evidently concerns the great problem of the inmost nature of man, and thus forces every naturalist to seek, in the sum of facts gathered up by his researches, either confirmatory or contradictory evidence of views which cannot but preoccupy his mind. Thus it is apparently advisable to change the order of examination of the flora of the coal measures of North America, studying it now in its stratigraphical and geographical distribution, and leaving for another opportunity the discussion concerning the nature of its vegetation and the specific and generic value of its representatives.

* This note is extracted from a work now in press and which will soon appear, entitled, *Electro-magnets and Magnetic adhesion*, and accordingly treats of electro-magnets and their application to locomotion upon railways, with the design of increasing the adhesion of locomotive engines. To the experiments which we made public in 1853 (v. this Jour., [2], vol. xvi, p. 337), we shall add new ones which allow us to expect a speedy solution of this problem, so important to railroads.

In regard to electro-magnets, we will only say at present, that we shall mention a great number of new species, which have compelled us to invent a new classification and a nomenclature based upon the principles of the natural method. We have also to make known a great number of new facts respecting the laws and properties of electro-magnets, to say nothing of what we wrote upon this point in 1853 (v. Am. Jour., [2] vol. xv, p. 381, and vol. xx, p. 100). J. N.

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I shall not attempt in any way either an exposition or a critical examination of the views of the celebrated English author. This task has already been admirably fulfilled in a former number of this Journal.* I shall merely expose the facts that appear surely ascertained by a long and careful exploration of the coal-fields of North America, leaving the naturalist-philosopher to take from these facts any conclusion that may appear just to him. It is a mite only. But the monuments of humanity, like the mountains of limestone, are built by the slow accumulation of minute remains.

The botanical palæontology of the coal-period and the succession and variation of species in the different strata of the coal-measures, cannot be studied with more advantage and with more chances of reliability than in the coal-fields of the United States. Their immense area, the uniformity of their generally unbroken stratification, the facility of ascertaining in many localities the order of this stratification, the numerous exposures of coal banks, not limited to a particular district, but opened and worked now at distant points over the whole area of the coal-fields; all this affords to a systematic exploration such advantages as cannot be found in any other country of the world.

Moreover, my explorations of the coal-fields of North America have been favored by peculiar circumstances. Connected at different times, during ten years, with the geological surveys of the states of Pennsylvania, Kentucky, Indiana, Arkansas and Illinois; called to survey for comparison the coal-fields of Ohio and part of those of Virginia; constantly limiting my researches to botanical palæontology, I have thus, I suppose, collected on the distribution of the coal-plants in North America, more materials and more reliable accounts than geologists may be able to get at for a long time to come.

The first important question in regard to the coal-plants of America is: what is their relation of forms with the plants of the same formation in Europe? The comparison of the coal flora of both continents has never been made except on few and insufficient data.†

Except two, all the genera of fossil plants of the coal-fields of America are represented in Europe. One of them is the remarkable *Whittleseya elegans* of Dr. Newbury, a flabellate, apparently short pedicillate, cuneiform-oval and truncate leaf, found hitherto always detached from the stem. It evidently differs from the genus *Cyclopteris* by its simple straight nervation and by its up-

* Vol. xxix, p. 1.

† I have attempted it formerly, in my palæontological report of the Geological State Survey of Penn., delivered January 1st, 1854, but only published five years afterwards. As I have been denied the privilege of reviewing and correcting my manuscript before its publication, I cannot consider myself accountable for the errors, and especially for the deficiency of conclusive data, which may be found in it.

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per margin being horizontally truncate and regularly wavy-denticulate by the percurrent and slightly emerging nerves. The typical affinity of this plant is unknown. It is perhaps more related to *Cordaites* or even to *Salisburia* than to a fern.

The second genus peculiar to the American coal-flora is my *Scolopendrites*, represented, like the former, by a single species; *Scolopendrites dentata* Lsqx., of which fragments only have been found. The name has no relation to the nervation of the leaf, but to its outline. This leaf is apparently five to six inches long, more than an inch broad, lanceolate, deeply cut by obtuse somewhat regular teeth and marked by a few very thin distant nervules, emerging from a narrow medial nerve in an acute angle, scarcely arched and forking twice. Both these genera represent peculiar types to which no form of the European coal-flora can be compared. I could also mention as a peculiar type of our coal-flora *Crematopteris Pennsylvanica* Lsqx., a thick stem or branch, on both sides of which short, lanceolate, obtuse, thick leaflets, without any trace of nervation, are pinnately attached. The single specimen which I have found of this plant was not well preserved enough, and it would be unsafe to consider the species which it represents as having been seen in its true form and full development.

If we admit the generic distribution of the fossil plants of the coal, as it has been established by Brongniart in his *Tableau des Genres,* (certainly the best that has been attempted either before or after him) all the European genera, even the undefined genus *Aphlebia*, (Sterbg.) have representative species in the coal-fields of America. From the nomenclature of Göppert and Corda, a few European genera, it is true, are hitherto without representative species in our coal-fields. But these genera, established on the form and the position of fructification, always very difficult to identify from even the best preserved specimens of fossil ferns, may be represented by some species of our *Sphenopterideæ* and *Pecopterideæ*. These genera, viz., *Rhodea* Sternb., *Trichomanites* Göpp., *Steffensia* Göpp., *Beinertia* Göpp., *Diplaxites* Göpp., *Woodwartites* Göpp., are no peculiar types. Thus, considering their generic distribution, the coal-plants of Europe and of North America show very little difference indeed. But in examining the species separately and comparing them on both sides of the Atlantic, the number of forms peculiar to America appears much larger than it was at first supposed, and thus the vegetation of our continent, at the epoch of the coal formation and considered in its whole, is far more different than it has been supposed heretofore.

It would be too long and tedious, perhaps, to take one by one and compare all the American species with those of Europe,

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mentioning the identity, the relation, or the difference of each of them. I will therefore give in a table the number of species of each genus, belonging either to North America or to Europe, or common to both continents, and complete this general view by a few remarks on some of the species, apparently the most interesting, by predominance of number, of typical form or by a peculiar distribution.

Genera of Coal plants.	Species peculiar to America.*	Species peculiar to Europe.	Species common to both.
1. Noeggerathia Sternb.,	3 + 2*	5	1
2. Cyclopteris Brgt.,	1	2	2
3. Nephropteris Brgt.,	5	4	4
5. Neuropteris Brgt.,	18 + 1*	16	12
6. Odontopteris Brgt.,	4 + 1*	6	3
7. Dictyopteris Gutb.,	1	1	0
8. Sphenopteris Brgt.,	10 + 9*	41	12
9. Hymenophyllites Göpp.,	6	10	2
10. Rhodea Sternb.,	0	1	0
11. Trichomanites Göpp.,	0	4	0
12. Steffensia Göpp.,	0	1	0
13. Beinertia Göpp.,	0	1	0
13. Diplaxites Göpp.,	0	2	0
14. Woodwartites Göpp.,	0	2	0
15. Alethopteris Sternb.,	9 + 2*	20	9
16. Callipteris Brgt.,	2	1	1
17. Tecopteris Brgt.,	12 + 4*	49	12
18. Aphlebia Sternb.,	0	6	1
19. Caulopteris Brgt.,	4	4	0
20. Psaronius Brgt.,	10	6	0
21. Crematopteris Schp.,	1	0	0
22. Scolopendrites Lsqx.,	1	0	0
23. Whittleseya Newb.,	1	0	0
24. Cordaites Ung.,	1	0	2
25. Diplothegium Corda,	0	0	1
26. Stigmaria Brgt.,	5	2	5
27. Sigillaria Brgt.,	12 + 9*	37	17
28. Syrigodendron Brgt.,	0 + 1*	0	2
29. Diploxylon Corda,	0	0	1
30. Lepidodendron Brgt.,	14	10	11
31. Ulodendron Rhode,	0	4	2
32. Megaphytum Artis,	1 + 1*	4	0
33. Knorria Sternb.,	2 + 2*	1	2
34. Halonia Ll. & Hutt.,	0	2	1
35. Lepidophyllum Brgt.,	7	2	4
36. Lepidostrobus Brgt.,	?	1	2

* In this enumeration, I count the species named as new ones in a catalogue published in 1853 by Dr. Newberry (Nos. 8th and 9th of the Annals of Science of Cleveland). As the species have not been described and did not come under my examination, they are still doubtful and separately marked by a *.

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Genera of Coal plants.	Species peculiar to America.	Species peculiar to Europe.	Species common to both.
37. Cardiocarpon Brgt.,	2 + 5*	6	0
38. Trigonocarpum Brgt.,	3 + 3*	5	5
39. Rhabdocarpos Göpp. & Brgt.,	1 + 1*	6	1
40. Carpolithes Sternb.,	12 + 1*	52	6
41. Selaginites Brgt.,	0	1	0
42. Lycopodites Brgt.,	1	12	0
43. Lomatophloios Corda,	1	0	1
44. Lepidophloios Sternb.,	0	0	1
45. Bothrodendron Göpp.,	0	1	0
46. Cycadloidea Buckl.,	0	1	0
47. Calamites Suck.,	2	5	11
48. Bornia Sternb. & Göpp.,	1	1	0
49. Equisetites Sternb.,	0	2	1
50. Asterophyllites Brgt.,	5	8	7
51. Annularia Sternb.,	0	0	5
52. Sphenophyllum Brgt.,	2 + 3*	3	3

Noeggerathia Sternb.—Two of the American species are closely related to *N. obliqua* Göpp. A third, *N. Bocksiana* Lsqx., which I referred to *Cyclopteris Bocksiana* Göpp., from the exact likeness of its terminal leaflet with the separate and only leaf on which the author has established his species, is evidently referable to the genus *Adiantites* Göpp. The two species of true *Noeggerathia* belong to the Old Red Sandstone of Pennsylvania; the third found at the base of the millstone grit of Pennsylvania belongs by its typical form to the true coal measures.

Nephropteris Brgt.—Cannot be separated from the genus *Neuropteris*, since it represents large, mostly rounded and deciduous leaflets attached around the stem at the base of secondary pinnæ of some *Neuropteris*. Two of our American species, viz., *Nephropteris fimbriata* Lsqx. and *Nephropteris laciniata* Lsqx. have a typical character which has never been seen on any of the fossil ferns of Europe. As the names indicate, both these beautiful species of leaves are fringed and laciniate on their circumference. The fringes and laciniæ are unequal in length and breadth, flexuous, and do not bear any likeness to the straight and regular points surrounding the fruit-bearing leaves of some ferns of our time.

Neuropteris Brgt.—Our *Neuropteris hirsuta* Lsqx. is, probably at least, the equivalent of *Neuropteris cordata*, *N. Scheuxeri*, *N. angustifolia* and *N. acutifolia*, species of M. Brongniart. A peculiar character of this species, viz., the long, straight, diffuse hairs of its upper surface may help to bring together the numerous and diverse forms of its leaflets, which are generally separated from the stem. Mr. Bunbury had already remarked and mentioned those hairs. I have not been able to detect them on the few, badly preserved specimens of the European *Neuropteris cor-*

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data Brgt. which I had opportunity to examine. It may be also that *Neuropteris smilacifolia* Sternb., *N. plicata* Sternb., *N. rotundifolia* Brgt., ought to be referred as varieties to *Neuropteris flexuosa* Brgt. Nevertheless, we have these same scarcely distinct species with just the same characters in America. They are mostly found in the same places where *Neuropteris flexuosa* abounds. Of the other American species of this genus, *Neuropteris Clarksoni* Lsqx. is closely allied to *Neuropteris auriculata* Brgt., and *Neuropteris Desorii* Lsqx., to *Odontopteris Reichiana* Gutb. But *Neuropteris speciosa* Lsqx., *N. rarinervis* Bunby., *N. gibbosa* Lsqx., *N. undans* Lsqx., *N. dentata* Lsqx., *N. Morii* Lsqx. are widely different from any European species, and may be considered as true American forms or types. *Neuropteris adiantites* Lsqx. should perhaps be referred, by its peculiar nervation, to the genus *Sonopteris* of M. Ponsel.

Odontopteris Brgt.—I have counted *Odontopteris Brardii* Brgt., among the species belonging to both continents, on the authority of M. Unger, who indicates it at Mauch-Chunk, Pennsylvania. Nevertheless I doubt of the identity of our species with the true *O. Brardii* Brgt. The small specimen which I formerly referred to it belongs to another species, *Odontopteris crenulata* Brgt., and I would rather suppose that some incomplete specimen of *Odontopteris alata* Lsqx., has been mistaken for the true *O. Brardii* Brgt. Our species, though closely related to the European form, evidently differs from it by its obtuse leaflets, and by the position of two large, opposite, cuneate, truncate leaflets attached to the rachis just below the base of the pinnæ.—*Odontopteris Schlotheimii* Brgt., common to both continents, is one of the few species which, either in its fructified or sterile form, show a perfect identity at any locality where it is found.

Dictyopteris Gutb.—The affinity of the European *Dictyopteris Brongnarti* Gutb. with our *Dictyopteris obliqua* Bunby., is so great that these species could be considered as mere varieties of the same. *D. obliqua* Bunby. has narrower, less obtuse leaflets. These are generally found detached from the stem and spread over the shales in the greatest abundance.

Sphenopteris Brgt.—This genus, to which I have united the *Gleichenites* of M. Göppert, appears from the number of its species, to be far more abundantly represented in Europe than in America. Probably the difference may be accounted for, first by the number of European species which, established on small specimens, represent variable parts of a frond and may be recalled to a common species. Secondly by the insufficiency of our explorations. Since the publication of my catalogue of fossil plants of the coal, in 1858, ten new species of *Sphenopteris* not counted in the table, because they are still undescribed, have come under my examination. From the number of species enu-

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merated by Dr. Newbury it would appear also that this genus is largely represented at some places and in restricted areas. Thus we may expect to see the number of American species largely increased.

The essential European types of this genus are all represented in our coal measures, either by identical species, or, by closely related forms. Thus we have: *Sphenopteris spinosa* Ll. and Hutt., *S. tridactylites* Brgt, *S. Davalliana* Göpp., *Sphenopteris irregularis* Gutb., *S. Gravenhorstii* Brgt., *S. Dubuissonis* Brgt., *S. latifolia* Brgt., *S. polyphylla* Ll. and Hutt., *S. artemisiæfolia* Sternb., &c. Besides, as American types, we have *Sphenopteris flagellaris* Lsqx., and especially *Sp. Newberrei* Lsqx. This last species has a ramification different from that of any other kind of fossil fern, viz., two secondary pinnæ, joined at their base and forking in an obtuse angle at the top of a short naked stem. The form of the leaflet and the nervation of this species is also peculiar. One of my species, *Sphenopteris decipiens* Lsqx. has, like *Neuropteris adiantites* Lsqx., the primary nerve folded along the base of the leaflets and the nervules curved and branching upwards, a character ascribed by Brongniart to the genus *Lonopteris* Tom.

Hymenophyllites Göpp.—From analogy of nervation, rather than from a relation of typical form, I have, perhaps wrongly, connected with this genus, some species referable either to *Aphlebia* Sternb., or to *Schizopteris* Brgt., or even to *Selaginites* Brgt. Four American species: *Hymenophyllites fimbriatus* Lsqx., *H. affinis* Lsqx., *H. hirsutus* Lsqx., *H. laceratus* Lsqx., are represented by fronds which form a broad base, divide in ascending, and are thus irregularly cut in simple, ordinarily short, curved, somewhat obtuse laciniæ or lobes. Each of the divisions is marked by a single nerve, ascending to its top. The fronds appear generally of a thick texture; but in *H. giganteus* Lsqx., which may be the same plant as *Schizopteris lactuca* Sternb., they are seemingly very thin. These species ought to constitute a separate genus. In the fossil flora of Pennsylvania I had attempted to group them together under the name of *Pachyphyllum;* but as some species have apparently thin leaves or fronds, the name could not be preserved of course. A discussion concerning the morphology of the plants of the coal would be out of place now. The subject ought to be separately treated. I needed only to mention these peculiar forms, for comparing the distribution of the genus *Hymenophyllites.* Two of our American species are closely related to the *H. elegans* Brgt. of Europe. The others, especially *H. fimbriatus* Lsqx., are apparently peculiar American types. This last, nevertheless, could be compared to *Selaginites Erdmani* Gutb.

Alethopteris Sternb.—All our American species have some affinity with European types of the same genus. Even the re-

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markable *Alethopteris serrula* Lsqx. is related, though distantly, to *A. erosa* Gutb., from which it differs, especially by its very long (sometimes 4 to 5 inches), narrow, linear pinnæ. It has been found only near Pottsville, Pennsylvania.

Callipteris Brgt.—One of the American species, *C. Sullivantii* Lsqx., is a beautiful fern of a peculiar character, apparently bi- or tri-pinnate, but of which pinnæ only have been found. The general outline of the pinnæ is like that of *Alethopteris Serlii* Brgt. But the pinnules are twice as broad, rounded at the top, marked by a broad and deep medial nerve, abruptly terminating in the middle of the leaflets, and from which emerge in acute angles, thin arched, forked nervules as closely placed as those of *Neuropteris flexuosa* Brgt. The other American species is closely related to *Alethopteris sinuata* Brgt. *A. nervosa* Brgt. is a common species over the whole extent of our coal fields. As it is generally found connected (though not on the same stem) with *A. Sauveurei* Göpp. (*A. nervosa* var. Brgt.), both forms are referable to the same species.

Pecopteris Brgt.—The identification of the species of this genus is difficult. Here, as with the *Sphenopterideæ*, some species have been established on fructifications, which are seldom found in such a state of preservation that the form and the position of the fruit-dots can be ascertained, and others have been differently named from the branching of the veinlets, which differs in the same species according to the position of the leaflets. Thus identical species have received different names or even been placed in different genera. The number marked in the table represents only species of ours of which the value is ascertained. Some of them show apparently true American types. Thus, *Pecopteris Sillimani* Brgt., *P. Loschii* Brgt., *P. Velutina* Lsqx., *P. distans* Lsqx., *P. decurrens* Lsqx., and *Pecopteris concinna* Lsqx., widely differ from any European fossil *Pecopterideæ* known till now.

Caulopteris Brgt.—Uniting *Stemmatopteris* Corda, to this genus, we have at least four distinct species.

Stigmaria.—I think that the varieties of *Stigmaria anabathra* Corda, described by M. Göppert, are true species, being generally found in our coal-fields at different geological horizons. The five species described as new in the report of the Pennsylvania survey, may be perhaps reduced to three, but some well characterized species have been found since the report was made.

Sigillaria Brgt.—The distribution of this genus does not operate identically on the coal-fields of both continents. We have few species of the narrow-costate *Sigillariæ*, more than 30 species of which are counted in Europe. We have more of the broad-costate forms and especially a large number of species belonging to the peculiar section of the smooth or rather uncostate *Sigillariæ*.

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In this last section, three well characterized forms are peculiar to our coal-fields. The one has its surface stellately wrinkled around the scars; the second is wrinkled crosswise, and the third has double, oval, obliquely-placed scars united by a deep groove, and the surface is beautifully reticulated by narrow wrinkles, obliquely intersecting each other. The beautiful *Sigillaria Schimperi* Lsqx., of which the large scars have nearly the form of an eye, is also a peculiar American form. The number of our species, as marked on the table, is too small. About ten new species belonging to the Geological State surveys of Kentucky, of Arkansas and of Illinois are not here counted.

Syringodendron Brgt.—The two species described by Mr. Brongniart are common in America. I have never seen any other. Dr. Newberry indicates a peculiar species of ours under the name of *S. Americanum* Newb.

Lepidodendron Sternb.—The great number of specimens of this genus collected from the base and the top of the millstone-grit series of our coal measures, has afforded a good opportunity for examining the development and variations of the scars at the different stages of growth of the trees. Except *L. oculatum* Lsqx., which might be referred perhaps to *L. distans* Lsqx., all the new species described in the report of the survey of Pennsylvania are well characterized. Some of them might be considered as American types. *L. distans* Lsqx., is related, by the distance of the scars only, to *Sagenaria rimosa* Sternb.

Megaphytum Artis.—A beautiful and large specimen of a new species of this genus is preserved in the cabinet of the Geological State Survey of Illinois. Another new species is mentioned by Dr. Newberry under the name of *M. discretum* Newb.

Knorria Sternb.—The number of American species is apparently pretty large; but the difficulty of determining the species from specimens generally badly preserved has prevented or retarded the descriptions. *Knorria imbricata* Sternb., is especially common below the millstone grit.

Lepidophyllum Brgt.—We have already seven well characterized American species, and three new and unpublished ones. The number of peculiar forms of these scales or leaves of the cones of *Lepidodendron*, evidently shows that the large proportion of species of *Lepidodendron*, which have been found in America, cannot be considered as resulting from peculiar changes of the same species, according to the age of the scars. *Lepidophyllum hastatum* Lsqx., *L. brevifolium* Lsqx., and *L. plicatum* Lsqx., are American types not related to any European species.

Lepidostrobus Brgt.—The number of cones of *Lepidodendron* is extremely large, especially in the shales of the first bed of coal above the conglomerate. All the species ascertained, from the

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form of their scales, apparently the only reliable specific character for a determination, are counted with *Lepidophyllum*.

The fruits distributed among the genera *Cardiocarpum* Brgt., *Rhabdocarpos* Göpp. and Berg., *Trigonocarpum* Brgt., *Carpolithes* Sternb., are generally found broken or flattened or divested of their outer envelope. Consequently the identification of the species is very difficult. The number of these fruits disseminated in the shales and in the sandstone of the low coal of our American basin is considerable. They follow the stratigraphical distribution of the genus *Lepidodendron*, though they do not appear in any way related to this genus. About twenty well marked but undescribed species ought to be added to those counted in the table. Some of them have peculiar forms without relation to any European species. One of the most remarkable, *Rhabdocarpos arcuatus* Lsqx., is described and figured for the fourth volume of the Geological Report of the State Survey of Kentucky, under the direction of Dr. D. Dale Owen.

Lycopodites Brgt.—Nothing, perhaps, shows more evidently the difference in the characters of the coal flora of both continents than the scarcity of species of *Lycopodites* and the abundance of species of *Lepidodendron*, in our coal measures; when a contrary distribution rather predominates in Europe. Both the genera are considered as closely related. Nevertheless we have a single species of *Lycopodites*, very rare indeed, since I found specimens of it only lately on both the extreme limits of the state of Kentucky, at the same geological horizon, viz., in the shale of coal No. 1B. By its concave leaves, decurrent and embracing at the base, it differs from any of the twelve European species of the coal yet described Dr. Newbury has not mentioned any species of this genus in his catalogue.

Asterophyllites Brgt.—As some species of this genus are represented by catkins, scarcely if ever found attached to the stems, and of which therefore the relation is obscure or unknown, I may have counted as peculiar to America a few species which are only fruiting modifications of some others. I consider those fruits as male catkins, attached to large branches, while the true fruit-bearing catkins, which are much smaller, were apparently terminating the branchlets;—a kind of fructification somewhat analogous, but in a position contrary to that of some pines.

Without including the species of Dr. Newbury, the table of distribution shows that from six hundred and fifty-five species of coal plants now determined; more than one hundred and sixty are peculiar to America; three hundred and fifty species are known only in Europe; and one hundred and fifty are common to both. It is certain that future researches will greatly add to the number of species common to both continents, but as much, also, to the number of species peculiar to America. There-

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fore, the difference pointed out by the table, may be admitted as fairly representing, in a proportional manner, the general distribution of the coal plants on both continents. The botanical remains of the coal-fields of Europe have been carefully collected and studied by learned naturalists for more than a century, while those of America are only beginning to be noticed by scientific explorers.

In the Introduction to the Fossil Flora of the coal-fields of Pennsylvania, I had already pointed out the great analogy existing between the plants now living on the peat-bogs of America and of Europe. Admitting the peat-formation of our time as being the actual representative of the coal-marshes of the coal-epoch, I was led to the conclusion: that at this last epoch, the flora of both continents was as different and even more different than its representative flora is at our time. Thus, on twenty-five species of mosses growing on the peat-bogs and entering into the formation and composition of the peat, a single one is peculiar to North America. By extending my researches to the South, namely to the latitude of Norfolk, in the great Dismal Swamp of Virginia, I found the proportion changed in some manner; but nevertheless, the most common forms of the genus *Sphagnum*, which in Europe and North America form the principal mass of the peat, were found there also, performing the same work in the composition of the combustible matter. In the family of the Ferns, out of ten species growing on our marshes, five are identical with European species growing in the same situations; and two more are so closely allied to their European congeners that in a state of petrifaction, they could not be distinguished from each other. Even now, in their normal state, they are admitted by some botanists as varieties only. Among the *Junceæ*, *Cyperaceæ* and *Gramineæ*, twenty-six species out of forty-one are common to both continents; and from the other families, of which representatives are found on the peat-bogs, twenty-six species of thirty-one are found in Europe and in America. The likeness of some species of this section, peculiar to both continents, is still remarkable. Thus, *Larix Americana* and *Larix Europœa*; *Nymphœa odorata* and *Nymphœa alba; Ledum latifolium* and *Ledum palustre*, *Trientalis Americana* and *Trientalis Europœa; Vaccinium macrocarpum* and *V. Oxycoccos* (many others still could be named), are so nearly related that their specific characters can be distinguished only on good and complete specimens. There are, indeed, on the peat-bogs of America, some peculiar types which are not found in Europe: *Xyris bulbosa*, *Taxodium distichium*, *Sarracenia purpurea*, and a few others. But it is even so with the plants of the coal-period where we have seen certain types peculiar to this continent. This peculiarity serves only to render the more strik-

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ing the analogy of distribution of the flora of both epochs. It shows the same degree of difference and of analogy. Some species, even a few types, peculiar to each country, the greatest number of them peculiar to America; many identical species, and especially many forms, so nearly related, that it becomes very difficult to separate them by specific characters.

Columbus, Ohio, April, 1860.

(To be continued.)

ART. X.—*On an Oil-Coal found near Pictou, Nova Scotia; and the Comparative Composition of the Minerals often included in the term Coals;* by HENRY HOW, Prof. Chemistry and Nat. Hist., King's College, Windsor, N. S.

THE name given to the substance I purpose describing indicates the use to which it is put, viz., the manufacture of paraffine-oil, and an inquiry into the association of elements in the minerals constituting the sources of this and similar "mineral-oils" and in the bituminous coals, may possess some interest in a chemical point of view. As regards the classification of these it is not necessary to do more than recall the attempt made a few years ago in courts of law, in Scotland, New Brunswick and Nova Scotia, to decide what should and what should not be called a coal. The great array of evidence of various kinds brought to bear on the question rendered it a very interesting one, and it is well known that the opinions of the numerous scientific men consulted on these occasions were so nearly balanced that the point at issue was determined on the commercial, rather than on the scientific merits of the cases. It will be remembered that the substances in dispute were the Torbane-hill "coal," found near Bathgate in Linlithgowshire, Scotland, and the Albert "coal," occurring at Hillsborough, New Brunswick. As respects the former, the result of the trial in Edinburgh, in 1853, was that the jury considered it to be "coal, in the common sense of that word;" and, as regards the latter, it was decided at Fredericton, N. B., 1852, and at Halifax, N. S., to be also a "coal." Notwithstanding these legal decisions, which, from the conflicting opinions of witnesses, were obviously arrived at from other than scientific considerations, the question as to what is and what is not a coal, must be held to be an open one in those sciences in whose province the matter lies; and it will probably long remain so, because it was not from the absence of data, but from differing interpretations of facts about which for the most part there was a general accordance, that there arose the well known want of unanimity among geologists, mineralogists, chemists and microscopists.

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In this paper I do not hope to decide the question, but I wish to point out as interesting facts the occurrence of true bituminous coal in contact with the Oil-Coal, and to call attention to the relative proportions of the ultimate elements in the latter and in the before-mentioned disputed substances as compared with bituminous coals, as important in explaining the different nature of their products of distillation, and in affording support to those who do not make one species only of these minerals.

Some of the analyses which follow are published for the first time; others, of my own, relating to bituminous coals, I have taken from among those given in the Report on Coals suited to the Steam Navy of Great Britain, by Sir H. Delabeche and Dr. Playfair, and those of cannel-coals are taken from sources hereafter indicated.

The oil-coal found near Pictou, N. S., was first met with by persons residing in the neighborhood, early in 1859, and its exact locality is called Fraser Mine. I occurs in the coal-measures. I am indebted to Henry Poole, Esq., manager of the Fraser Mine, for the following particulars relating to the geological position, etc., of the substance:—

"The lowest measures about sixty yards, on the surface, short of the distance where the oil-coal crops, are composed chiefly of strong bands of sandstone, actual thickness not yet proved; then shales with bands of ironstone, and Stigmaria roots with Sigillaria stems, and a few detached fern leaves, in such soft shale that I have not been able to preserve any good specimens. Immediately above the oil-coal is a seam of bituminous coal about fourteen inches thick. Where we commenced to open a mine by driving a slope, the oil-coal was fourteen inches thick, but at 200 feet down at the bottom of the slope the oil-coal was twenty inches thick; it has a smooth regular parting at top next the coal, as also at the bottom next to the Oil-Batt below, but throughout its entire thickness it is of a curly twisted structure; many of its fractures look like the casts of shells, and the sharp edges are polished of a "slickensides" character. No fossils that I am aware of have hitherto been found in the curly Oil-Coal. The Oil-Batt next below is nearly two feet thick, of a homogeneous character with a slaty cleavage of various thicknesses. In this band two or three varieties (species?) of Lepidodendron beautifully preserved have been found, also leaves about one-fourth of an inch wide, and in lengths of from four to six inches, which have undergone so little change, that when the damp shale was fresh split, they could be removed, and were so elastic that they could be bent considerably without breaking. At the bottom of the slope another thin seam of curly Oil-Coal has appeared of a few inches in thickness, but is not worked at present. In the roof-coal were found pieces of de-

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cayed wood very little changed, which I consider a great curiosity. On M'Lellan's Brook shale is above the Oil-Coal, and Oil-Batt below in which have been found Lepidodendra and apparently molar teeth with three fangs, flattened modiola shells, and spines or small fish-teeth. The Oil-Batt has been found in several places without the curly band or so-called Oil-Coal. Two thousand tons of Oil-Coal have been raised (Dec. 1859) at the Fraser Mine."

The Oil-Coal varies in color from brown to black, is dull where not polished as just mentioned, has a reddish-brown lustreless streak, its powder is dark chocolate colored, it is very tough and breaks at last with a hackly fracture, its specific gravity, in mass, after the vessel of water containing it had been in an exhausted receiver =1·103. It takes fire very readily, and when removed from the lamp still burns for some time with a brilliant smoky flame, and flaming melted fragments continually drop from it in a truly characteristic manner. Ignited in coarse powder in an open crucible it gives off abundant smoke and flame, then seems to boil quickly, and a "coke" is left of the bulk of the original material, and showing when turned out a complete cast of the interior of the crucible. The ash of the "coke" is grey, and consists mainly of silicate of alumina, at least no lime or a mere trace is dissolved by acid, while some alumina is taken up and a great deal of solid remains undissolved. The powdered Oil-Coal digested with benzine and with ether does not more than sensibly color these fluids, but some residue remains on evaporation in each case.

The bituminous coal occurring with the oil-coal had the usual characters belonging to its species; it was black, brilliant, and very brittle. The proximate analyses of the two are placed side by side: and it will at once be obvious that they contrast very strikingly.

	Oil-coal.	Bitum. coal.
Volatile matters,	66·56	33·58
Fixed carbon,	25·23	62·09
Ash,	8·21	4·33
	100·00	100·00

The following is the ultimate analysis of the Oil-Coal, for which I am indebted to Mr. Slessor, assistant to Prof. Anderson of Glasgow, whose aid I requested from want of the necessary apparatus:

Carbon,	80·96
Hydrogen,	10·15
Nitrogen[a] (by loss),	0·68
Ash (as above),	8·21
	100·00

[a] with oxygen and sulphur?

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The Oil-Batt appears to be decidedly a shale, and a specimen from Bear Brook, Frazer Mine, gave these results:

Volatile matters,	30·65
Fixed carbon,	10·98
Ash,	58·47=100·00

I proceed to compare the Torbane Hill mineral, and the "Albert coal." A specimen of the former, examined at the time of the trial before mentioned, in 1853, gave me—

Volatile matters,	71·17·	Carbon,	66·00
Fixed carbon,	7·65	Hydrogen,	8·58
Ash,	21·18=100·00	Nitrogen,	0·55
		Sulphur,	0·70
		Oxygen,	2·99
		Ash,	21·18=100·00

In a recent examination, a specimen of "Albert coal" gave:

Volatile matters,	54·39	Carbon,*	87·25
Fixed carbon,	45·44	Hydrogen,	9·62
Ash,	0·17=100·00	Nitrogen,	1·75
		Ash,	0·17
		Oxygen and S,	1·21=100·00

These results I place in a table with corresponding data obtained from bituminous and cannel coals, the specific gravities of the substances, and the ratio of carbon to hydrogen as calculated directly from analysis, with the authority for the numbers. The first 7 analyses are from the Report on Coals by Sir H. Delabeche and Dr. Playfair, 1848, and Memoirs Geol. Survey, vol. ii; the 8th and 9th from Miller's Chemistry, iii, p. 201, and the 10th from Report of Trial on Torbane Hill "Coal," Edinburgh, 1853; the 11th has not been heretofore published.

	Name or Locality.	Sp. gr.	Proximate analysis. Volatile matters.	Fixed carbon.	Ash.	Ultimate analysis. C.	H.	N.	S.	O.	Ratio of C : H.	Observers.
†	Powell's Duffryn.	1·326	15·70	81·04	3·26	83·26	4·66	1·45	1·77	0·60	100 : 4 82	H. How.
†	Mynydd Newydd.	1·310	25·20	71·56	3·24	84·72	5·76	1·56	1·21	3·52	100 : 6·79	"
†	Ebbw Vale.	1·275	22·50	76·00	1·50	89·78	5·15	2·16	1·02	0·39	100 : 5·73	"
‡	Grangemouth.	1·290	43·40	53·03	3·52	79·85	5·28	1·35	1·42	8·58	100 : 6·61	"
‡	Fordel Splint.	1·25	47·97	48·03	4·00	79·58	5·50	1·13	1·46	8·33	100 : 6·93	"
§	Broomhill.	1·25	40·80	56·13	3·07	81·70	6·17	1·84	2·85	4 37	100 : 7·55	"
§	Parkend Sydney.	1·283	42·20	47·80	10 00	73·52	5·69	2·04	2·27	6·48	100 : 7·73	"
‖	Wigan.	1·276	39·64	57·66	2·70	80·07	5·53	2·12	1·50	8 08	100 : 6·90	Vaua.
¶	Lesmahagow.	1·251	56·70	37·26	6·034	73·44	7·62	?	1·145	?**	100 : 10 43	W. A. Miller.
¶	Capledrae.	?	?	?	25·40	56·70	6·80	1·90	0·35	8·80	100 : 11·99	A. Fyfe.
	Torbane Hill, Scot.	1·170	71·17	7·65	21·18	66·00	8·58	0·55	0·70	2·99	100 : 13·00	H. How.
	Hillsborough, N. B.	1·091	54·39	45·44	0·17	87·25	9·62	1·75	?	?††	100 : 11·02	J. Slessor and H. How.
	Pictou, N. S.	1·1039	66·53	25·23	8·21	80·96	10·15	0·68	?	‡‡	100 : 12·53	J. Slessor and H. How.

In this table we observe in the first place the resemblance of the last three substances in having a density much below that of

* This analysis kindly furnished by Mr. Slessor, as regards C, H, and N.
† Welsh bituminous coals. ‡ Scotch bituminous coals.
§ English bituminous coals. ‖ English cannel. ¶ Scotch cannel coals.
** N and O =11·761 per cent. †† S ? and O =1·21. ‡‡ N, S, and O =0·68.

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all the others, and secondly, that in all the bituminous coals but one the volatile matters are considerably less in amount than the fixed carbon, while in the cannel coals this is also the case with one of the two whose proximate analyses are given; as regards the other we see that it contains a large percentage (=11·761) of N and O which would of course be included as "volatile matters," and in the last three substances the volatile matters greatly exceed the fixed carbon. It is well known that in discussions on the chemical nature of coals, etc., much stress is laid on the relative proportions of these products, and also on the ratio of carbon to hydrogen, but it appears to me that an important element in the calculation has generally been omitted, or has not received due attention; I allude to the quantity of oxygen present, which of course can only be found by ultimate analysis. It is constantly stated that the gas and oil-producing value of a coal is indicated by the weight lost in coking, but this is obviously true only to a certain extent, and indeed is in some cases clearly untrue, for if we do not take into consideration the effect of oxygen present we cannot make a just comparison of the chemical nature of the substances, nor find the ratio of C : H, neither can we give the real gas or oil value, when, as above, from eight to ten per cent of what is generally supposed to be carbon and hydrogen is really oxygen with nitrogen. If for example we consider the effect of the oxygen in the composition of the substances given in the table we shall see the last three present such differences from the others as to strengthen the position of those who decline calling them 'coals.' Limiting our view to the cannel-coals, which, as seen above, exhibit the ratio of C : H apparently equal or nearly so to that in the substances in question, we observe that they all contain much more oxygen, and if we deduct the equivalent quantity of H in all, as is theoretically necessary for arriving at the heating power, we shall find this similarity greatly lessened; as thus,

		Ratio of C : H after deducting H=O.
Cannel coal from	Wigan,	100 : 5·65
" "	Lesmahagow,*	100 : 8·71
" "	Capledrae,	100 : 10·05
Mineral "	Torbane Hill,	100 : 12·43
" "	Hillsborough,	100 : 10·85
" "	Fraser Mine,	100 : 12·43

The last three substances should prove, theoretically, the excellent 'Oil-Coals' they are known to be. Of course the practical yield of oil will vary according to the manipulation, the perfection of the manufacturing processes and the quality of samples employed, but the following statement of the comparative

* After allowing two per cent for nitrogen.

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amounts of oil afforded by some of the above may be taken as a good illustration of the point brought forward in this paper. I am indebted for these details to H. Poole, Esq.:—

In Scotland the Lesmahagow cannel coal gives 40 gallons crude oil, and 32 gallons rectified oil per ton.

At M'Lellan Brook the Fraser Oil-Coal gives 40 gallons crude oil per ton.

At Coal Brook the Fraser Oil-Coal and Oil-Batt together give 53 gallons per ton.

At M'Cullock Brook the Fraser Oil-Coal gives 77 gallons per ton.

The "Albert coal" gives 100 gallons per ton.

The "Torbane Hill coal" gives 125 gallons per ton.

And some picked samples of Oil-Coal from Fraser Mine, tried in Boston, U. S., gave no less than 199 gallons of oil per ton.

Windsor, Nova Scotia, May, 1860.

ART. XI.—*The Great Auroral Exhibition of Aug.* 28*th to Sept.* 4*th,* 1859*; and the Geographical Distribution of Auroras and Thunder storms.*—5TH ARTICLE. By Prof. ELIAS LOOMIS.

SINCE the publication of our former articles on the great aurora of Aug. 28th to Sept. 4th, we have received some additional observations which we here subjoin.

1. *Extract from a Journal of the weather in Swedish Bothnia,* (lat. 67° N., long. 22° E.), *by* ROBERT RAWLINSON, *copied from the London Times of Oct.* 5.

Aug. 27–28. Morning gloomy; clouds gray and electric looking; a sort of dense "Noah's Ark" sky.

Aug. 28. Night, heavy rain.

Aug. 29. Night bright and clear, but bitterly cold; ice a quarter of an inch thick round tent.

Aug. 30. Day fine; sun very hot. No observation at night.

Aug. 31. Day fine, clear and calm. No observation at night.

Sept. 1. Morning, heavy clouds showing for wet.

Sept. 2. Heavy thunder storm at night, vivid lightning and deluges of rain.

Sept. 3. Morning cloudy; thick mist over forest.

Sept. 4. Morning cloudy; heavy dew in night, thick fog.

Sept. 5. Cloudy, but fine.

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2. *Observations at St. Petersburg, Russia,* (lat. 59° 56′), *communicated by* Prof. A. T. KUPFFER, *Director of the Central Physical Observatory.*

From Aug. 28th to Sept. 4th, 1859, the disturbances of the magnetic instruments at St. Petersburg were very remarkable not only for their extent but for their long continuance. The magnetic observations are ordinarily made every hour, but on the preceding days, observations were made every five minutes. We have not room to publish these observations entire, but the following table exhibits the most remarkable deflections of the magnetometers.

The north pole of the unifilar magnetometer moves towards the east, when the numbers of the scale increase. The value of one division of the scale is 26″·3.

When the numbers on the scale of the bifilar magnetometer increase, the magnetic intensity increases. The value of one division of the scale is 0·0001 of the total intensity.

In these observations the day is supposed to begin at noon.

Hour.	Unifilar.	Bifilar.
August 28th.		
h m		
0 0	132·5	185 4
3 0	105·8	193·7
9 0	143·0	207·0
13 0	108·0	186·0
16 0	169·3	420·0
19 0	122·0	420·0
22 0	192·0	420·0
23 05	105·0	106·7
23 20	165·0	230·2
23 40	102·0	127·5
August 29th.		
0 0	135 5	209·0
0 45	103 0	168·0
0 55	194·0	321·2
1 5	111·0	66 5
2 40	143·0	222·8
4 0	132·3	187 0
5 5	220·5	257·5
5 30	140·0	195·0
6 10	185·0	190·0
7 20	100·0	98·0
7 35	90 0	100·0
8 0	150 0	138·0
8 45	129·0	133·3
19 0	153·0	124·0
23 0	137·4	125·0
September 1st.		
0 0	101·2	136·5
2 0	75·5	183·0
9 0	150·8	167·9
12 0	120·0	159·0
20 0	151·5	419·0
20 30	410·5	419·0
21 0	530·0	419·0
21 10	601·5	419·0
21 30	302·0	216·0
21 40	9·0	12·0
21 45	off scale	7·8
21 55	94·0	0·0
22 5	off scale	119·0
22 15	320·0	177·0
22 30	85·0	0 0
22 50	123·7	98·5
23 15	10·0	150·0
23 30	128·0	100·0
23 45	59·5	86·0
September 2nd.		
0 5	135·0	205·3
0 35	67·0	150·0
0 45	134·5	65·0
1 55	81·0	178·0
2 20	186·0	393·0
2 25	82·0	555·0
2 40	off scale	304·5
3 20	134·0	306·0
3 40	13·5	519·2
4 0	147·0	292·5
4 45	85·0	420·5
5 0	122·4	571·5
5 10	41·0	501·0
5 50	104·5	277·5
6 45	100·0	172·2
8 0	234·5	420·0
8 5	242·0	303·4
8 25	115·0	166 0
10 35	95·3	90·3
12 0	70·1	50·2
12 15	63·0	10·2
12 25	47·3	off scale
12 50	160·9	70·6
13 35	156·0	off scale
14 45	116·2	78·0
15 15	122·4	44·6
16 20	113 0	108·0
17 5	98·0	104 2
18 35	160·0	109 5
20 20	147·0	129·0
20 55	127·0	123·0
21 45	150·3	115·2
22 5	136·0	106·3
23 55	120·0	136·0
September 3rd.		
0 15	126·0	143·0
0 50	111·9	127·5
1 55	85·0	247·0
2 55	75·5	210·0
4 15	50·0	237·0
4 55	45·0	229·0
6 45	110·0	275·0
8 0	144·0	142·0
8 40	44·0	55·0
9 0	101·0	52·5
10 0	158·0	154·0
13 40	72·0	off scale
14 20	65·0	35·5
16 20	157·0	135·2
18 15	150·0	140·0

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Hour.	Unifilar.	Bifilar.	Hour.	Unifilar.	Bifilar.	Hour.	Unifilar.	Bifilar.
h m						h m		
20 55	127·0	84 0		September 4th.		5 10	142·5	158·0
21 20	84·2	109 0	h m			5 40	180·0	247·0
21 45	131·0	109·0	0 40	104·0	122·0	7 20	124·5	127·0
23 55	110·7	140·4	1 15	98·0	135 6	9 35	113.5	150·0
			2 35	113·8	134·0	16 0	140·2	134·2
			3 20	79·0	233·0	20 0	126·3	159 0
			4 0	101·5	257·0	23 0	115·0	136·0

From the preceding observations we see that the range of the magnetometers for each day was as follows:

	Unifilar magnetometer.			Bifilar magnetometer.		
	d	*d*	*d*	*d*	*d*	*d*
Aug. 28	From 102 to 192	= 90	= 39′ 27″	From 106·7 to 420	=313·3	=·031
Aug. 29	" 90 " 220·5	=130·5	= 57 12	" 66 5 " 321·2	=254·7	=·025
Sept. 1	" 0 " 601·5	=601·5	=263 39	" 0 " 419·0	=419·0	=·042
Sept. 2	" 0 " 242	=242	=107 44	" 0 " 571·5	=571·5	=·057
Sept. 3	" 44 " 158	=114	= 49 58	" 0 " 275	=275	=·027
Sept. 4	" 79 " 180	=101	= 44 16	" 122 " 257	=135	=·013

It should be remarked that in the preceding table, 0 indicates that the magnets passed beyond the range of their scales; so that we can only conclude that on Sept. 1st and 2nd the range of the magnets certainly *exceeded* the values here given.

3. *Observations at Catherinenburg, Russia* (lat. 56° 50′, long. 60° 34′ E.), *communicated by* Prof. A. T. Kupffer.

The following observations are arranged in the same manner as those from St. Petersburg. The value of one division of the unifilar scale is 33″·4. The value of one division of the bifilar scale is 0·0001 of the total intensity.

Hour.	Unifilar.	Bifilar.	Hour.	Unifilar.	Bifilar.	Hour.	Unifilar.	Bifilar.
	September 1st.		h 22	off scale	off scale	h 8	536·5	300·5
			23	off scale	180·0	10	481·0	295·7
h						11	519·0	297·0
0	496·0	308·5		September 2nd.		14	512·0	283·0
2	490·0	311·6				20	539·0	285·4
6	507·5	319·0	0	434·0	238·0	23	528·0	291·8
12	510·3	321·0	3	519·0	281·2			
18	525·0	320·2	6	427·0	396·0			
21	580·0	201·0	7	520·0	412 0			

4. *Observations at Barnaul, Russia* (lat. 53° 20′, long. 83° 27′ E.), *communicated by* Prof. A. T. Kupffer.

The value of one division of the unifilar scale is 32″·8. One division of the bifilar scale =0·0001 of the total intensity.

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Hour.	Unifilar.	Bifilar.	Hour.	Unifilar.	Bifilar.	Hour.	Unifilar.	Bifilar.
	August 28th.			August 29th.			September 1st.	
h			h			h		
0	391·5	171·6	0	416·5	off scale	22	413·2	152·5
1	383·6	175·4	2	402·5	200	23	519·3	off scale
4	392·6	185·0	3	382·0	121·0		September 2nd.	
10	402·3	171·4	5	420·0	48·0	0	off scale	466·5
14	408·0	161·5	6	437·0	94·0	2	358·5	32·3
18	423·4	69·0	9	404·0	116·0	6	385·0	262·0
20	431·8	7·2	10	422·0	106·5	8	350·6	156·5
23	412·5	off scale	18	417·5	125·0	9	440·5	77·4
			23	408·5	107·5	19	397·0	166·6
						23	404·0	107·5

5. *Observations at Nertchinsk, Russia*, (lat. 51° 18′, long. 119° 20′ E.), *communicated by* Prof. A. T. KUPFFER.

The value of one division of the unifilar scale is 33″·8. One division of the bifilar scale =0·0001 of the total intensity.

Hour.	Unifilar.	Bifilar.	Hour.	Unifilar.	Bifilar.	Hour.	Unifilar.	Bifilar.
	September 2nd.		h			h		
h			5	294·8	205·6	11	450·0	260·0
0	347·0	285·1	6	327·1	224·1	12	333·0	246·0
1	383·0	235·4	7	339·5	256·5	13	345·4	243·0
2	635·4	370·1	8	335·0	250·4	17	327 0	269·0
3	355·5	321·5	9	312·2	288·5	19	352·0	279·0
4	214 0	181·0	10	311·9	243·5	23	343·5	243·0

6. *Observations at Athens, Greece* (lat. 37° 58′), *by* J. F. L. SCHMIDT, *Director of the Observatory.* (Communicated through Rev. Mr. King, Am. Missionary at Athens.)

The aurora was not seen at Athens Aug. 28th and 29th. Both evenings were very clear and still, especially Aug. 28th. Aug. 29th from 7ʰ to 8ʰ P. M. some clouds were seen in the west over the Morea. Aug. 30–31 was likewise clear, with a very few small clouds. Aug. 31st, in the evening, lightning in the N.W. Sept. 1st, evening, partly clear, partly cloudy, with lightning in the west.

Sept. 2d, 7ʰ 15ᵐ A. M., beginning of a storm from the west, rain and thunder; at 8ʰ 30ᵐ A. M., rain, hail and lightning. From noon to 12ʰ 40ᵐ P. M., violent shower from the west. Then became clear with sunshine. The evening was clear, and in the north there appeared a dark bank of ordinary cloud (not the dark segment of the aurora), above which, from 7ʰ 30ᵐ P. M. to 8 P. M., was seen a fine aurora of a carmine red color. The cloud bank, which extended 60° in azimuth, was elevated somewhat above the horizon, so that stars were seen beneath it. The centre of the auroral light was not in the north, but N.N.W. On the west it was bounded by Cor Caroli, and on the east by

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α Persei. No streamers or fluctuations of light were observed. By 10 P. M. the cloud bank had disappeared, the auroral light having disappeared previously.

Sept. 3d. The entire day was clear; and at 4 P. M. I went on board a steamer for Syra. From 7½–8 P. M., near the island Egina I saw in the north and northwest the carmine red light of an aurora. From 9½–10½ P. M., near Cape Sunium, a faint trace of the aurora was still seen; but no dark segment, streamers or fluctuations of light.

7. *Observations at Camp Simeahmoo, Washington Territory*, (lat. 49°, long. 122° 30′ W.), *by* ARCHIBALD CAMPBELL, *Commissioner of N. W. Boundary Survey.*

At 8 P. M. Aug. 28, 1859, a diffused light, without definite form, was observed a little east of north, covering about one-fourth of the heavens, which gradually increased to the west, sending across from east to west an arch of a whitish color, the arch itself being much brighter than the circumjacent light. This arch remained visible until 2 A. M. At $9^h\ 25^m$ P. M. strongly marked rays became visible, which rising from the horizon converged to a point on the arch a little south of the zenith, and in this position remained visible about one hour. The rays in the northwest were of a pink color, those in the southeast were purple, alternately brightening and fading to a whitish color. At midnight, all disappeared except the arch, and at intervals undulating flashes of light appeared, not visible longer than three seconds. Occasionally streamers shot up from the horizon, the lower part disappearing before the upper part had reached the zenith. Sometimes these streamers were broad at the horizon, and came to a point near the zenith, and sometimes the reverse. The arch before mentioned was easily identified, and was still visible at 2^h A. M., and probably remained so until daylight, which at that season, in this latitude, occurs not long after that time. This arch was situated very little, if any, to the southward of the zenith, and was the limit of all light in that direction. The light was sufficiently intense, between 11 and 12 o'clock, to enable a person to read the ordinary print of a newspaper. After the aurora was fully formed, it remained stationary, and did not move either to the west or east. At midnight, the barometer stood at 30·13; external thermometer 64° F.

August 29th a faint diffused light was seen in the north at 9 P. M., and was still visible at midnight.

August 30th a similar light was first seen at 8½ P. M. and was still visible at midnight. No observations were made after midnight. There was no exhibition of the auroral light at this place from Aug. 31st to Sept. 4th.

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The view of the northern horizon at this place is cut off by a dense forest of firs, and the sight of the heavens in that direction is some 5 or 6 degrees above the horizon.

8. *Observations at Hamilton, C. W.* (lat. 43° 16′ N.), *by* Dr. J. HURLBURT.

Answers to the questions in our first article, vol. xxviii, p. 408.

1. On the 28th of August the sky was overcast by a dark sombre cloud-like substance, but which was not cloud as the stars could be seen through it. There was no dark segment resting on the northern horizon, but one was seen at the south between 8 and 9 P. M. rising 8° or 10° in the centre. Sept. 1st between 8 and 9 P. M. this dark segment was well defined at the north, with an altitude of about 10°, and skirted the horizon fully 120°. At 1 A. M. Sept. 2d, an unusually large segment was distinctly defined at the south, where it rose fully 15° in the centre, and stretched over 130° of the horizon.

2. At 8 P. M. Aug. 28th, and from 1 to 3 A. M. Sept. 2d, the streamers of the aurora converged to a point a little east of the meridian, and 15° or 20° south of the zenith, forming a brilliant corona.

3. Aug. 28th, both in the southwest and southeast there was a dark red spot about 14° in breadth, and extending from altitude 35° to alt. 55°. Both spots presented the same appearance, and hung in corresponding parts of the heavens east and west.

4. At 1 A. M. Sept. 2d, the whole of the southern half of the sky was lighted up, resembling the sky at late dawn.

5. On the night of Aug. 28th, the most frequent and conspicuous color was red and its different shades. The aurora of Sept. 1st was scarcely at all marked with any of these colors. The light was chiefly white, resembling the dawn just before sunrise.

6. The aurora of Aug. 28th was most brilliant at 8 P. M. It was also very brilliant at 1 and 3 A. M. Aug. 29th. The aurora of Sept. 2d was most brilliant at 1 A. M.

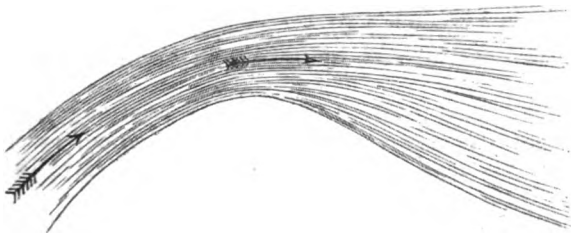
9. *Observations at Rome, N. Y.* (lat. 43° 13′), *by* Mr. EDWARD HUNTINGTON.

During the auroral display on the evening of Sept. 2d, there was a very rapid and incessant flashing of white light, like waves running up to the corona, the light being intermitted for some seconds after the passage of each wave, and the next wave pursuing the same course, and *following certain curvatures which remained constant for a considerable time*, perhaps half an hour. There was a succession of waves flashing up from a point in the northwest, where the effect was as if a luminous fluid were in its course passing through a narrow strait, from which it issued,

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expanding like water violently forced through a narrow passage out upon a broad level area, and being at the same time somewhat deflected from its former course. Fig. 1 is designed to convey some idea of this appearance.

1.



2.



From another point nearly in the east, there was a succession of waves of light, directed to a point north of the corona, but turning suddenly into it nearly at a right angle with their former course, the light disappearing after the passage of each wave, and being renewed after a short interval in precisely the same form. Fig. 2 is designed to give some idea of this appearance.

10. *Observations at Cleveland, Ohio* (lat. 41° 31′), *by* Capt. B. A. Stanard.

Aug. 28th, at 8^h 30^m P. M., the aurora began to show itself, lighting up the northern sky, rising towards the zenith, in a broad belt of luminous haze of about 40° in width, the southern edge starting a little to the westward of the star Arcturus, and running through the head of Hercules, a little south of Altair in Aquila, through the head and neck of Pegasus. The eastern end was bright enough to light up the edges of the detached masses of cumuli that were driving over from the north. In the zenith and the western end it was of moderate brightness.

At 9 P. M. another belt began to rise up in the north, and as the convex edge attained a height of about 40° it began to shoot out long, attenuated, bright rays, close together, moving slowly to the westward, and reaching to the zenith. Near the convex edge they were of a bright yellow, changing as they shot up to

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orange, and near the zenith to a bright red, the middle and lower ends remaining yellow and orange. As the fiery points of the rays shot into the broad belt overhead, which had still remained like a belt of luminous haze or fog, the whole thing was changed in an instant into a bright red color, deepening as it neared the eastern horizon, to a bright crimson, and at the western end, near the star Arcturus, into a bright scarlet, gradually growing fainter in the zenith, and increasing in brightness nearer the horizon.

At $9^h\ 15^m$ P. M. it resolved itself into converging rays. The zenith at that time was covered by a massive cloud, covering the point where the rays would meet, which must have been near the star β Cygni.

At $9^h\ 30^m$ P. M. the whole had disappeared, except a steady white light in the north, very bright, forming a curve from the western edge of Böotes, through Cepheus and Perseus. The northern horizon was too clouded the whole time to see anything near it.

Sept. 2nd, at $7^h\ 45^m$ P. M., the northern lights began to show, forming a low narrow arch extending from the N. W. to N. E., the lower edge about 10° in height, well defined, of moderate brightness, and remaining without perceptible change until $8^h\ 45^m$ P. M., when bright vertical rays shot up simultaneously the whole length of the arch, of an exceedingly bright white color, with no perceptible motion E. and W. Shooting slowly up towards the zenith, at an altitude of about 45°, they began to change color rapidly, varying from white, yellow, orange, and at the upper ends red; diffusing a soft pink color over the northern sky at an altitude of about 45°. At 9 P. M. they gradually disappeared, the arch was broken up, leaving some irregular white blotches in the north, which faded away and disappeared altogether. Then commenced a series of quick, sudden flashes of undefined light; here and there in the north, scimetars bearing a strong resemblance to heat lightning, sometimes in undefined rays, and sometimes in undefined shimmering light.

This continued until $9^h\ 45^m$ P. M., when a double arch was formed of two narrow belts of light about 15° in height, running from Canes Venatici to the southern edge of Perseus, the bright star Capella shining through the narrow black space between the two arches.

At $9^h\ 55^m$ P. M. bright rays suddenly shot up in quick successive flashes from the lower through the upper arch, reaching nearly to the zenith, and moving slowly to west until they reached the constellation Corona Borealis, lighting up the north western sky with yellow, orange and red. After the last rays from the east had passed the Pole, there commenced a sudden flashing of horizontal wavy bands from the upper arch towards the zenith.

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At $10^h\ 10^m$ P. M. the rays and arches disappeared and the northern sky seemed to be covered with a steady white light, with horizontal wavy bands of dark haze rolling up in quick succession, and vanishing as they attained an altitude of about 60°, continuing until $10^h\ 30^m$ P. M. and gradually fading away.

11. *Observations at Fort Bridger, Utah Ter.* (lat. 41° 14′, long. 110° 33′), *by* KIRTLEY RYLAND, *Assist. Surgeon U. S. Army.*

Sept. 1st, 1859, a brilliant aurora was seen at this place. It was first observed about 11 P. M. and attained its greatest brilliancy before midnight. It extended from the northeastern horizon to the southern horizon, and was in fact a Borealo-Austral Aurora. Generally the light assumed the form of spikes and bars, but high above the horizon in the northeastern sky was a large blotch or spot, whose diameter was perhaps three times that of the constellation Orion. This blotch was of a deep crimson color, and remained for a considerable time unchanged in form, color or intensity, and faded gradually away. In the other portions of the aurora the light appeared to spout from the horizon, in the shapes already named, frequently reaching the zenith, and was of great brilliancy. It appeared to flow gradually from N.N.E. to the southern sky.

12. *Observations made at Cantonment Burgwin, New Mexico* (lat. 36° 21′, long. 105° 42′), *by* W. W. ANDERSON, *Assist. Surgeon U. S. Army.*

A member of the guard mounted Sept. 1st observed a light reflected from the clouds on that night about 10 P. M. as he was walking post, the clouds having probably just then thinned out or broken away a little. No other member of the guards from Aug. 28th to Sept. 5th saw anything unusual during the intervening nights. By reference to the Meteorological Register I find that the weather was cloudy during the whole time that the aurora was visible elsewhere. There was rain on the last four days of August, and also on the 1st, 2nd and 4th of September.

The exhibition was witnessed at Taos, about ten miles north of us, by persons residing there, but was not observed with sufficient attention to enable them to answer any of your questions with accuracy. A physician, Dr. Ferris, who arrived at Taos from Pike's Peak during the fall, states that he was in the South Park at the time, and thinks that it was on the night of the 28th of August that the Aurora was observed by him. He saw it but one night. Streamers were seen to converge to the zenith about 10 P. M., but the aurora was not seen in the southern half of the heavens. The light was like a large fire in the distance, so that at first it was thought to be an extensive fire on the

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mountains. The aurora exhibited sudden flashes, and there were pulsations like waves of light rushing up from the horizon. I have been told that Capt. John G. Walker, of the Rifle Regiment, wrote a description of the aurora as it appeared at Fort Defiance (lat. 35° 44′), where it is said to have been very brililant.

13. *Observations at the Sandwich Islands* (lat. 20° N., long. 157° W.), *from the Pacific Commercial Advertiser.*

The Advertiser of Sept. 8th, 1859, states: "There was quite a display of the Aurora a few nights since, visible at Honolulu. Broad fiery streaks shot up into and played among the heavens, almost as beautifully as those which are sometimes seen in more northern climes."

The Advertiser of Sept. 17th contains the following letter from S. E. Bishop, dated Lahaina, Sept. 9th. "Your statement that the Aurora was seen in Honolulu enabled me to account for the phenomenon I observed here a few nights since. At 10 P. M. I noticed a bright, unsteady crimson glow upon the sky, extending from N. E. to N., and about 35° of altitude. It resembled the reflection of a great conflagration at twenty or thirty miles distance, and I attributed it to heavy fires on the other side of the mountain. I was puzzled however by the fact that the clouds which rested on the mountain did not give the slightest reflection of the supposed fire. Moreover the light was far too pure and rich a crimson to have been caused by a fire.

14. *Observations at Porto Rico, West Indies* (lat. 18° N.), *by* M. du COLOMBIER, *from L'Institut of Feb. 1st*, 1860.

Having awakened at $2^h\ 30^m$ A. M., Sept. 2nd, I was greatly astonished to see my windows, which looked towards the north, brightly illumined by a brilliant purple light. Rising immediately, I perceived that this light proceeded from a magnificent aurora, which, according to the testimony of the guard, commenced at 2^h A. M. and was observed till 4^h A. M. The luminous rays, red, purple and violet, extended even to the zenith. The oldest inhabitants of the place declared that they had never before seen a phenomenon of this kind.

15. *Observations at Santiago de Chili* (lat. 33° 26′ S.), *by* C. MOESTA, *Director of the Observatory.*

The aurora you allude to did occur at this and several other places in the south of Chili, during the night between Sept. 1st and Sept. 2nd, 1859. I did not witness the phenomenon myself, but it appears that the aurora was visible from about half past 1 until 4 A. M., showing a motion to the west. The watchmen were much alarmed at the colored light with which the southern

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part of the sky was covered, which gave rise to the belief that a small village about three leagues south of Santiago was on fire. This seems to be the first time that a polar light has been seen at Santiago. No notice has reached me as to its appearance north of this place.

16. *Observations near Cape Horn* (lat. 57° S., long. 66° W.), *by* RICHARD SCHUMACHER, *communicated by C. Moesta.*

Mr. Richard Schumacher, assistant to the Chili Observatory, was at the time on board a ship near Cape Horn. Being informed Aug. 29th that an aurora had been seen during the preceding night, he begged the mate of the vessel would let him know if there should be another. Accordingly he was awakened during the night from Sept. 1st to 2nd, between 2 and 4 A. M., when the aurora was already in its splendor. In a southerly direction there appeared a bright yellowish light forming an ellipse, whose diameters were as two to one, the centre of the ellipse being elevated about 15° above the horizon. The part of the horizon below this light seemed to be a cloudy mass of a dirty reddish color. From this ellipse, emanated a red light, apparently all over the heavens up to the zenith, and thence onwards to the north. Mr. S. did not distinguish any beams or columns of light, though the sky seemed illuminated all round nearly uniformly, except that the light of the ellipse was much brighter than the surrounding parts. There were also some light transparent clouds discernible near the zenith. The light was so brilliant that he could easily read the title page of the Nautical Almanac, and distinguish the seconds hand of a box chronometer.

The vessel was at noon, Sept. 1st, in lat. 57° 8′, long. 66° 38′ W.
" Sept. 2nd, " 57 36 " 66 47 "

ON THE GEOGRAPHICAL DISTRIBUTION OF AURORAS IN THE NORTHERN HEMISPHERE.

Auroras are very unequally distributed over the earth's surface. They occur most frequently in the higher latitudes, and are almost unknown within the tropics. At Havana (lat. 23° 9′) but six auroras have been recorded within a hundred years; and south of Havana, auroras are still more unfrequent. As we travel northward from Cuba, auroras increase in frequency and brilliancy; they rise higher in the heavens, and oftener attain the zenith. The following tables furnish the most precise data I have been able to collect for constructing an auroral chart of the northern hemisphere. Column first gives the name of the station of observation; columns second and third its latitude and longitude; column fourth the average number of au-

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roras observed per year; column fifth, the greatest number of auroras recorded in a single month; column sixth shows the number of years embraced in the comparison; and column seventh shows the authority for the statement.

The numbers for several of the stations are derived from an article contained in vol. viii of the Smithsonian Contributions, entitled, Record of auroral phenomena observed in the higher northern latitudes, compiled by Peter Force. Such observations are indicated by the word 'Force' in the last column.

TABLE I.—*Average annual number of Auroras in North America and its vicinity, from longitude 30° to 170° west from Greenwich.*

Place.	Latitude.	Longitude.	Per year	Richest month.	Years embraced.	Authority.
	° ′	° ′				
Havana,	23 9	82 22	1/6	1	100	Am. Jour., [2], xxviii, 403.
San Francisco,	37 47	122 26	1/3	1	4	Smithson. Report, 1854, p. 258.
Sacramento, Cal.,	38 34	121 27	1/3	1	9	Am. Jour., [2], xxix, 260.
Washington, D. C.,	38 53	77 0	9	3	4	Gilliss's Met. Observations.
Wilmington, Del.,	39 44	75 32	8	4	7	Am. Jour., xxxiii, 299.
Philadelphia,	39 58	75 10	6	3	4	Bache's Met. Observations.
New York,	40 42	74 0	14	5	1	N. Y. Regents Rep., 1850, 255.
New Haven,	41 18	72 55	26	12	16	E. C. Herrick's observations.
North Salem, N. Y.,	41 26	73 38	8	2	10	N. Y. Regents Rep., 1850, 258.
Deerfield, Mass.,	42 33	72 35	4	2	1	Am. Jour., iv, 337.
Fayetteville, Vt.,	42 53	72 40	21	3	11	Am. Jour., vols. xii to xxiv.
London, C. W.,	42 58	81 25	27	7	4	Regents Rep., 1850, 290–293. Am. Jour., [2], xiv, 156.
Toronto, C. W.,	43 39	79 23	39	9	12	" " "
Kingston, C. W.,	44 8	76 40	36	11	4	Am. Jour., [2], xiv, 156.
Somerville, N. Y.,	44 26	75 20	70	14	2	" " "
Halifax, N. S.,	44 39	63 36	55	13	2	" " "
Montreal, C. E.,	45 31	73 32	34	10	2	Regents Rep., 1850, 290 & 291.
Quebec, C. E.,	46 49	71 12	42	12	4	" " "
St. John's, N. F'nd,	47 33	52 38	52	10	3	" " "
Michipicoton,	47 56	85 2	43	9	2	Am. Jour., [2], xiv, 156.
Matawagomingen,			26	7	1	" " "
Moose Factory,	51 10	81 0	141	19	1	" " "
Martin's Falls,	51 52	86 45	79	14	1	" " "
Cumberland House,	53 56	102 16	104	25	2	Gehler's Wörterbuch, vii, 144.
Athabasca Lake,	58 43	111 18	91	21	2	Athabasca Obs., p. 145. Am. Jour., [2], xiv, 156.
Frances Lake,	61 30	129 0	33		2	Lake Athabasca, p. 148.
Lewis and Pelly,	61 30	130 0	36	12	1	Am. Jour., [2], xiv, 156.
Fort Simpson,	61 51	121 32	50	24	1½	Athabasca Obs. & Am J., xiv, 156.
Great Slave Lake,	62 46	109 1	105	28	2	Capt. Back, 1833–1835.
Godthaab,	64 10	51 53	72	15	6	Observat. Meteorolog., 166–228.
Fort Enterprise,	64 28	113 6	142	28	1	Force, pp. 24–35.
Fort Norman,	64 40	124 45	32	15	½	Force, p. 54.
Fort Franklin,	65 12	123 12	43	17	½	Force, pp. 60–64.
Youcon,	66 0	147 0	24	7	½	Am. Jour., [2], xiv, 156.
Winter Island,	66 11	83 10	25	8	1	Force, pp. 66–72.
Fort Hope,	66 32	86 56	39	13	1	Force, pp. 75–77.
Fort Confidence,	66 54	118 49	100	30	2	Athabasca Obs., 324–350. Am. Jour., [2], xiv, 156.
Peel's River,	67 27	134 30	65	16	1	Am. Jour., [2], xiv, 156.
Jakobshaven,	69 12	51 0	18	5	8	Observat. Meteorolog., 82–164.

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TABLE I.—*Continued.*

Place.	Latitude.	Longitude.	For year	Richest month.	Years embraced.	Authority.
	° ′	° ′				
Felix Harbor,	69 59	92 1	23	10	1	Force, pp. 84–87.
Point Barrow,	71 21	156 15	131	26	1	Phil. Trans., 1857, p. 497.
Port Bowen,	73 14	88 55	47	15	1	Force, p. 91.
Melville Island,	74 47	110 49	26	8	1	Force, pp. 97–103.
Smith Sound,	78 37	70 40	10	5	2	Dr. Kane's Obs.

The following auroral observations are generally confined to brief periods of time, and have therefore less importance than the preceding. They are nevertheless of some value in deciding in what part of the world auroras are most frequent and brilliant. These notices are all taken from Force's article in the Smithsonian Contributions, vol. viii.

TABLE II.—*Notices of Auroras in North America and its vicinity, from longitude 30° to 170° west from Greenwich.*

Place.	Latitude.	Longitude.	Auroras.
	° ′	° ′	
Cedar Lake,	53 13	100 10	Extremely brilliant and covered the whole sky.
Off Cape Farewell,	56 17	42 51	Frequently most brilliant.
At sea,	57	49	Occurred almost every night.
York Fort,	57 2	93	Very few winter nights without the aurora. One may read distinctly by it.
At sea,	57 30	45	Whole southern hemisphere illumined. Gave nearly as much light as the full moon.
At sea,	58 12	49 15	Radii shot from the southward.
At sea,	58 30	44 30	Far surpassed anything of the kind observed at Port Bowen.
At sea,	59	50	Seen in every part of the heavens.
At sea,	59 58	59 53	Brilliant coruscations.
Hoarak,	59 59	44 30	Four luminous arches.
At sea,	60	56	Whole sky illuminated.
Kikkertak,	60 4	43 2	Unusually brilliant coruscations.
Nennortalik, Greenland,	60 8	45 16	Of every day occurrence. Brighter than the full moon.
Davis Straits,	60 10	49 40	Yellow and reddish coruscations, extending near the zenith.
At sea,	61 4	49 50	Illumined the whole southern sky.
Davis Straits,	61 37	52	The whole sky was one living fire of aurora.
Davis Straits,	61 58	54 40	Most brilliant aurora danced to the zenith.
At sea,	62 30	63	Aurora seen in the south.
Rankin's Inlet,	62 35	93	Aurora very bright, spreading all over the sky.
Hudson Strait,	62 45	72 24	Aurora unusually splendid.
Good Hope,	64 10	51 22	Auroras always spring up in the E. or S.E.
Cape Lavenorn,	64 30	39 30	Auroras unfrequent.
At sea,	65	63	Very brilliant, spread all over the heavens.
Southampton Island,	65 28	84 40	Visible during the whole of the night.
At sea,	65 50	61	Very brilliant; shooting rays to the zenith.
Hudson's Bay,	66 11	82 56	Very brilliant.
Chamisso Island,	66 18	161 47	Very brilliant.
At sea,	66 30	59	Seen in the south.
Kotzebue's Sound,	66 30	163	Aurora always seen to the northward.
Fort Macpherson,	67	135	Six auroras seen in fifteen days.

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TABLE II.—*Continued.*

Place.	Latitude.	Longitude.	Auroras.
	° ′	° ′	
Cape Krusenstern,	67 8	163 46	Shot up to the zenith; pink, purple, and green rays.
Fort Good Hope,	67 28	130 54	Spread all over the sky.
Behring's Sea,	68 30	167	Unusually brilliant display.
Hearne's Sea,	68 48	115 31	Most superb display.
Igloolik,	69 15	81 45	2 auroras in Nov.; 1 in Dec. 1822. Generally faint.
Baffin's Bay,	70 43	63 44	Faint aurora to southward.
Baffin's Bay,	71 20	62 28	Brilliant aurora.
Baffin's Bay,	72 10	68 36	Eleven auroras seen in Feb. 1851.
Somerset House,	72 48	95 41	Seldom seen in 1833.
Baffin's Bay,	72 49	70 59	Nine auroras seen in Jan. 1851.
Batty Bay,	73 17	91	Great luminous rays issued from the zenith.
Austin's winter quarters,	74 10	94 16	11 auroras in Jan. 1851; 12 in Feb. 1851; and 4 in March, 1851.
Lancaster Sound,	74 18	82 10	Eleven auroras in Dec. 1850.
Griffith Island,	74 30	95 20	Vastly inferior to those seen in more southern latitudes.
At sea,	74 31	111 38	Aurora faint.
Off Beechy Island,	74 40	92	Ten auroras seen in Nov, 1850.
Assistance Harbor,	74 40	94 16	Much less vivid than in more southern latitudes.
Barrow Strait,	74 45	94	Two auroras in Sept. 1850.
Wellington Channel,	74 54	93	Seven auroras seen in Oct. 1850.
Northumberland Sound,	76 52	97	Five auroras seen in Dec. 1852.

The following table exhibits the average annual number of auroras in Europe. The table is arranged in the same manner as Table I.

TABLE III.—*Average annual number of Auroras in Europe.*

Place.	Latitude.	Longitude.	Per year.	Richest month.	Years embraced.	Authority.
	° ′	° ′				
Bologna,	44 32	11 23 E.	4	6	24	Mairan Aurore Boreale, p. 505.
Paris,	48 50	2 20 E.	19		64	Gehler Wört., vii, 1, 135.
Montmorency,	49 0	2 19 E.	5	4	27	Cotte Meteorologie, 355.
Carlsruhe,	49 1	8 25 E.	8	5	11	Gehler Wört., vii, 1, 146.
Plymouth,	50 22	4 9 W.	6	5	21	Am. Journal, xxxiii, 298.
Leyden,	52 9	4 29 E.	26		29	Cotte Meteorologie, 355.
Berlin,	52 30	13 23 E.	4	5	28	Mairan Aurore Boreale, 500.
Franecker,	53 12	5 32 E.	25	8	7	Cotte Meteorologie, 355.
Kendal,	54 19	2 45 W.	31	10	7	Dalton's Met. Essays, 54–58.
Makerstoun,	55 35	2 31 W.	31		6	Am. Jour., [2], xi, 139.
Dunse,	55 47	2 20 W.	24	11	10	Phil. Trans. Abstracts, vi, 291.
Upsala,	59 52	17 38 E.	37	14	21	De la Rive Elec., iii, 301.
Christiana,	59 54	10 43 E.	33	13	16	De la Rive Elec., iii, 300.
St. Petersburgh,	59 56	30 18 E.	21	8	11	Mairan Aurore Boreale, 510.
Bossekop,	69 58	23 34 E.	143		1	Pouillet Physique, ii, 663.

The following observations are less definite than the preceding, and are therefore given in a separate table.

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TABLE IV.—*Notices of Auroras in Europe and its vicinity from Longitude* 60° *East to* 30° *West from Greenwich.*

Place.	Latitude.	Longitude.	Auroras.	Authority.
	° ′	° ′		
Teneriffe,	28 16	16 39 W.	Aurora seen Nov. 1837 for the first time in the memory of man.	Alfred Diston.
Shetland Islands,	60 20	1 9 W.	Constant attendant of clear evenings.	Rees' Cyc., v. iii.
At sea,	60 30	25 W.	A bright arch with coruscations.	Force, p. 13.
At sea,	61	25 W.	Passed through the zenith and eclipsed the moon in splendor.	Force, p. 14.
Iceland,	64	22 W.	Seen almost every clear night.	Henderson, p. 148.
Torneo,	65 52	24 13 E.	Frequently occupy the whole sky. Commonly seen in the north.	Maupertuis, ii, 155.

The following are the most definite observations I have been able to collect from the Asiatic continent.

TABLE V.—*Auroras in Asia and its vicinity from Longitude* 60° *East to* 170° *West from Greenwich.*

Place.	Latitude.	Longitude.	Auroras.	Authority.
	° ′	° ′		
Irkutsk,	52 20	103 50 E.	Two auroras seen in Dec. 1735.	Gmelin, p. 434.
Tomsk,	56 35	86 30 E.	Only one aurora per month in 1741.	Gmelin, p. 477.
Catherinenberg,	56 50	63 35 E.	Four auroras seen in 1854.	Kupffer Obs.
Turinsk,	57 45	63 45 E.	Three auroras seen in March 1742.	Gmelin, p. 326.
Kirenskoi Ostrog,	58 0	108 E.	Five auroras seen in March 1739.	Gmelin, p. 458.
Tobolsk,	58 12	68 18 E.	Not much more frequent than under the same latitude in Europe.	Erman, i, 394.
Jeniseisk,	58 30	92 E.	Three auroras seen in Feb. 1738.	Gmelin, p. 453.
Beresov,	63 56	65 4 E.	Sometimes seen for months together throughout the night.	Erman, i, 470.
Virchni Koorina,	66	152 E.	Constant and very brilliant.	Billings, p. 57.
Koliutchin Island,	67 26	175 35 W.	More frequent and brilliant than at Nijnei Kolymsk.	Von Wrangell, 506.
Nijnei Kolymsk,	68 32	160 56 E.	Seen almost every evening.	Von Wrangell, 83.
On the ice,	69 58	168 41 E.	An aurora of extraordinary beauty.	Von Wrangell, 103.
On the ice,	70 20	174 13 E.	Beautiful aurora all night.	Von Wrangell, 318.

If we project all the preceding observations upon a chart, we shall discover considerable uniformity in the distribution of auroras over the earth's surface. If we travel from the equator northward along the meridian of Washington, we find on an

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average near the parallel of 40°, only ten auroras annually. Near the parallel of 42°, the average number is 20 annually; near 45°, the number is 40; and near the parallel of 50° it amounts to 80 annually. Between this point and the parallel of 62°, auroras are seen almost every night. They appear high in the heavens, and as often to the south as the north. Further north they are seldom seen except in the south, and from this point they diminish in frequency and brilliancy as we advance towards the pole. Beyond lat. 62° the average number of auroras is reduced to 40 annually. Beyond lat. 67° it is further reduced to 20, and near lat. 78° to 10 annually. If we make a like comparison for the meridian of St. Petersburg, we shall find a similar result, except that the auroral region is situated further northward than it is in America; the region of 80 auroras annually being found between the parallels of 66° and 75°.

Upon the accompanying chart, the deep red color indicates the region where the average number of auroras annually amounts to at least 80; and the pale red color indicates the region where the average number of auroras annually amounts to at least 40. We thus see that the region of greatest auroral action is a zone of an oval form surrounding the north pole, and whose central line crosses the meridian of Washington in lat. 56°, and the meridian of St. Petersburg in lat. 71°. Accordingly auroras are more frequent in the United States than they are in the same latitudes of Europe. On the parallel of 45°, we find in North America an average of 40 auroras annually; but in Europe less than ten.

Geographical Distribution of Thunder Storms.

The geographical distribution of auroras is believed to be intimately related to the geographical distribution of thunder storms. I have therefore made a considerable collection of facts showing the average annual number of days of lightning at different points of the earth's surface. In the following table, column 4th shows the average annual number of days of lightning for the places named in column 1st. Column 5th shows the number of years embraced in the comparison; and column 6th shows the authority for the statements.

Table VI.—*Average number of days of Lightning annually.*

Place.	Latitude.	Longitude.	No. per year.	Y'rs embraced.	Authority.
	° ′	° ′			
Georgetown, Guiana,	6 49	58 11 W.	60	11	Brit. Guiana Met. Obs. 1846–56.
Ethiopia,	11	36 E.	82	5	Astron. Nachricht., 1204, 62.
Lima, Peru,	12 0	77 2 W.	0		Arago Met. Essays, p. 109.
Abyssinia,	13	37 E.	38	1	Arago Met. Essays, p. 129.
Madras,	13 4	80 16 E.	144	5	Madras Obs., 1841–45.
Central Africa,	12 to 18	4 E. to 4 W.	65	2	Barth's Central Africa.

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TABLE VI.—*Continued.*

Place.	Latitude.	Longitude.	No. per year.	Y'rs embraced.	Authority.
	° ′	° ′			
Martinique,	14 40	61 3 W.	39		Arago Met. Essays, p. 129.
St. Helena,	15 55	5 54 E.	0	3½	St. Helena Obs., 1840–43.
Guadeloupe,	16 12	61 45 W.	37		Arago Met. Essays, p. 129.
Bombay,	18 50	72 50 E.	42	5	Bombay obs., 1845–49.
Calcutta,	22 35	88 25 E.	60	1	Arago Met. Essays, p. 128.
Rio Janeiro,	23 0 S.	43 14 W.	50·6	6	" " " " "
Patna, India,	25 31	85 10 E.	53	1	" " " " "
Cairo,	30 6	31 26 E.	3·5	2	" " " " 134.
Buenos Ayres,	34 30 S.	58 28 W.	22·5	7	" " " " 130.
Gibraltar,	36 7	5 21 W.	6		Berghaus Annalen, 200, 6.
Athens,	37 58	23 44 E.	11	3	Arago Met. Essays, p. 133.
Palermo,	38 7	13 19 E.	13·5		Kämtz Meteorology, p. 362.
Smyrna, Asia,	38 23	27 11 E.	19	1	Arago Met. Essays, p. 130.
Washington,	38 53	77 0 W.	13·5	3	Gilliss's Met. Observations.
Maryland,	39 0	75 30 W.	41	1	Phil. Trans., 1759, p. 58.
Janina, Turkey,	39 47	20 59 E.	45	10	Kämtz Met., ii, 463.
Pekin, China,	39 53	116 40 E.	5·8	6	Arago Met. Essays, p. 134.
Philadelphia,	39 58	75 10 W.	10	3	Journal Franklin Institute.
New York,	40 42	74 0 W.	25·4	10	N. Y. Regents Rep., 1843–56.
Hudson, Ohio,	41 14	81 25 W.	13	7	Manuscript observations.
North Salem, N. Y.	41 20	73 38 W.	22·7	10	N. Y. Regents Rep., 1843–51.
Litchfield, Conn.,	41 45	73 8 W.	25·0	3	" " " 1851–52.
Amenia, N. Y.,	41 48	73 36 W.	18	1	" " " 1850, 241.
Rome, Italy,	41 54	12 25 E.	42·4	11	Kämtz, Met., ii, 463.
Rodez, France,	42 0	2 39 E.	11	1	Annuaire Met., 1851, p. 122.
Albany, N. Y.,	42 40	73 44 W.	21·6	7	N. Y. Regents Rep., 1840–49.
Marseilles, Fr.,	43 18	5 22 E.	9·3	9	Kämtz Met., ii, 463.
Toulouse, Fr.,	43 36	1 26 E.	17·1	8	Annuaire Met., 1851, 131. Arago Met. Essays, p. 131.
Toronto, C. W.,	43 39	79 21 W.	20	2	N. Y. Regents Rep., 1850, 371.
Lowville, N. Y.,	43 47	75 33 W.	12·7	3	N. Y. Regents Rep., 1840–43.
Newbury, Vt.,	44 6	72 5 W.	8·7	3	" " " 1854–56.
Orange, France,	44 7	5 50 E.	12·2	31	Annuaire Met., 1851, p. 167.
Somerville, N. Y.,	44 10	75 25 W.	17·3	3	N. Y. Regents Rep., 1850–52.
Padua, Italy,	45 24	11 52 E.	41·9	12	Kämtz Met., ii, 463.
La Rochelle, Fr.,	46 10	1 9 W.	21·0	8	Kämtz Met., ii, 455.
Geneva, Switz.,	46 12	6 10 E.	19	4	Gehler, 4, 2, p. 1585.
Bourg, France,	46 12	6 15 E.	37	1	Annuaire Met., 1851, p. 188.
Morges, Switz.,	46 31	6 28 E.	22·8	5	Bibliotheque Universelle, March 1860, p. 229.
Quebec, C. E.,	46 49	71 12 W.	23·3		Arago Met., p. 129.
Dijon, France,	47 19	5 2 E.	16	1	Annuaire Met., 1851, p. 84.
Viviers, France,	47 30	4 40 E.	24·7	10	Arago Met., p. 129.
Ofen, Hungary,	47 30	19 2 E.	28·0	11	Kämtz Met., ii, 459.
Tegernsee, Bav.,	47 30	11 32 E.	23·2	9	" " " 457.
Peissenberg, Bav.,	47 48	11 1 E.	23·0	12	" " " "
St. Andex, Germ.,	47 58	11 12 E.	27·1	12	" " " "
Denainvilliers, Fr.,	48 0	2 20 E.	20·6	24	Arago Met., p. 130.
Munich, Bav.,	48 8	11 35 E.	22·7	12	Kämtz Met., ii, 457.
Vienna, Austria,	48 12	16 20 E.	8·3	20	" " " "
Augsburg, Bav.,	48 21	10 54 E.	22·3	12	" " " "
Tubingen, Würt.,	48 32	9 2 E.	14·6	9	Arago Met., p. 132.
Lugan, Russia,	48 35	39 21 E.	35	1	Berghaus Annalen, 200, 5.
Strasbourg, Fr.,	48 36	7 46 E.	17	20	Arago Met., p. 131.
Giengen, Würt.,	48 37	10 15 E.	21·9	12	Kämtz Met., ii, 456.
Hohenfurth, Boh.,	48 37	17 40 E.	28		Berghaus Annalen, 200, 10.
Stuttgart, Würt.,	48 46	9 10 E.	20·6		Kämtz Met., ii, 456.

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TABLE VI.—*Continued.*

Place.	Latitude.	Longitude.	No. per year.	Y'rs em-raced.	Authority.
	° ′	° ′			
Versailles, France,	48 48	2 7 E.	15	1	Annuaire Met., 1851, p. 55.
Paris, France,	48 50	2 20 E.	13·6	51	Arago Met., p. 132.
Goersdorff, France,	48 57	7 48 E.	27	1	Annuaire Met., 1851, p. 181.
Regensberg, Bav.,	49 1	11 56 E.	16·9	10	Kämtz Met., ii, 457.
Rehberg, Boh.,	49 6	13 27 E.	29		Berghaus Annalen, 200, 10.
Metz, France,	49 7	6 10 E.	16	1	Annuaire Met., 1851, p. 109.
Tabor, Bohemia,	49 24	16 59 E.	5		Berghaus Annalen, 200, 10.
Rouen, France,	49 26	1 5 E.	13	1	Annuaire Met., 1851, p. 149.
Mannheim, Bav.,	49 28	8 27 E.	20·8	12	Kämtz Met., ii, 456.
Seelau, Boh.,	49 32	17 33 E.	12		Berghaus Annalen, 200, 10.
Brzesnitz, Boh.,	49 34	9 17 E.	27		" " " "
Deutschbrod, Boh.,	49 36	17 55 E.	16		" " " "
Würzburg, Bav.,	49 46	9 54 E.	13·9	8	Kämtz Met., ii, 427.
Brzezina, Poland,	49 49	15 58 E.	27		Berghaus Annalen, 200, 10.
Landskron, Boh.,	49 55	18 57 E.	11		" " " "
Tepel, Bohemia,	49 58	12 52 E.	11		" " " "
La Chapelle, Fr.,	50 0	3 35 E.	15·7	18	Arago Met., p. 131.
Eger, Bohemia,	50 5	14 42 E.	15		Berghaus Annalen, 200, 10.
Prague, Bohemia,	50 6	14 28 E.	17·7	10	Kämtz Met., ii, 457.
Smetschna, Boh.,	50 11	16 22 E.	11		Berghaus Annalen, 200, 10.
Königgrätz, Boh.,	50 13	18 10 E.	28		" " " "
Polpero, Eng.,	50 20	4 10 W.	10	23	Arago Met., p. 133.
Oberwiesenthal, Boh.	50 25	12 58 E.	15		Berghaus Annalen, 200, 10.
Hohenelbe, Boh.,	50 38	17 54 E.	20		" " " "
Altenberg, Boh.,	50 45	13 48 E.	15		" " " "
Tetschen, Boh.,	50 47	16 32 E.	17		" " " "
Brussels, Belg.,	50 50	4 30 E.	16·1	8	Kämtz Met., ii, 455.
Maestricht, Hol.,	50 51	5 42 E.	16·5	11	Arago Met., p. 131.
Zittau, Sax.,	50 54	14 48 E.	19		Berghaus Annalen, 200, 10.
Freiberg, Sax.,	50 55	13 20 E.	13		" " " "
Erfurt, Prussia,	50 58	11 2 E.	14·1	8	Kämtz Met., ii, 457.
Schluckenau, Boh.,	51 1	14 27 E.	14		Berghaus Annalen, 200, 10.
Dresden, Sax.,	51 3	13 44 E.	20		" " " "
Nertschinsk, Rus.,	51 18	119 20 E.	3·1	6	Kämtz Met., ii, 459.
Middleburg, Belg.,	51 28	3 37 E.	21·3	6½	" " " 455.
London, Eng.,	51 30	0 7 W.	8·3	13	Arago Met., p. 133.
Sagan, Prussia,	51 37	15 19 E.	29·3	12	Kämtz Met., ii, 457.
Münster, Pr.,	51 58	7 38 E.	29·7		Berghaus Annalen, 200, 5.
Utrecht, Hol.,	52 6	5 8 E.	15		Arago Met., p. 132.
Leyden, Hol.,	52 9	4 30 E.	13·5	29	" " " 133.
Irkutsk, Rus.,	52 17	104 17 E.	8·5	2	Kämtz Met., ii, 459.
Minden, Pr.,	52 18	8 55 E.	5·8		Berghaus Annalen, 200, 5.
Berlin, Prussia,	52 31	13 21 E.	17·3	120	Kämtz Met., ii, 457.
Lüneberg, Han.,	53 14	10 28 E.	20·2	20	" " " 456.
Barnaul, Russia,	53 20	83 27 E.	24	1	Berghaus Annalen, 200, 6.
Hamburg,	53 33	9 58 E.	10·7		Kämtz Met., ii, 456.
Cuxhaven, Germ.,	53 53	8 44 E.	11·5	10	" " " "
Braunsberg, Pr.,	54 20	19 54 E.	30·7		Berghaus Annalen, 200, 5.
Apenrade, Den.,	55 3	9 25 E.	19		" " " "
Tilsit, Prussia,	55 5	21 45 E.	12·6		" " " "
Slatoust, Russia,	55 11	59 45 E.	14	2	" " " "
Copenhagen, Den.,	55 41	12 35 E.	1	4	Gehler Wört., v. 4, 2, p. 1585.
Memel, Prussia,	55 42	21 6 E.	4	26	Berghaus Annalen, 200, 5.
Moscow, Russia,	55 47	37 44 E.	22·4	9	Kämtz Met., ii, 459.
Kasan, Russia,	55 52	49 30 E.	9	1	" " " "
Jekaterinenburg, Rus.	56 50	60 34 E.	23·3	3	Berghaus Annalen, 200, 5.

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TABLE VI.—*Continued.*

Place.	Latitude.	Longitude.	No. per year.	Y'rs embraced.	Authority.
	° ′	° ′			
Skara, Sweden,	58 22	12 28 E.	9·2	25	Kämtz Met., ii, 461.
Stockholm, Sweden,	59 21	18 4 E.	9·3	10	" " " "
Spydberga,	59 38	9 E.	7·7	8	" " " "
Bogoslowsk, Rus.,	59 45	59 59 E.	2	1	Berghaus Annalen, 200, 6.
Petersburg, Rus.,	59 52	30 25 E.	12·4	10	Kämtz Met., ii, 459; Arago.
Abo, Russia,	60 15	22 10 E.	10	12	" " " "
Bergen, Nor.,	60 24	5 18 E.	5·8	7	" " " 461.
Sondmör, Nor.,	62 30	6 20 E.	3·9	12	" " " "
Archangel, Rus.,	64 32	40 33 E.	6·5	18	Berghaus Annalen, 200, 5.
Reikiavik, Iceland,	64 8	22 0 W.	½	2	Arago Met., p. 111.
Fort Franklin,	65 12	123 12 W.	1	2	" " " "
Baffin's Bay,	65 30	80 W.	1	1	" " " 110.
Melville Island,	74 47	110 49 W.	0	2	" " " "
At sea,	75	100 W.	0	1	" " " "

It is obvious from an examination of the preceding table, that thunder storms prevail most frequently in the equatorial regions and diminish as we proceed towards the poles. It is also evident that the frequency of thunder showers depends upon other circumstances than simply latitude; but without stopping to enquire what these circumstances are, we will take the average of the observations included between different parallels of latitude. We thus find that:—

between lat. 0° and lat. 30°	the average number of thunder storms annually is	51·6
" "30 " " 50		19·9
" "50 " " 60		14·9
" "60 " " 70		4·0
beyond "70		0·0

Maury's storm and rain charts of the Atlantic Ocean furnish most important information on the same subject. The following table presents a summary of the results of these charts. The ocean is divided into squares by parallels of latitude drawn at intervals of five degrees from each other, and meridians of longitude at intervals of five degrees. Each square of the following table contains three numbers. The first shows the number of days of observation within the given square; the second shows the number of days of lightning reported, and the third is the number of days of lightning which would occur in one year, as computed from the numbers actually observed. Thus in the square included between the parallels of 30 and 35 degrees of north latitude, and between the meridians of 65° and 70°, the first number is 548, which shows that 1644 observations have been obtained in that square. Each observation represents a period of 8 hours, so that 1644 observations represent 548 days. During this period, lightning was reported on 44 days, which is at the rate of 29 cases for one year.

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TABLE VII.—*Days of Lightning in the Atlantic Ocean, by Maury's charts.*

Lat.	80°	75°	70°	65°	60°	55°	50°	45°	40°	35°	30°	25°	20°	15°	10°	5° W.	0°	5° E.	10°	15°	20°
60°	—	—	—	—	—	—	—	—	—	—	—	20	34	41	39	26	10	—	—	—	—
												0	1	0	1	0	0				
												0	11	0	9	0	0				
55	—	—	—	—	—	—	—	—	50	140	170	231	283	311	420	464	194	—	—	—	—
									0	2	1	6	6	2	5	4	4				
									0	5	2	9	8	2	4	3	7				
50	—	—	—	—	42	96	306	414	523	579	542	513	493	437	305	—	104	—	—	—	—
					1	0	5	7	7	11	8	11	5	5	2		0				
					9	0	6	4	5	7	5	8	4	4	2		0				
45	—	607	1083	824	893	806	621	527	373	244	132	90	56	43	22	—	—	—	—	—	—
		14	48	56	46	37	24	23	14	5	5	0	0	0	0						
		7	16	25	19	17	14	16	14	8	14	0	0	0	0						
40	81	1398	991	599	464	370	248	160	116	81	114	89	101	113	111	75	—	—	—	—	—
	1	70	81	40	24	22	12	3	1	0	7	3	0	1	0	0					
	5	18	29	24	19	22	18	7	3	0	22	12	0	3	0	0					
35	511	755	548	255	241	287	329	298	249	131	58	43	78	26	1	—	—	—	—	—	—
	41	42	44	7	17	8	11	13	17	2	0	1	1	0	0						
	29	20	29	10	26	10	12	16	25	5	0	8	5	0	0						
30	648	464	379	316	131	117	188	242	319	221	70	51	29	—	—	—	—	—	—	—	—
	33	15	24	23	12	7	6	4	29	6	2	0	0								
	19	12	23	27	33	22	12	6	33	10	10	0	0								
25	105	120	87	217	212	89	87	151	268	221	113	45	5	—	—	—	—	—	—	—	—
	5	6	1	8	2	1	0	2	8	2	0	0	0								
	17	17	4	14	4	4	0	4	11	3	0	0	0								
20	81	107	51	61	153	180	109	101	150	237	213	53	2	—	—	—	—	—	—	—	—
	2	9	2	5	1	3	3	3	0	5	0	1	0								
	9	31	14	30	2	6	10	11	0	8	0	7	0								
15	22	0	18	18	32	129	198	169	138	222	278	207	75	—	—	—	—	—	—	—	—
	0	0	0	0	0	4	4	18	9	13	2	3	3								
	0	0	0	0	0	11	7	39	24	21	3	5	15								
10	—	—	—	—	—	—	96	223	211	246	556	336	161	239	27	23	18	—	—	—	—
							1	6	16	14	42	12	1	6	0	0	0				
							4	10	28	21	27	13	2	9	0	0	0				
5 N.	—	—	—	—	—	—	32	140	204	335	668	421	112	36	121	78	33	74	—	—	—
							0	8	4	13	40	32	6	3	3	9	1	3			
							0	21	7	14	22	28	19	30	9	42	11	15			
0	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—

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								28 0 0	545 11 7	817 13 6	1182 15 5	680 12 6	193 3 6	30 0 0	20 0 0	5 0 0	9 0 0	4 0 0		
5 S.									91 0 0	1365 12 3	733 2 1	558 0 0	383 0 0	100 0 0	6 0 0	2 0 0	6 0 0	5 0 0	8 0 0	
10									337 5 5	1260 5 1	481 0 0	138 0 0	266 0 0	479 0 0	251 0 0	11 0 0	5 0 0			
15									1314 14 4	1171 5 2	604 1 1	121 1 3	51 0 0	101 0 0	457 0 0	548 0 0	64 0 0	13 0 0		
20								438 21 17	1108 28 9	930 16 6	858 5 2	257 2 3	65 0 0	23 0 0	52 3 20	287 0 0	523 0 0	121 0 0	10 0 0	
25							295 13 16	731 37 18	741 30 15	706 13 7	597 12 7	400 1 1	198 1 2	71 0 0	47 0 0	77 0 0	154 0 0	523 4 3	187 0 0	5 0 0
30						279 29 38	802 26 12	799 27 12	577 16 10	416 19 17	334 2 2	552 4 3	502 6 4	392 1 1	299 0 0	280 1 1	299 1 1	656 2 1	1077 5 2	500 9 6
35					168 19 41	682 37 20	1119 67 22	773 9 4	495 9 7	218 0 0	348 0 0	715 1 1	523 8 6	688 6 3	695 0 0	876 2 1	814 0 0	818 0 0	619 1 1	992 5 2
40				268 7 10	727 33 17	750 36 17	658 9 5	515 10 7	157 2 5	33 0 0	18 0 0	33 0 0	3 0 0	33 0 0	96 0 0	292 0 0	127 0 0	163 0 0	53 0 0	47 0 0
45			66 1 6	692 27 14	595 10 6	466 10 8	311 1 1	55 0 0	10 0 0	6 0 0	0	0	0	0	2 0 0	2 0 0	0	1 0 0	1 0 0	1 0 0
50			236 2 8	927 11 4	560 2 1	316 0 0	36 0 0	3 0 0	3 0 0	0	0	0	0	0	0	0	0	0	0	0
55			1469 15 4	866 11 5	135 0 0	9 0 0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
60																				

We see in this table abundant evidence that the frequency of lightning depends upon other circumstances than simply latitude. Throughout the western half of the Atlantic Ocean, lightning occurs *three times* as frequently as throughout the eastern half of that ocean. If we take the average of all the observations upon the same parallel of latitude, we shall find the number of days of lightning to be as follows:

	North Atlantic.	South Atlantic.
From lat. 0 to lat. 5°	20	6
" " 5 " 10	17	2
" " 10 " 15	14	1
" " 15 " 20	8	2
" " 20 " 25	7	6
" " 25 " 30	19	9
" " 30 " 35	19	7
" " 35 " 40	19	6
" " 40 " 45	15	9
" " 45 " 50	5	8
" " 50 " 55	5	3
" " 55 " 60	4	4

The average frequency of lightning in the North Atlantic is *two and a half times* as great as in the South Atlantic; also the average frequency of lightning on land, at least in the northern hemisphere, appears to be *more than twice* as great as in the North Atlantic. The bearing of the preceding facts upon the theory of auroras will be considered hereafter.

ART. XII.—*On the Products of the Distillation of Common Rosin;* by JAMES SCHIEL, of St. Louis.

THE chemical process taking place in the distillation of common rosin or colophony, and the two different liquids which are obtained by that distillation, have as yet escaped the attention of chemists; the whole process is almost completely wrapped in darkness. The two liquids just mentioned are distinguished as *essence of rosin* (in Europe "German oil of turpentine") and *rosin oil;* the former is thin, of a light yellow color, and a strong, almost aromatic odor; the latter is a thick liquid of a somewhat disagreeable odor.

The *essence of rosin* is a mixture of two substances, which may be separated by fractional distillation if often repeated. During this distillation the liquid passing over sometimes assumes a milky aspect, produced by the formation of a small portion of water and a trace of acetic acid; a small piece of lime or baryta instantly renders the liquid perfectly transparent. Of the two liquids into which the essence of rosin separates, the first is very thin, perfectly colorless and transparent, and of a strong, refracting power. I call it *colophonon.* The specific gravity of

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colophonon is 0·84 at 14° C., its boiling point 97° C. (Barom. ·756); with concentrated sulphuric acid it forms a brown liquid from which water separates a green oil, having much the smell of Ol. anthos. Chlorhydric acid produces a similar transformation. With potassium it gives rise to a lively emission of gas and coagulates into a brown-yellow mass. Heated in a closed vessel above the boiling point it colors brown and assumes an odor of peppermint.

The composition of colophonon is expressed by the formula

$$C_{11}H_{18}O_2,$$

One hundred parts consist of

	Calculated.	Found.		
Carbon,	79·52	78·90	78·95	79·50
Hydrogen,	11·85	11·59	11 61	12 01
Oxygen,	9·63	9·51	9·44	9 49
	100·00	100·00	100·00	100·00

The determination of the specific gravity of (vapor of) colophonon gave the number 5·1, which however I consider merely as an approximation. It is remarkable, that this substance, whose composition is distinguished from that of *phoron* by C_2H_4 should have a boiling point as low.

The second constituent of the essence of rosin has its boiling point at 160° C., and possesses all the chemical properties of oil of turpentine. The analysis of it showed it to be composed of

Carbon,	87·44
Hydrogen,	11·78

The formula of oil of turpentine $C_{10}H_{16}$ requires

Carbon,	88·23
Hydrogen,	11·77
	100·00

As it does not seem to have any effect on the plane of polarization, it may be identified with the *therebène* of Deville.

The oil of rosin, with a boiling point above that of mercury, does not seem to have a constant composition; at least, there is sometimes a difference between the raw oil and the refined oil. The raw oil, which has the property of fluorescence, loses this property in a great degree by a very simple refining process. This process consists simply in placing the earthen vessels containing the raw oil on the hot wall around the neck of the iron retort, from which the rosin is distilled, and allowing them to stand there for some hours, keeping them covered up. A portion of the raw oil that had been heated in the water-bath in contact with caustic lime showed the composition $C_{20}H_{28}O_2$ of the *resineine* of Deville and Frémy, viz:

	Calculated.	Found.
Carbon,	84·51	84·70
Hydrogen,	9·80	9·69
Oxygen,	5·69	5·61
	100·00	100·00

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A portion of the refined oil, treated in the same way, gave the formula $C_{15}H_{20}O_2$, viz:

Carbon,	86·44	86·25
Hydrogen,	9·62	9·95
Oxygen,	2·94	2·77
	100·00	100·00

Further investigation will show whether this difference is purely accidental, and there is good reason to believe it to be so; or whether there is really a difference in the composition of the two products.

The gases formed during the distillation of the rosin burn with a bright light and may be used for illumination. I have analyzed a portion of these gases, collected in glass tubes which were sealed up with the blowpipe, in the gas-room of Prof. Bunsen. They contain 14·96 p. c. carbonic acid, 11·45 p. c. of oxyd of carbon, 5·89 p. c. ethylene and butylene (ditetryl), besides oxygenated nitrogen very nearly in the proportion of 1 : 6. Towards the close of the distillation the oxygen nearly disappeared, and a portion of light carburetted hydrogen made its appearance.

I have to remark that it must be left undecided whether there may not be an admixture of some propylene with the above 5·89 parts of ethylen and ditetryl, as we do not yet possess any means of analyzing these homologous gases. The analysis of the three homologous gases, ethylene, propylene and butylene, would furnish the equations:

$$x+ \quad y+ \quad z=V$$
$$2x+ \quad 3y+4z=B$$
$$2x+2{\cdot}5y+3z=C$$

V being the volume analyzed, B the volume of carbonic acid formed by the combustion, and C the contraction. From the first of these equations we have $x=V-z-y$, and this value of x introduced into the two other equations, makes them

$$y+2z=B-2V$$
$$2y+4z=C-2V$$

The second of these is evidently the half of the first and $C=\frac{1}{2}B$. For the determination of y and z we therefore have only *one* equation. If there is a large quantity of the three gases to be disposed of, they may be absorbed by sulphuric acid and the alcohols of the two first may be distilled off after dilution with water.

By sending a current of common coal-gas slowly for an entire week through a number of flasks containing sulphuric acid, then diluting with water and distilling; I found on the surface of the water distilled over, a light oily substance, collecting in large drops, and having a strong, disagreeable odor. It is very likely that to this substance dissolved in the odorless parts of the gas is due the offensive odor of coal-gas. I hope yet to obtain enough of this oily substance for an analysis.

Heidelberg, May 1, 1860.

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ART. XIII.—(1.) *An account of the fall of Meteoric Stones at New Concord, Ohio, May 1st*, 1860; by Prof. E. B. ANDREWS, of Marietta College. With (2.) *Computations respecting the Meteor;* by Prof. E. W. EVANS, of the same Institution. *To which are added further notices of the same by* D. W. JOHNSON, Esq. and Dr. J. LAWRENCE SMITH.

(1.)

ABOUT fifteen minutes before one o'clock on the first day of May, 1860, the people of southeastern Ohio and northwestern Virginia were startled by a loud noise, which was variously attributed to the firing of a heavy cannon, to the explosion of steamboat boilers, to an earthquake, and to the explosion of a meteor. In many cases houses were jarred. To persons within doors the noise generally seemed as if produced by the falling of a heavy soft body upon the chamber floor. Many persons heard a rumbling reverberation which continued for a few seconds. The area over which this explosion was heard was probably not less than one hundred and fifty miles in diameter. At Marietta, O., the sound came from a point north or a little east of north. The direction of the sound varied with the locality. An examination of all the different directions leads to the conclusion that the central point, from which the sound emanated, was near the southern part of Noble county, Ohio.

At New Concord, Muskingum Co., where the meteoric stones fell, and in the immediate neighborhood, there were many distinct and loud reports heard. At New Concord there was first heard in the sky, a little southeast of the zenith, a loud detonation, which was compared to that of a cannon fired at the distance of half a mile. After an interval of ten seconds another similar report. After two or three seconds another, and so on with diminishing intervals. Twenty-three distinct detonations were heard, after which the sounds became blended together and were compared to the rattling fire of an awkward squad of soldiers, and by others to the roar of a railway train. These sounds, with their reverberations, are thought to have continued for two minutes. The last sounds seemed to come from a point in the southeast 45° below the zenith. The result of this cannonading was the falling of a large number of stony meteorites upon an area of about ten miles long by three wide. The sky was cloudy, but some of the stones were seen first as "black specks," then as "black birds," and finally falling to the ground. A few were picked up within twenty or thirty minutes. The warmest was no warmer than if it had lain on the ground exposed to the sun's rays. They penetrated the earth from two to three feet. The largest stone, which weighed 103 lbs., struck the earth at the foot of a large oak tree, and after cutting off two roots, one five inches in diameter, and grazing a third root,

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it descended two feet ten inches into hard clay. This stone was found resting under a root which was not cut off. This would seemingly imply that it entered the earth obliquely. It is said that other stones which fell in soft ground entered the earth at a similar angle. They must have been flying in a northwest direction. This fact, added to the other facts, that the detonations heard at New Concord came lower and lower from the zenith toward the southeast, and that the area upon which the stones fell extends with its longer axis in a southeast and northwest direction, would imply that the orbit of the meteor, of which these stones are fragments, extended from southeast to northwest. This conclusion is confirmed by the many witnesses who saw, at the time, a luminous body moving in the same direction. It is a fact of some interest that the larger stones were carried by the orbital force further than the small ones, and were found scattered upon the northwest end of the area referred to. This fact is readily explained by the larger proportional surface presented to the atmospheric resistance in the smaller stones. The stones thus far found vary in weight from a few ounces to over a hundred pounds. They show a decided family resemblance. All are coated with a black crust and show a bluish gray feldspathic interior with numerous brilliant points of nickeliferous iron. Although in some instances the edges remain quite sharply

1.



defined, generally they show that they have been rounded by fusion. The accompanying figure shows the appearance of the larger stone now in the cabinet of Marietta College. Viewed from most positions this stone is angular and appears to have

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been recently broken from a larger body. On one side it is much rounder and smoother, and this (the outer surface in the figure) appears to be a part of the original surface of the main meteor. Two of its edges extend more than a foot in length, and two of its diameters are fourteen inches. In the small stones the edges are, I think, more rounded than in the larger ones. The angle at A, fig. 2, is an exact copy of a specimen in my possession. The dotted line shows the thickness of the crust.

2.

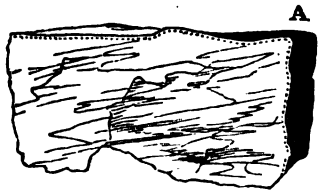
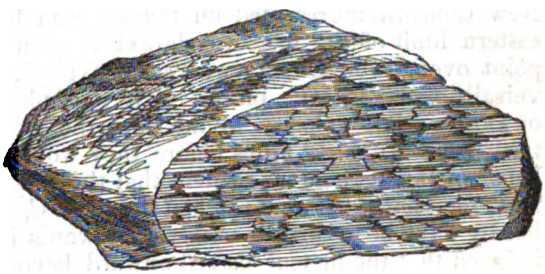


Figure 3 represents the appearance of a small stone, one side of which shows a surface only partially glazed. There was evidently a flaw in this little meteorite, and the heat entering the crack was only sufficient to fuse the surface in a very slight degree. The heat apparently penetrated the crack in straight lines as if driven backward by the high velocity. The edge of the stone surrounding this peculiar surface is a feather edge made by the melting of the metallic crust in an unusual manner.

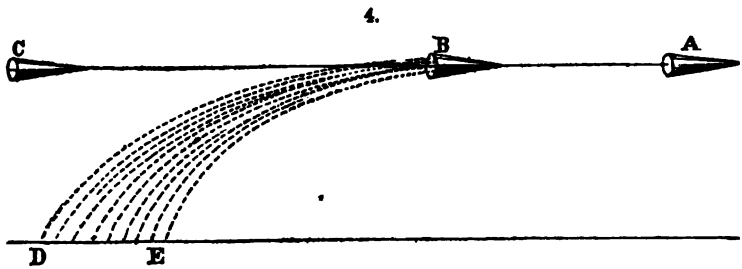
3.



In the examination of this interesting meteoric phenomenon, I am led to believe that the people of New Concord and in the immediate vicinity of the district where the stones fell, heard different sounds, and consequently of different origin, from those heard by people living at a greater distance. The former heard many distinct detonations followed by a rumbling roar like that of thunder. The latter heard but a single explosion followed by a somewhat similar rumbling noise but less distinct. This explosion seemed to take place at a point in the air over the southern part of Noble County. The people of the northern part of the same county heard it in a southern or southeastern direction, and *not in a northwestern direction towards New Concord.* This fact would indicate that the great explosion which was heard more than seventy-five miles away, took place in Noble County, and that the several distinct detonations heard at and near New Concord were directly connected with the falling of the several stones in that district. A diagram will illustrate this and also one or two other points.

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By the careful computations of Prof. Evans the meteor passed in its orbit from the point A over Newport, Washington Co.,



Ohio, to the point C, which is quite as far to the northwest as New Concord represented on the diagram by D. The southeastern limit of the meteoric shower is shown by E. B is the point over Noble County where the explosion was almost universally *heard* to take place. If it took place there, then the onward orbital velocity would take the stones forward from twenty-five to thirty miles before they would drop to the earth between D and E. It is evident that the stones were thrown off from the meteor before it reached the point C, as the stones could hardly have been thrown backward towards E. It is also to be inferred that the meteor passed onward beyond C, although the clouds prevented further observations in that direction. The detonations heard between D and E must be accounted for by the rapid passing of the several stones through the lower atmosphere.

(2.) *Prof. Evans's Computations.*

Owing to the cloudy state of the atmosphere, the time was unfavorable for observing such facts as are necessary for the accurate determination of the height of the meteor, the direction of its path, its size and its velocity. After careful investigation, however, the following results have been obtained.

1. *Direction of its path.*—The district along which the meteorites are known to have fallen is about ten miles long and from two to three miles wide, extending in a northwesterly direction from a little west of the village of Point Pleasant, in Guernsey County, to within a mile of New Concord, in Muskingum Co.* The fragments fell with a northwesterly inclination. This is proved both by the testimony of those who saw them descend, and by the direction in which they were subsequently found to have penetrated the earth. As the sky along this district was overcast with clouds, the main body of the meteor was not seen

* As nearly as can be made out from the data, the path of the meteor appears to have crossed the Ohio River in lat. 39° 30′, long. 81° 20′, and to have disappeared in lat. 40° 2′, long. 81° 90′.—Eds.

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by those who witnessed the fall of the fragments; but the sounds, as heard by them, first proceeded from the zenith, and gradually receded towards the southeast. This seemingly contradictory fact agrees perfectly with the hypothesis that the course of the meteor was northwesterly; for if it approached with a velocity greatly exceeding the velocity of sound, the explosions which occurred last must have been the first heard. At some distant stations towards the south and west the view was not wholly obstructed by clouds; and there are many witnesses who relate that a few minutes before any report was heard, they saw a large ball of fire pass across the northern sky, towards the northwest. By tracing out the line along which the reports were loudest and seemed to proceed from the zenith, I am led to the conclusion that the meteor passed over the eastern end of Washington County, then across the interior of Noble County, then over the southwestern corner of Guernsey and the northeastern corner of Muskingum, with a direction of about 42° west of north.

2. *Its height above the earth.*—Mr. William C. Welles of Parkersburg, Virginia (lat. 39° 10′, long. 81° 24′), a gentleman of liberal education, testifies that being about three miles east of that place at the time of the occurrence, he happened to look up to the northeast of him, and saw a meteor of great size and brilliancy, emerging from behind one cloud and disappearing behind another. When about 35° east of north he thinks its altitude was 65°. Now the distance, in a direction 35° east of north, from his station to the line directly under the meteor's path, is 20 miles. Calculating from these data I find for the vertical height, taken to the nearest unit, 43 miles. This was at a point in Washington County near the border of Noble.

Mr. C. Hackley testifies that he saw the meteor from Berlin in Jackson County. It crossed a cloudless space in the northeast, and he thinks its altitude, at the highest point, was 30°. Now the distance from Berlin to the nearest point under the meteor's path is 70 miles. These data give nearly 41 miles for its vertical height over Noble County, a few miles to the south of Sarahsville (lat. 39° 53′, long. 81° 40′).

Many other reliable witnesses have been found who saw the meteor through openings in the clouds from various points west of its path; and whose testimony so far agrees with the foregoing as to give results ranging between 37 and 44 miles. Care has been taken as far as possible to verify the data in each case by personal examination of the witnesses. The angles have in most instances been taken as pointed out by them from their respective posts of observation. It is unfortunate that no case has come to our knowledge in which the meteor was seen from the region east of its path. But it was a circumstance in some

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respects favorable to the definiteness of the observations made from the west side, that the observers in nearly all cases saw the meteor only at one point, or within a very small space on the heavens. It is impossible to reconcile the various accounts without granting that its path was very nearly as above described, and that its height did not vary far from 40 miles as it crossed Noble County.

In regard to the time which intervened, at different places, between seeing the fire-ball and hearing the report, the statements are so vague that not much reliance has been placed upon them. It may be remarked, however, that they will essentially agree with the foregoing conclusions, if we suppose that the loudest explosion took place in the southern part of Noble County.

I will add under this head the statement of Mr. Joel Richardson, of Warren, Washington County, who from a place six miles west of Marietta, saw the meteor as much as 15° or 20° west of north, at an altitude of about 45°. The direction in this case was so oblique to the meteor's path, that the data are of little value for simply determining the height; but they are important on account of their connection with the place of the meteor's last appearance. Mr. Richardson was visited by the writer, and his testimony was subjected to close scrutiny. If we take the azimuth at 15° west of north, we shall have a distance of 41 miles to the line under the meteor's path; and these data will give about 41 miles for its vertical height over a point not more than a mile from New Concord, at the extreme western limit of the district along which the meteorites were scattered. If we take the azimuth at 20° west of north, both the distance and the height will be greatly augmented. I have found two persons living near Bear Creek, nine miles north of Marietta, who make statements closely corroborating that of Mr. Richardson.

3. *Velocity of the meteor.*—Mr. Welles furnishes data from which we can now determine approximately the meteor's rate of motion. As this gentleman is somewhat accustomed to astronomical observation, his judgment as to angles may be strongly relied upon. He thinks he saw the meteor pass from a point 50° east of north to a point 20° east of north in about three seconds. These two points in the meteor's path are over the townships of Newport in Washington County, and Elk in Noble County. The distance between them is 12 miles. According to these data, then, its relative velocity was about four miles a second. No other statement regarding the velocity has been obtained that is sufficiently definite to be of any value.

4. *Its size and shape.*—Those who saw the fire-ball from stations not less than 20 and not more than 30 miles to the westward, agree in stating that it appeared as large and as round as

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the full moon. Its intense brilliancy may have produced exaggerated conceptions of its size. But if we take the minimum apparent diameter of the moon, and the minimum distance of the meteor (its height being assumed as 40 miles) we shall have for its diameter 38-hundredths, or about three-eighths of a mile.

The train is described as a cone, having its base upon the fireball. As seen from near Parkersburg its length was estimated at twelve times the diameter of the ball. The part next the base appeared as a white flame; but not so bright as to render the outline of the ball indistinct. About half way toward the apex it faded into a steel blue.

(3.) *Notice of the Fall from* D. M. JOHNSON, Esq., *of Coshocton, O.*

[Mr. Johnson's notice of this shower of meteoric stones is the result of a visit to the locality a few days after the event. We copy the following particulars from Mr. Johnson's account as giving circumstantial detail and historic interest to the facts recounted by Profs. Andrews and Evans. Mr. Johnson also adds a chemical analysis of the meteor.—EDS.]

Two carpenters, Samuel L. Hines and Samuel M. Noble, were at work near the house on the farm of Jonas Amspoker, of New Concord. Upon hearing the first report they looked up and saw two dark looking objects, apparently about the size of an apple, come through a cloud, producing a twirling motion in the vapor of the cloud. One of them they saw fall to the ground about one hundred and fifty yards from where they stood. The other one passed behind the house out of their sight. They went immediately to the one which they saw strike the ground, and found it at the bottom of a hole two feet deep. When taken out it was still warm and in a few seconds dried the moist earth adhering to its surface. It was found to weigh 51 pounds.

Nathanael Hines, who was ploughing in a field adjoining Mr. Amspoker's place, heard a report like the blasting of rocks in a well, followed by several smaller reports. He looked up and saw a black body descending to the earth at an angle of about 30° to the vertical. It struck the ground about two hundred yards from him. Repairing to the place he found that in its descent it came in contact with the corner of a fence, breaking off the ends of the three lower rails, and entering the ground about eighteen inches. It was warm, and had a sulphurous smell. This stone was not weighed, but it is estimated to have been between 40 and 50 pounds in weight before any portions were broken off from it. This was probably the stone that the carpenters saw but lost sight of when it passed behind the house.

James M. Reasner was in his house at the time of the explosion, but hearing a noise like striking against the door with the fist he went out, when his attention was attracted by a whizzing

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sound over head. Looking up he saw what appeared to be a black streak descending in a slanting direction towards the earth. After he heard that stones had fallen in that vicinity he sought for and found a stone weighing 36½ pounds.

Wm. Law was in his house, one mile east of Concord. Upon hearing the first report he went out into the yard. He heard a buzzing sound passing over the house in a northwest direction, and saw the sheep running in a field not far from the house. Hearing that stones had fallen he went to the field in which the sheep were and found a stone weighing 53 pounds. It had fallen upon the end of a partially decayed log, through which it passed and buried itself it the ground. This was the largest stone that had been found at the time I visited the district. But I since learn that the one described by Prof. Andrews was found after my visit to the place.

A *blazing* meteor was seen from other parts of the state on the same day. The Columbus Statesman of May 5th says that "near McConnellsville several boys observed a huge stone descend to the earth, which they averred looked like a red ball, leaving a line of smoke in its wake." McConnellsville is 25 miles south of Concord.

Mr. D. Mackley of Jackson Co., in a communication to the Cincinnati Commercial, says, "On the first day of May, at precisely half past 12 o'clock, I was standing on the platform at the railroad station in Berlin when I saw, in a northeast direction, a ball of fire, about 30° above the horizon. It was flying in a northerly direction with great velocity. It appeared as white as melted iron, and left a bright streak of fire behind it which soon faded into a white vapor. This remained more than a minute, when it became crooked and disappeared." Berlin is about 80 miles southwest of Concord.

The meteor seen from McConnellsville and Berlin was undoubtedly the same that exploded and fell in Guernsey County. No one of the many persons who saw the stones fall and were in the immediate vicinity at the time, noticed anything of the luminous appearance described by those who saw it from a distance.

All the stones that I have yet seen have the same general appearance. They are irregular blocks, and are covered with a very thin black crust, which looks as if it had been fused. The edges of the blocks are not sharp but rounded, and the faces present the usual pitted appearance of meteorites. They absorb water with a hissing sound. The fragments are of a bluish gray color. Under the lens five substances can be detected. A snow-white mineral is largely disseminated throughout the mass. A clearer white mineral can be distinguished in some specimens. Metallic grains are quite numerous, a yellowish brown mineral

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in patches, and black particles scattered over the surface. One specimen had very thin veins of a shining black mineral. When in large masses the stone is exceedingly tough, requiring repeated blows with a hammer to fracture it, but when broken into small pieces it can be crushed with ease in an agate mortar.

The specific gravity of the mass was found to be 3·5417. On analysis one gramme of the stone was found to contain:—

Silica,	·51250
Protoxyd of iron,	·25204
Magnesia,	·08873
Alumina,	·05825
Lime,	·00785
Iron,	·08803
Nickel,	·02360
Sulphur,	·01184
Chromium,	*trace.*
Phosphorus,	*trace.*
Water,	·00035
	1·03819

Cosbocton, Ohio, June 4th, 1860.

(4.) Prof. J. LAWRENCE SMITH, M.D., of *Louisville, Ky.*, informs us that, on hearing vague rumors of the event two days after its occurrence (reported as an earthquake accompanied by a fall of stones), and although four hundred miles distant from the place, he immediately visited New Concord and obtained all possible data respecting the phenomenon. He is convinced from a thorough examination of the facts that no fall of meteoric stones before recorded possesses so many points of interest as the one in question, surpassing even the far famed fall at *L'Aigle*. He reserves many details of the event with his chemical examination for a paper in the next number of this Journal. The analyses, so far as they are complete, show the constitution of the New Concord stones to be identical with those that fell about the same time last year, March 28, 1859, in Indiana (see this Jour., xxviii, 409).

Louisville, Ky., June 6, 1860.

Thus far about thirty stones have been recovered from this fall, and one witness estimates the entire weight of the fragments discovered at about seven hundred pounds.

The fine specimen (figure 1) secured by Prof. Andrews for Marietta College, is, we believe, the largest meteoric fragment hitherto recorded as existing in one piece. Profs. Silliman and Kingsley estimated the weight of a fragment of the Weston meteorite (1807), which was dashed in many pieces by falling on a rock, as about 200 pounds.—Mem. Conn. Acad, i, 149.

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ART. XIV.—*Review of Dr. Antisell's Work on Photogenic Oils, &c.*

[The following Review of Dr. Antisell's book on Photogenic Oils has been for some months in type waiting an opportunity when our other engagements would permit its publication. It will amply repay the careful perusal of all who are interested in this important practical subject.—EDS.]

REVIEW.

1. *The Manufacture of Photogenic or Hydro-Carbon Oils from Coal and other Bituminous Substances capable of supplying Burning Fluids*; by THOMAS ANTISELL, M.D., Professor of Chemistry in the Medical Department of Georgetown College, D. C., etc. etc. New York and London: D. Appleton & Co. 1859. pp. 144.—In entering an earnest protest against the work before us, we would not have our motives misunderstood. We are not of those who would condemn a book solely on the ground that it is "not so good as it should be," and will not therefore urge this objection against the effort of our Author, although it would be hard to find a case to which the charge would more forcibly apply. But we do condemn most heartily the presumption of the man who in these days attempts to write a handbook upon any scientific or technological subject with which he is not somewhat familiar. We believe, moreover, that errors, either of omission or of commission—accidental or intentional—in scientific writings, which exceed the well-understood conventional limits of *tolerance*, should not be allowed quietly to pass without correction.

Dr. ANTISELL, from his position of chemical examiner in the Patent Office at Washington, has naturally had a rare opportunity of familiarising himself with the recent improvements which have been made—or claimed—in the manufacture of coal oils. In the work in question, he has published an index of these, which cannot but be acceptable to all who are interested either in the practical or scientific consideration of the subject. Had this list been published by itself, or had it been incorporated with a portion of the materials which Dr. A. has now exhibited, in an article, or a short series of articles, in some one of our scientific or technological magazines, it would have been most gratefully received, and, we doubt not, widely copied. Diluted and scattered as this information has been, however, that it might fill a volume, its value has been lessened in no slight degree.

We have endeavored, in vain, to make out the point of view from which the Author regarded his subject. Claiming the attention of all persons engaged in the manufacture of liquid products from the distillation of mineral combustibles, his work is nevertheless not a didactic one. In it scarcely any attempt is made to instruct the manufacturer, either by a clear enunciation of general principles to be followed, or of special details to be observed in given cases;* while a most lamentable lack of familiarity with the chemistry of the subject is continually exhibited throughout the work. Indeed the book is simply a jumble of badly selected extracts, huddled together in a manner which must be anything but edifying to the student. As a compilation, it has the merit of directing attention to a number of sources from which valuable information may be derived; while it has the great fault of omitting to mention numerous other sources of knowledge of equal or of greater value.

In several instances, moreover, erroneous assertions are made, or wrongful conclusions drawn. One or two of these we propose to discuss and correct

* In this respect our author has fallen far below the level attained by previous writers upon the subject. Compare for example: UHLENHUTH, *Handbuch der Photogen-und Paraffin-Fabrikation*. Quedlinburg Basse, 1858.

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in this article. Our attention will be especially directed to the first chapter of Dr. Antisell's book—"History of the Art"—for in it are errors which have too long been current in the annals of chemical science—errors, the repetition of which by our author is the more unpardonable, since, from his very position, he should have known them to be such. Indeed, from statements to be found in various parts of his work, it would appear that he must have known of these errors—that he must have been in possession of most of the facts which will here be brought forward.

That we may form a correct notion of the subject under discussion, let us here digress for a moment.

As a general rule, when any bituminous substance is subjected to distillation—in the ordinary acceptation of the term, *i. e.*, when it is gradually heated in any appropriate apparatus, a quantity of an oily fluid is produced, which may be collected in receivers; small quantities of gas, water, and other incidental products being at the same time obtained.

The oily liquid, which alone interests us here, known in this country as *crude coal oil*, is a mixture of various hydrocarbons, among which the wax-like substance Paraffine is an almost never-failing constituent. Crude oil, though of course varying greatly, according to the sources from which it is derived, like the various marketable "coal oils" obtained from it by purification, is specially characterized by its low specific gravity, being capable of floating upon water.

When, on the other hand, a bituminous substance, instead of being gently and gradually heated, is suddenly exposed to the action of an intense heat—when, as in the ordinary process of gas-making, it is thrown into vessels of iron or clay, which have *previously* been brought to a bright red heat, a different set of products is obtained. A large quantity of permanent gas is produced, while the liquids formed are no longer the light oily compounds just spoken of, but are composed of another set of hydrocarbons which taken collectively, are *heavier than water*. These constitute coal-tar. Among them paraffine is no longer found, excepting in comparatively rare instances, another solid substance, Naphthaline, being a characteristic component of the mixture. When the process to which the bituminous matter is subjected is a mixed one, *i. e.*, when a portion of the substance comes in contact with strongly heated surfaces, while other portions receive only an amount of heat sufficient to distill off oils of the kind first described, a mixed product, containing both coal-oil and coal-tar, is naturally obtained. As an instance of such mixed product may be mentioned the tar obtained in the preparation of gas from Boghead coal,* it being almost impossible, in this case, to maintain the retorts at the temperature best suited for gas-making, on account of the great amount of heat which is rendered latent by the enormous volume of gas generated by this highly bituminous substance.

It should be mentioned, that both crude coal-oil and coal-tar contain a quantity of "light stuff," composed of several exceedingly volatile and inflammable liquids. Some of these naphtha-like fluids, for example benzol—the benzine of the French—(known as *benzule* in the private vocabulary of Dr. Antisell, or that of his proof-reader)—may occur both in crude-oil and in tar; others do not. We refer to these "light-stuffs" here merely for the purpose of explaining that they have been at times spoken of as "volatile oils," from the resemblance which they bear to spirits of turpentine and other essential oils, and to eliminate them from the discussion. They are of but minor interest at the present moment, when compared with the true "coal-oil" now so largely employed in this country. We may mention, in passing, that Dr. Antisell has very inconsiderately obscured his historical sketch of the progress of the art of distilling coal-oil

* In the same class are several Scotch cannels, our own Breckenridge and allied coals, also the Albert coal of New Brunswick and the like.

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by blending with it the question of coal-tar naphthas. He has, for that matter, been unfortunate throughout in the presentation of this part of his subject; all the crude liquid products of distillation, at whatever temperature the process has been conducted, being indiscriminately classed by him as *tar*. Now, it is well known to practical men, as has already been described, that the products obtained from bituminous matters by slowly distilling them, is as different from coal-tar as ether is from alcohol. The term crude-oil, by which the first-named liquid is known to manufacturers in this country, characterizes it perfectly; so does the term *huile de schiste* (written at times simply "*schiste*") of the French.*

It is surprising that Dr. Antisell should have followed the example of several German authors—without their excuse—in thus perplexing his readers.

In returning from this digression, we would expressly declare our disbelief in the adage which allows for the existence of no novelty. Still we do believe that very few of the arts have sprung into existence in a day, their perfection, and especially their development, having almost always resulted from the successive labors of numerous individuals; and we do believe that the inventor, who first practically "applies" any abstract knowledge, and thus creates a new art or branch of industry, is entitled to credit therefore—and to far more credit, and that of a different order, than the man who subsequently introduces this art into a foreign country. We would not detract from the efforts of the latter; on the contrary, would accord them high praise; but we desire, first of all, to see justice meted out to him who created the art—to those who increase human knowledge, sooner than to its mere diffusers.

We would therefore join issue with Dr. Antisell when, in his preface, he tells us that his book is a "record of the origin and condition of an infant art," and again mentions "this new branch of industry." So, also, in the first lines of his Historical Introduction, where he speaks of "the new and extensive manufacture of oils from coal and other bituminous substances." For these statements are not only erroneous in themselves, but they—no less than the unfair allusions which appear on subsequent pages—tend to do great injustice to earlier inventors, and especially to the memory of a man whose name must ever remain inseparably connected with the history of the *art* of manufacturing the fluid now known as coal or paraffine-oil. We refer to Selligue. More than twenty-five years ago, this inventor's method of obtaining such oil was described in the *Journal des Connaissances Usuelles*, for Dec., 1834, p. 285. (See also *Dingler's Polytechnisches Journal*, 1835, lvi, 40.) This article was subsequently followed by numerous others, until in Selligue's patent of March 19, 1845, we find the whole subject treated of most fully and clearly. As a lucid and truthful description of his processes and of the products obtained, this specification is most praiseworthy. Few subsequent writers upon the subject have been able to add anything to the stock of knowledge which it imparts. Taken for all in all, it is doubtless the most meritorious essay which has ever been published upon the art of manufacturing coal-oil. We can but reiterate our statement, that the brief, inaccurate, and exceedingly superficial comments which have been bestowed by Dr. A. (pp. 9, 80, etc.) upon the information which Selligue has imparted in his admirable series of essays, does great injustice to the subject as well as to this author.

Leaving for a moment the minute consideration of Selligue's improvements, let us first glance at the labors of some of his predecessors.

As Dr. Antisell has truly said (p. 7), the discovery of the production of oil from coal appears to date as far back as the time of Boyle, (1728–1799), when the well known experiments of Dr. Clayton were made.†

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* We may here observe, that throughout this article we shall translate the French term *huile de schiste*, by its English equivalent, *coal-oil*.

† Philosophical Transactions, Jan. 1739, No. 452, p. 59; in Martyn's Abridgment, vol. ix. p. 395.

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In distilling coal from a pit near Wigan in Lancashire, this observer obtained, first phlegm (water), then oil, and finally gas.

No doubt an earlier record of similar experiments might be found in the writings of the alchemists, who, as is well known, subjected almost every substance to processes of distillation.

During the last century attention was again several times called to the fact.*

It would seem, however, that nothing very definite was published before the year 1830. UNVERDORBEN† had, indeed, in the preceding year, called attention to oils distilled from petroleum, and even appears to have obtained paraffine—to which however he gave no name.‡ The attention of the scientific world was first really attracted to this substance by the memorable memoir of Reichenbach,§ who separated it, in the first instance, from wood tar, and described its properties at length. In the following year, Reichenbach‖ is at great pains to prove that the crude-oil, obtained by slowly distilling coal, contains no naphthaline,¶ that naphthaline is not a product of the slow distillation of coal, but is a result of the subsequent decomposition of such products by heat; and that the coal-tar of gas-works is not crude-oil, but an impure mixture of the products of distillation with those resulting from their decomposition.**

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* In addition to the authorities cited by Dr. A. (p. 8), we would mention the following from *An Experimental History of the Materia Medica, or of the Natural and Artificial Substances made use of in Medicine;* by WILLIAM LEWIS, M.B., F.R.S., 3rd Edit. 8vo, Dublin, MDCCLXIX, vol. ii, p. 148, Article *Petroleum;* also, (according to American Druggists' Circular, iv, 86,) in the London edition of Lewis. 4to, 1761, p. 436:

"Some mineral oils, procurable among ourselves, are used by the common people, and often with benefit. The empirical medicine, called British oil, is of the same nature with the petrolea; the genuine sort being extracted by distillation from a hard bitumen, or a kind of stone coal, found in Shropshire and other parts of England."

† *Berzelius's Jahresbericht,* x, 181, from *Kastner's Archiv,* xvi, 122; also in *Schweigger-Seidel's Journal für Chemie und Physik,* 1829, lviii, 248.

‡ For allusions to other earlier German researches bearing upon the subject, see Reichenbach's Memoirs, which will be cited directly. Compare also Gmelin's Handbook of Chemistry (Cavendish Soc. Edit.), xii, 439.

§ *Journal für Chemie und Physik,* (or *Jahrbuch der Chemie u. Physik, Band,* xxix) *von* Schweigger-Seidel, 1830, lix, 436.

‖ *Ibid,* (or *Neues Jahrbuch der Chemie u. Physik,* B. 1,) lxi, 175.

¶ Dr. Antisell dismisses this article (p. 11) with the statement that "in 1830-'31, Reichenbach discovered naphthalin." It may not be amiss to state that naphthaline was discovered at least ten years earlier, having been described by GARDEN in 1820 (Thomson's *Annals of Philosophy,* xv, 74), to whose labors as well as to those of CHAMBERLAIN, KIDD, and others, Reichenbach particularly refers in this very article. See also *loc. cit.* B. lxviii, [B. viii, of the "*Neues-Jahrbuch,*"] S. 233.

** It must here be explained that Reichenbach has suffered great injustice at the hands of those who, in translating portions of his papers, have rendered his term "*Steinkohlentheer*" literally—coal-tar. Now the term coal-tar, in countries abounding in gas-works like England or the United States, means the tar of gas-works, and it means nothing else. Gas-works, it must be remembered, were, until quite recently, by no means so common in Germany, and were doubtless rare enough in 1830, consequently, it is not at all strange that the English idea of "coal-tar" should not have become current in that country. Reichenbach, for that matter, distinctly and repeatedly asserts, that his "*Steinkohlentheer*" is a very different substance from the tar of gas-works. In a word, it was *crude-oil.* If, perchance, there may be any person who would accuse us of mistranslating certain *words* used by Reichenbach, we would at once refer such an one to the orginal memoirs of this author. Submitting it to the judgment of any competent chemist, whether we have misinterpreted his lan-

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These experiments were made upon a manufacturing scale, Reichenbach being, at this time, "chief of an extensive system of mines, iron furnaces, machine shops, chemical works, etc., most of them established by himself on the estate of Count Salm [Blansko, Moravia]. These works lie along a line some fifteen miles [5 *Stunden*] in length." (Schweigger Seidel).

In another article published later, in 1831,* he describes his method of obtaining paraffine from the distillation of flesh and of *coal* (portions of 75 lbs. weight having been operated upon). With regard to coal, he particularly urges the necessity of slow distillation, in order to prevent the decomposition of the first products and the consequent formation of naphthaline, as explained in his previous article, to which he refers. The paraffine was separated from the less volatile portions of the rectified oil by cooling—the description of which oil R. reserves for a separate article.† He also obtained paraffine from petroleum. Two more papers upon the subject were published by Reichenbach in this year,‡ only the first of which is of particular importance in this connection. It relates to *Eupion* (εὖ very, πίον fat). A term by which Reichenbach designates, in some instances, a portion, in others the whole of the somewhat difficultly volatile, fat-like oils, prepared by purifying the first product obtained by slowly distilling substances of animal or vegetable origin. This eupion was, in fact, a mixture of several hydrocarbons—the same which, in similar mixtures, are now collectively known in commerce as coal-oil; called paraffine oil by some, and designated in the retail trade by innumerable other names of only local significance.

Eupion was obtained by Reichenbach from the products of the slow distillation of animal and vegetable substances, as well as from coal, and was minutely described by him. We make but a single extract from this article, which occupies some thirty-two pages: "When any one shall succeed in separating eupion, at a sufficiently cheap rate, from the tars [crude-oils], it will very probably enter into the circle of substances useful in household economy. For, since it burns from a wick, brightly and clearly, and is free from smoke, it is in no wise inferior to the finest oil as an illuminating material. It does not grease nor crust the wick, nor stiffen when cold. If we consider, in addition to this, that for all purposes where cold can exert no influence, the paraffine need not be separated, but can be left dissolved in the eupion, and used in conjunction with it for lighting; we shall perceive that this is of some importance, since the two substances are thus mutually improved for technical purposes."

In 1832, Reichenbach§ again published a note upon eupion; and, in 1834, another long article,‖ in which he once more dwells upon its useful properties.

Reichenbach's contributions on the subject of the dry distillation of organic substances, are comprised in some twenty or more long articles, not counting

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guage, [compare, for example, *loc. cit.*, B. lxviii., [B. viii, of the *Neues-Jahrbuch*,] S. 226].

It may be worth while also to call the attention of the reader to the fact that all of the substances discovered by Reichenbach in "tar" (as the text-books tell us) were in reality obtained from crude-oil. Knowing this, every one familiar with recent chemical literature, will perceive at once why so few of R.'s scientific results have been corroborated. For, until quite recently, the attention of chemists interested in such researches, has been almost completely occupied with the subject of coal-tar. Compare also Reichenbach's complaint against Dumas and Laurent, in Schweigger-Seidel's *Journal für Ch. u. Phys.*, 1838, lxviii, 223.

* *Loc. cit.*, lxi, [or B. 1, of the *Neues-Jahrbuch*], S. 273.

† *Vid. infra.*

‡ *Loc. cit.*, lxii, [or B. ii. of the *Neues-Jahrbuch*], S. 129, 273.

§ *Loc. cit.*, B. lxvi, [B. vi. of the *Neues-Jahrbuch*], S. 318.

‖ Erdmann's *Journal für praktische Chemie*, i, 377.

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several smaller "notes." A tolerably complete list of which may be found in Erdmann's Journal für praktische Chemie, i, 1. It is very much to be regretted that these memoirs have never been collected and published as a separate volume. Even now, any chemist who could find time to collect these scattered articles and translate them into English or French, would unquestionably promote the cause of science by so doing.

Looking at the question for a moment, solely in its scientific bearings, we cannot refrain from an expression of astonishment, that the details of Reichenbach's researches are so little known to the generality of chemists;* while, on the other hand, we are forced to confess, that it is indeed rare that scientific researches, conducted by a chemist in his laboratory, have so fully described a future art—have so accurately pointed out the methods to be followed and precautions to be observed by the practical manufacturer. We must not omit to mention that, in 1831, Christison† of Edinburgh made known his discovery of paraffine in petroleum from Rangoon. Not knowing of Reichenbach's previous publication, Christison named it *Petroline*, but subsequently admitted its identity with paraffine. In 1833, Bley‡ distilled oils from lignite.

A little later, in 1834, Gregory§ published an able article upon paraffine and eupion, and their occurrence in petroleum. Of this memoir we cite but two lines, (vid. Trans., p. 129, or Rep., p. 113). "It follows," says Gregory, "that there are some kinds of naphtha [petroleum] which contain paraffine and eupion, and are consequently the results of destructive distillation."

In the following year, v. Kobell‖ also noticed paraffine in petroleum.

For the labors of Hess in Russia, and of several other chemists in Germany, as well as for the interesting discussions which followed between these

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* This lack of information appears to depend upon the circumstance, that the writers of most recent chemical text-books seem to have derived their knowledge of the subject in question, from Gay Lussac's brief abstract of Reichenbach's earlier memoirs, which was published in 1832, in Poggendorff's *Annalen*, xxiv, 178; also in the *Annales de Chimie et de Physique*, [2], l, 69; and quite extensively copied by the journals of the day.

In this connection we would respectfully urge upon all those who have fallen into the common habit of regarding as somewhat apocryphal the numerous substances of greater or less scientific interest, which Reichenbach separated from the products of dry distillation, that before seeking to discredit—or allowing themselves to disbelieve—them, they should conduct experiments similar to his, *on a scale of equal magnitude*. Let us here also bear in mind the luminous conclusion of the late Dr. Hope of Edinburgh, who, as the story goes, (Vid. London Chemical News, i, 56), one day informed his class that Reichenbach had discovered in tar, "creosote, picamar, paraffine, cedriret, capnomor, and a host of other substances of no interest or importance whatever." Of these "unimportant" substances, two at least, eupion and paraffine, are to-day as well known, in the world, as bees-wax or spermaceti, although comparatively little—we had almost said nothing—has been added to the scientific knowledge of them, since the publication of Reichenbach's memoirs. If, perchance, any other of these well-nigh forgotten bodies should be found to possess any technical importance, we would quickly enough find some one claiming credit for its "discovery," and oppressing chemical nomenclature, by adding yet another name to the existing "host." Even now we await, with no little interest, the elucidation of the question—whether the new violet dye, prepared by oxydizing anilin, which is exciting so much interest, under the names anilein, Perkin's purple, *mauve*, etc., is not identical with, or a component of, the *pittical* of Reichenbach.

† Transactions of Royal Society of Edinburgh, xiii, 118; also in Repertory of Patent Inventions, 1835, [N. S.] vol. iii, p. 390.

‡ Schweigger-Seidel's *Journal für Chemie u. Physik*, B. lxix, [B. ix, of the *Neues Jahrbuch*], S. 129.

§ *Transactions of Royal Society of Edinburgh*, xiii, 124; also in *Repertory of Patent Inventions* 1835, [N. S.] vol. iv, p. 109.

‖ J. pr. Chem. v. 213.

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observers and Reichenbach, the reader may consult the general index [Namen-u. Sach-Register zu den Bänden i. bis lx, Leipzig, 1845] to Poggendorff's Annalen der Physik u. Chemie.

At the same time that these scientific researches were in progress in Germany and Scotland, or even earlier, numerous practical efforts to manufacture oils from bituminous substances were made in France.

Although the precise date at which these experiments were commenced is somewhat obscure, it will not be difficult to trace the history of the successful development of the industry to which they gave rise.

As stated by Dr. Antisell, the MM. Chervan* had a patent, dated in 1824, for distilling bituminous substances. Blum and Moneuse,† in 1832, claim only the application of coal-oil to purposes of lighting—not its manufacture, which they allude to as being well known.

Subsequently (7th October, 1833) Boscary‡ obtained a patent for extracting pyrogenous oil from different substances, asphaltums, etc., and especially from the shales which occur in the environs of Autun (*Saone* et *Loire*), and finally from all the bituminous matters in France. The oil, which is obtained by distilling the shale in metallic cylinders, may be used, according to Boscary, instead of fish-oil or resin, for gas-making—a much better gas than that prepared from coal being thus obtained.

In 1833, Laurent§ occupied himself with the investigation of various bituminous shales, both French and English, at the instance of the MM. Blum, whom he mentions as being occupied with the distillation of oil from the shales of the environs of Autun.‖ Laurent gives the details of the process employed by himself, telling us that the retort in which his shales were distilled attained a sombre red heat at the close of the operation; also of the percentage amounts of oil (20 p. c.), gas, coke and water obtained from the Autun shale; how the oil cannot be burned in ordinary lamps, on account of smoking, but affords a very luminous flame when consumed in lamps furnished with suitable chimneys. He shows moreover that the oil contains paraffine, and does not contain naphthaline.

Laurent subsequently published another paper¶ upon this oil, in which article he records his efforts to ascertain what definite chemical compounds are contained in the oil. One of the products obtained by fractional distillation, viz., an oil boiling at 167° to 170° (C.) = 333° to 338° F., he considers as identical with eupion.

In 1834, we find, for the first time, an article** describing the process of Selligue, although it would appear from the statements of this chemist and of others, that his attention had been directed to the subject of distilling bituminous shales several years earlier. The cited article relates how the shale is *slowly* distilled in iron cylinders, until no more oil comes over; how the oil obtained is characterized by containing neither oxygen nor naphthaline, but a solid substance differing from the latter, and resembling that called paranaphthaline†† by Laurent.

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* Brevets d'Invention xviii, 232. † *Ibid.* lxv. 250.

‡ *Ibid.* lxviii, 359.

§ *Annales de Chimie et de Physique*, liv, 392.

‖ According to Laurent, he had himself proposed to a company, in 1829, to work these shales, in order to extract the oil contained in them, and to employ it for lighting.

¶ *Comptes Rendus*, 1837, iv, 909; more fully in *Annales de Chimie et de Physique*, lxiv, 321.

** *Journal des Connaissances Utuelles*, Dec. 1834, p. 285; also in Dingler's *Polytechnisches Journal*, 1835, lvi, 40, from which our extract is taken.

†† The inadvertency of confounding this body with paraffine was subsequently corrected by Selligue.

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In 1834, '35 and '36, Selligue* was principally occupied with his well-known process for making water-gas. In calling the attention of the French Academy† to this, he remarks that, in conjunction with David Blum, he holds a patent granted in 1832 for the application of oils obtained from shale to purposes of direct illumination, and that the working of the shale employed is in the hands of a company capable of developing the business to any extent which commerce or the arts may require.

In the same year Payen‡ in reporting upon Selligue's water-gas, remarks upon the great importance of the new industry of distilling oil from shales which S. has introduced.

In the following year we again find Selligue before the Academy§ requesting that body to appoint a committee to examine the merits of his new system of gas-lighting; his process of distilling bituminous shales on the great scale by means of apparatus, each one of which furnishes from 1,000 to 1,400 pounds of crude oil per day—this being about 10 per cent of the weight of the shale employed, and being almost all which exists in the raw material; also of his process of separating various products from the crude oil, some of which are applicable to the production of gas, others to ordinary purposes of illumination, and others to different uses in the arts. This petition was referred to a committee of three—Thenard, D'Arcet and Dumas—who reported in 1840.‖ They mention the localities of Selligue's three establishments for obtaining oil from shales; the amounts of oil obtained from the different kinds of shale, &c.

In 1838 Selligue also obtained a new patent¶ "for the employment of mineral oils for lighting." In his specification he informs us that the principles upon which his processes for rendering the oil obtained from shales proper for the purposes of direct** illumination depend, are:

I. Removal of almost all odor. II. Removal of all tar. III. Removal of the most volatile portions of the oil, which are also the most inflammable and the most odorous, the presence of which would cause the oil to have too great fluidity for the capillarity of ordinary wicks. * * *

The operations, continues Selligue, consist in slowly distilling the bituminous shale, and collecting the liquid products in large receivers. These products are redistilled, and divided into fractions by refrigerating. They are treated with concentrated sulphuric acid for a longer or shorter time according to the nature of the shale employed. Twenty-four hours are ordinarily sufficient, the oil being agitated from time to time. The quantity of acid used varies from $\frac{1}{10}$ to $\frac{1}{20}$. After this the oil is to be carefully drawn off from the tar, and washed with water. Slaked lime is then added and a current of steam passed through the oil in order to carry off by distillation all the more volatile and odorous liquids. This last, says Selligue, is the most important part of my process, for if this very inflammable portion were allowed to remain in the oil, one could not use the latter in ordinary lamps *à courant d'air.* * * * This patent it should be observed claims only to be an improvement upon that of Blum and Moneuse (*vid. Supra*). Selligue asserts, however, that coal-oil had never before been prepared in such a manner that

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* See seven patents in *Brevets d'Invention*, lxx, 269. Of these patents two are dated 1834; two, 1835; and three, 1836. For a description of his process of gas-making see also *Bulletin de la Société d'Encouragement*, Oct. 1838, p. 396; or Dingler's *Polytechnisches Journal*, lxxi, 29.

† *Comptes Rendus*, 1837, iv, 969.

‡ Dingler's *Polytechnisches Journal*, lxviii, 201; from *Bulletin de la Société d'Encouragement*, Dec. 1837, p. 493.

§ *Comptes Rendus*, 1838, vii, 897.

‖ *Comptes Rendus*, x, 861; also in Dingler's *Polytechnisches Journal*, lxxvii, 137.

¶ *Brevets d'Invention*, lxviii, 395.

** The term "direct illumination" is constantly used by Selligue in contradistinction to the indirect use of the oil in his process of gas-making.

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it was fit for use in common lamps. This has, indeed, he says, been the subject of many researches, but no one has hitherto succeeded in avoiding the empyreumatic odor, and the very inflammable products which caused the oil to rise too quickly to the summit of the wick. He goes on to define the difference between his purified oil and the crude oil obtained directly from shale. On the 27th of March, 1839, Selligue specifies certain additions and improvements to the preceding patent. I should add, he says, that I now divide the products of distillation into four distinct parts, which afford me every facility for employing these products in the arts and for domestic economy. In these divisions there are indeed some anomalies which arise from differences in the shales, &c. which I treat; but the following products are always obtained:

I. A light, volatile oil more or less odorous according to the source from which it is derived. * * * This can be used in painting, for dissolving resins, &c., for lighting by vaporising it (it being very volatile) or for the production of gas according to my system.

II. A fat oil only slightly volatile, and having but little odor; this can be used for domestic purposes in ordinary lamps with or without admixture of animal or vegetable oils.

III. A fatty substance almost odorless, possessing all the properties of the fats, and well adapted for use in the arts. It can also be used for lighting, either by mixing it with light oils or by decomposing it for the production of gas. It can moreover be used for soap* since it saponifies very well, and being without odor affords a very good soap; with ammonia the fat forms a sort of pomade.

IV. An odorless pitch of great purity and tenacity, suitable for preparing a black solid varnish for preserving wood, iron-work, &c. * * *

In 1839, Selligue† in alluding to the use of his oils in the treatment of cutaneous diseases speaks of the three large establishments for the distillation of bituminous shale which he has erected in the Department Saone et Loire, and mentions the fact that the oil (crude?) is furnished at the rate of about two cents [ten centimes] per pound.

The question of price is again discussed a few years later, when Selligue‡ says: it has been stated that crude shale oil costs only $1 50 per 100 pounds, and that it contains 60 per cent of a very light volatile ethereal oil well suited to afford light, as well as 40 per cent of a fat substance. Now since 1837, I have extracted more than 4,000,000 pounds of oil from bituminous shale, but the oil (crude?) costs 20 cents a gallon (22 frs. the hectolitre) or even 27 cents when delivered in Paris. From every hundred measures of the crude oil are obtained (by distillation) 20 measures of volatile oil boiling at 100° C.=212° F.; 30 measures of less volatile oil boiling at 150° to 260° C.=302° to 500° F.; 14 measures of an oil containing paraffine, and 28 measures of fat—five measures being lost. In purifying these products a further portion is lost.

The clearest of all Selligue's specifications, however, is that of the patent granted to him March 19, 1845§ for the distillation of bituminous shales and sandstones.

After describing the various forms of apparatus used in distilling, into one of which superheated steam was introduced, he enumerates the products of distillation as follows: I. A white, almost odorless, very limpid mineral oil—somewhat soluble in alcohol—which may be used as a solvent, or for purposes of illumination in suitable lamps, &c.

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* This "soap," (emulsion) is described more fully in the sequel.

† *Comptes Rendus*, ix, 140; also *Annalen der Pharmacie*, von Wœhler u. Liebig, xxxii, 128.

‡ Dingler's *Polytechniches Journal*, xci, 198; from the *Moniteur Industriel*, 1843, No. 770.

§ *Brevets d'Invention*, [new series, (loi, du 5 Juillet, 1844,)] iv, 30.

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II. A sparingly volatile mineral oil of sp. gr. 0·84 to 0·87, of a light lemon color, perfectly limpid, almost odorless, never becoming rancid, and susceptible of being burned in ordinary oil lamps, of constant level *à reservoir supérieur*, with double current of air—a slight modification of the form of the chimney and burner being alone necessary. This oil can also be mixed with the animal or vegetable oils. Oils thus prepared do not readily become rancid, nor do they congeal easily when subjected to cold.

III. A fat mineral oil, liquid at the same temperature as olive oil. This oil contains a little paraffine; it is peculiarly well adapted for lubricating machinery, and has an advantage over olive and other vegetable oils, or neats-foot oil in that it preserves its unctuosity when in contact with metals and does not dry up. It saponifies easily, and forms several compounds with ammonia.

IV. From the oils Nos, I, II, and III, I extract a red coloring matter which can be used in various arts.

V. White crystalline paraffine which needs but little treatment in order to be fit for making candles; this substance does not occur in very large proportion in the crude oil, and the proportion varies according to the different mineral substances upon which I operate. There is but little of it in petroleum, and in the oil obtained from bituminous limestone. I often leave a great part of the paraffine in the fat oil and in the grease in order that these may be of superior quality.

VI. Grease. This grease is superior to that of animals for lubricating machinery, and for many other purposes, since it does not become rancid, and remains unctuous when in contact with metals.

VII. Perfectly black pitch—very "drying"—suitable for preserving wood, metals, &c.

VIII. An alkaline soap obtained by treating the oils with alkalies.

IX. Sulphate of ammonia. X. Manure prepared by mixing the ammoniacal liquor, or the blood of animals, with the crushed fixed residue (coke) of the shale. XI. Sulphate of alumina from the residue of the shale. F. H. S.

[*To be concluded.*]

SCIENTIFIC INTELLIGENCE.

I. CHEMISTRY AND PHYSICS.

1. *On Chlorophyll.*—Frémy has communicated the results of an interesting investigation of the green coloring matter of plants. His conclusions are as follows:

1. Chlorophyll may be decomposed into a blue and into a yellow substance.

2. These substances give compounds with alumina which are insoluble in water. When hydrate of alumina is added to an alcoholic solution of chlorophyll, neither of the constituents of the latter unites with the earth. When, however, water is added gradually, the blue substance and a little of the yellow first unite with the alumina to form a dark green precipitate, while the alcoholic solution remains yellow. Both bodies are precipitated together from a very dilute aqueous solution.

To separate the blue and yellow substances completely, the author introduces a mixture of two parts of ether, and one part of muriatic acid, diluted with a little water into a bottle, and shakes the whole till the mu-

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riatic acid is saturated with ether. The natural chlorophyll which may be extracted from leaves by means of alcohol, may be easily decomposed into its constituents by shaking it with this mixture of muriatic acid and ether. The liquid first takes a brown color, after which the aqueous muriatic acid takes a beautiful blue, while the ether separates, holding the yellow coloring matter in solution. Frémy calls the blue substance phyllocyanin, and the yellow phylloxanthin. When alcohol is added, so that the two liquids mix, a green is again produced, of the color of the natural chlorophyll.

Young leaves exposed to the vapors of muriatic acid, quickly assume a beautiful green color. The yellow autumn leaves, on the other hand, contain only phylloxanthin. Under certain circumstances, phyllocyanin is capable of yielding another yellow substance, to which the author gives the name of phylloxanthein; from this substance phyllocyanin may be again obtained. The blue coloring matter of chlorophyll is more easily altered than the other. The author promises a further investigation of this very interesting subject; his results are of equal interest to naturalists and chemists.—*Compt. Rendus*, l, p. 405.

2. *On the Separation and Estimation of Phosphoric Acid.*—Chancel has given a method of estimating phosphoric acid, which depends upon the entire insolubility of phosphate of bismuth in liquids containing free nitric acid. When a solution of acid nitrate of bismuth, so dilute as not to be rendered turbid by water, is added to a liquid containing a phosphate dissolved in nitric acid, a very dense, white precipitate is immediately formed, which has the formula BiO_3, PO_5, and the constitution of which, according to Chancel, is perfectly constant.

The neutral phosphate of bismuth is perfectly insoluble in water, and dilute nitric acid, whether cold or hot; it dissolves sensibly in solutions containing a large proportion of salts of ammonia. The filtration and washing is very easy and rapid; the dried salt may be ignited in a platinum crucible, and does not fuse at a red heat.

Pyrophosphoric acid is also precipitated completely by the acid nitrate of bismuth; a white precipitate is formed which is much more voluminous than the tribasic phosphate. This precipitate has the formula $2BiO_3.3pPO_5$: it is easily and completely transformed into the tribasic phosphate by simply boiling it in the presence of an excess of the acid nitrate of bismuth.

The metaphosphates behave in the same manner, but the precipitate requires a longer boiling to be completely transformed into the ordinary phosphate.

Chancel asserts that he has been able to detect the presence of one milligram of phosphoric acid when mixed with 120mm. of alumina in a dilute solution containing more than a gramme of free nitric acid. As the precipitation is very rapid in a hot solution, and as the liquid becomes almost instantly clear, it would be easy to estimate phosphoric acid by a titrated solution of acid nitrate of bismuth.

Chancel prepares the acid nitrate by dissolving, with the aid of heat, one part of pure crystalline subnitrate of bismuth in four parts of nitric acid of specific gravity 1·36, adding to the solution 30 parts of distilled water, and filtering if necessary. In using this solution, the phosphate,

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if not soluble in water, is to be dissolved in nitric acid, avoiding a large excess. The solution is to be diluted with water, the nitrate of bismuth added as long as a precipitate is produced, and the whole brought to boil, filtered and washed with boiling water. After drying, the filter must be separated as completely as possible from the precipitate, burned by itself, and the ashes added to the precipitate, which may be heated to redness in a platinum crucible. The bases are easily estimated in the filtrate, after removing the excess of bismuth by sulphuretted hydrogen. This process requires that the liquid should be free from chlorids and sulphates, which, when present, are easily removed by nitrate of silver and chlorid of barium.—*Comptes Rendus*, l, p. 416.

3. *On a New Mode of Preparing Calcium.*—CARON has succeeded in preparing large quantities of calcium by the following process: A mixture of 300 parts of fused and pulverized chlorid of calcium with 400 parts of granulated distilled zinc and 100 parts of sodium in pieces is to be heated to redness in a crucible. The reaction is feeble, and after some time flames of zinc appear. The heat is then to be moderated, the temperature remaining as high as possible without volatilizing the zinc; after a quarter of an hour the crucible may be withdrawn from the fire. It contains a well fused metallic mass, which is highly crystalline, and which contains from 10 to 15 per cent. of calcium. The alloy is then to be placed in a crucible of gas-retort carbon and the zinc expelled by heat: in this manner Caron obtained masses of 40 grammes at a single operation, and containing only the impurities of the zinc employed. As thus obtained calcium has a brass-yellow color and a density of from 1·6 to 1·8, it is not sensibly volatile, but filings of the metal burn with red sparks of remarkable beauty without formation of vapor, which seems to show that the metal is not volatile at the temperature of its combustion. The author promises to communicate the results of similar experiments in the preparation of barium, strontium, &c.—*Compt. Rendus*, l, p. 547.

4. *On a New Metallic Element.*—VON KOBELL has discovered in euxenite, æschynite, and samarskite, and a tantalite from Tammela, a new metallic acid belonging to the same group with tantalic and niobic acids. To the new metal contained in this acid, the author has given the (not very well selected) name of *Dianium*. When dianic acid, as precipitated by ammonia from its solution in chlorhydric acid, is boiled with chlorhydric acid and metallic tin, a beautiful deep sapphire blue solution is produced, which remains blue after filtration. When tantalic acid, from the tantalite of Kimeto, or niobic acid from Bodenmais, are treated in the same way, the solution becomes bluish; and on adding water, the color quickly vanishes, and the solution, on filtering, passes through colorless.

When dianic acid is boiled with chlorhydric acid and zinc, instead of with tin, the blue solution does not appear, the precipitated acid becomes blue, but filters colorless, and is decolorized by water without being sensibly dissolved. When equal quantities of dianic, tantalic, and hyponiobic acid are boiled with concentrated chlorhydric acid, upon a funnel of platinum-foil for three minutes, all three give yellowish milky liquids; if water be then added, the solution of dianic acid becomes perfectly clear, while the tantalic and hyponiobic acids remain undissolved.

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When freshly precipitated dianic acid is heated to boiling with dilute sulphuric acid, the milky liquid poured into a glass, and grains of distilled zinc thrown in, the dianic acid in a few moments becomes smalt blue, even dark blue, and retains this color for some time on addition of water; but the liquid passes through the filter colorless. In this respect, dianic acid resembles hyponiobic acid, while tantalic acid, under the same circumstances, becomes pale blue, and immediately loses this color on addition of water. In this manner, tantalic may be distinguished from dianic and hyponiobic acids. The relations of the three acids to chlorhydric acid and tin, and to sulphuric acid and zinc, are thus sufficient to distinguish them from each other.

Dianic acid appears to exist, though in a less pure state, in the tantalite from Greenland, in pyrochlore from the Ilmengebirg, and in the brown Wöhlerite—though the author had but small quantities of these minerals at his disposal. A small piece of black yttrotantalite, believed to be from Ytterby, gave the reaction of dianic acid. A second specimen, however, the specific gravity of which was found to be 5·55, contained tantalic acid.

Titanic acid is easily distinguished from the other acids of the same group, by boiling it with muriatic acid and tin, and diluting the solution with water. The blue color then passes to rose red, and the solution retains this color several days. When dianic acid is present, the blue color predominates, but after standing some hours the rose color of titanic acid appears.

The tantalite from Tammela, which Von Kobell terms dianite, has a specific gravity of 5·5—while the other tantalites vary in density from 7·06 to 7·5. The streak of dianite is dark grey, while that of the tantalites from Tammela is dark brown red. Before the blowpipe, dianite exhibits no sensible difference from the tantalite of Kimeto.

H. Rose, to whom Von Kobell sent a specimen of dianic acid for examination, considered it probable that the peculiar reactions of this substance might be due to the presence of tungstic acid. Von Kobell has, however, shown by special experiments, that this is not the case. In conclusion, the author recommends those who desire to repeat his experiments, to employ the same proportions of water, acid, etc., of which he himself made use, and for the details of which we must refer to the original paper.—*Bull. der Acad. der Wissenschaften*, March 10th, 1860, (Munich). W. G.

5. *Note on the extent to which Mercury volatilizes along with the vapor of water at* 100° C.; by J. W. Mallet.*—In Berzelius' Traité de Chimie it is stated that Stromeyer drew attention to the fact of the evaporation of mercury in considerable quantity at 60° to 80° C. with the vapor of water, the more volatile substance carrying with it the less volatile, as in the case of a solution of boracic acid when heated.

This fact does not seem to have been very generally noticed by the compilers of chemical text-books in treating of the history of mercury, though it is always stated that the metal is capable of volatilizing *to a very slight extent*, even when alone, at the common temperature of the atmosphere. Some doubt too would seem to have been thrown upon Stromeyer's observation by the experiments made, under peculiar condi-

* Communicated by the author.

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tions, by Fresenius, and reported by him in the appendix to his treatise on quantitative analysis. It was found that 6·4402 grm. of mercury, *covered* with a considerable quantity of water, and heated to the boiling point of the latter for a quarter of an hour, lost but ·0004 grm., while exposure to the air at summer heat for six days produced a further loss of ·0005 grm.

I have lately observed the tolerably rapid evaporation of the metal with steam, produced not from a mass of water covering the quicksilver, but from a porous and highly absorptive clay which was dry to the touch, but gave off some eight or ten per cent of water when heated to 100° C. A specimen of this soil had been in contact with mercury, and several grams of the metal in small globules had become mixed up with the mass. It was placed in the common copper box with double sides which serves as a steam-bath, and exposed to the temperature of boiling water. In half an hour I was surprized to see that the little piece of glass tube through which the heated air and vapor from the inside of the steam-bath escapes was coated with a bright specular deposit of metallic mercury. This was brushed off, and the tube replaced, when again it became in a short time similarly coated. The steam-bath was kept in constant use in drying this and other specimens of soil for twenty or thirty hours, and in this time between four and five grains of mercury was collected from the glass tube. Doubtless the condensation of the mercury vapor on the latter must have been far from complete—a large proportion of mercury probably escaped into the atmosphere. At any rate it seems clear that the metal can volatilize in very considerable amount when surrounded by vapor of water at 100° C., and not at the same time pressed upon or affected by the cohesion of a mass of liquid water. Hence an obvious necessity for thoroughly effective condensation when mercury is to be determined in its compounds by ignition with soda-lime.

TECHNICAL CHEMISTRY.

6. *Disinfectants.*—The use of a mixture of coal-tar and plaster-of-Paris for purposes of disinfection and for dressing wounds, as proposed by CORNE and DEMEAUX (*Comptes Rendus*, xlix, 127; see this Journal, xxviii, 425), has been recently reported upon in the French Academy by a committee—Chevreul, J. Cloquet, and Velpeau (*rapporteur*)—to which the subject was referred in July, 1859.

The great interest, which this method,—so favorably commented upon by the distinguished surgeon Velpeau soon after its publication,—excited among the medical men of France gave rise to the publication of numerous other systems of disinfection, which being submitted to the Academy for its approval were also referred to the committee in question. The labors of its members have thus been materially increased, and their report swelled to the dimensions of a general treatise upon disinfectants—especially those applicable to wounds.

In numerous experiments made at the Hospital *de la Charité* the mixed coal-tar and plaster of Corne was employed, both in the state of powder and as a poultice made by mixing it with oil. When applied as a thick layer, three or four times a day, upon putrid, gangrenous and sanious wounds, the powder destroyed their odor without giving rise to any special pain. Upon indolent sores, however, or upon recent burns,

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the contact of the powder produced considerable smarting upon some patients, though well borne by others. Wounds of the first class were often found to be cleaned as well as disinfected; while those of the second class generally acquired a dirty, pale, gray tint, their circatrization being hindered.

The poultices were found to be more advantageous than the powder in the treatment of cavernous wounds, purulent or fetid, and sinuous foci, open suppurating abscesses, anthracoidal suppurations, etc.

Applied directly to the sore, the poultices destroyed the putrid odors, allayed the inflammation without augmenting the pain, leaving beneath them a healthier pus, and the surfaces in better condition. In a word, the mixed coal-tar and plaster, when properly applied, disinfects wounds and putrid suppurations. As for the absorbent and detergent qualities which its inventors also claim for it, these are less clearly evident.

The powder absorbs better than the poultices,—the latter, it is true, take up a portion of the morbid exudations, but unless the dressing is carefully renewed, five or six times a day, pus will nevertheless collect beneath it. From this it follows that after having been somewhat cleansed the wound ceases, at the end of a few days, to clean itself, or to heal more rapidly than it would with the usual topical applications.

Upon ulcerated cancers, the mixture, either as powder or poultice, disinfects them partially, but neither dries up the suppuration nor alleviates the pain.

It is in the dissecting-room, upon organic matter in a state of putrefaction, that the mixed coal-tar and plaster is all powerful. The most infectious masses, when imbued with the powder, or simply rolled about in it, lose at once their disagreeable odor. According to Velpeau, his autopsy room was as approachable towards the close of last summer as it had formally been repulsive. It was freed from flies and other insects, as well as from putrid odors.

Although it would have been out of the province of the committee to experiment upon the application of this mixture in disinfecting filth upon the great scale, they have nevertheless proved that it can be advantageously used in hospitals for deodorizing urine or fecal matters.

The following inconveniences, to which the use of the mixture in surgery would give rise, are enumerated:

It not only soils the clothes of the patient, but hardens them and causes them to weigh more heavily upon or about the wound; it imparts to the bandages, with which the poultices are covered, a very tenacious rusty or yellow color; it must be frequently renewed, and although it destroys putrid smells, it retains a bituminous odor by no means agreeable to many persons.

These inconveniences are of comparatively slight importance, it is true, and may possibly admit of being remedied.

Of the other disinfectants submitted to the committee, several were only modifications of that of Corne and Demeaux:—Vegetable tar, as shown by Renault, may be substituted for coal-tar.—A mixture composed of hydraulic lime and tar did not disinfect wounds to which it was applied, nor could it be supported by the patients. With regard to the assertions of some practitioners, that common earth, talc, flour, or other

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vegetable and mineral powders—even poudrette—when mixed with coal-tar furnish a more convenient and less costly disinfectant than that prepared with plaster, the experiments of the committee have proved that while coal-tar, mixed with common earth, well dried, or with sand, is equally, or perhaps much more, efficacious for disinfecting fecal matter as when mixed with plaster; that while comparative experiments made from this point of view upon sulphate of lime, clay, charcoal, linseed meal, and earth have resulted in favor of the latter, the same is by no means true in surgery. When applied to wounds or infectious suppurations these different mixtures were only partially successful, having proved to be less efficacious than the mixed plaster and coal-tar. In like manner the proposal to use an emulsion of coal-tar and tincture of saponine has not been found advantageous in practice: most patients complained of it, their wounds exhibited scarcely anything satisfactory, while the disinfection was very imperfect. The mixture of plaster and coal-tar was substituted for it, upon the same wounds, with decided advantage.

Although the modifications of Corne and Demeaux' process have not been particularly felicitous thus far, they have nevertheless served to confirm the fact that in reality it is the coal-tar which acts the principal part as disinfectant in these various mixtures.*

Among the numerous other substances proposed as disinfectants, or for dressing wounds, the following have not afforded satisfactory results:

Chlorate of potash,—mixed with clay or kaolin (for example, 10 parts of chlorate to 90 parts of white clay or fine sand) which was proposed as an absolute disinfectant, neither disinfected nor absorbed the pus of fetid wounds. The mixture would be in any case much more costly than coal-tar and plaster and certainly less efficacious.

Whites of eggs,—mixed with chalk and applied to wounds, previously oiled, succeeded no better than simple-cerate.

* The inefficiency of sulphate of lime as a general disinfecting agent when used by itself may be readily demonstrated by the following experiment which is of interest in view of the fact that a belief in the utility of gypsum as a deodorizer appears to be widely spread among recent writers. For that matter we are told by Paulet (*Comptes Rendus*, xlix, 199) that during the last 25 years more than fifty authors of processes of disinfection have announced, each as he believed for the first time, the use of plaster as a means of disinfection.

If a mixture of about equal volumes of powdered gypsum and fresh urine be introduced into a small phial, the mixture placed in a warm room and thoroughly shaken several times a day until the urine has become putrid, it will be observed that an exceedingly disagreeable odor will be developed, differing from that of ordinary stale urine inasmuch as it is unalloyed with the odor of ammonia. For the complete success of this experiment it is important that a large excess of sulphate of lime should be present and that the mixture should be frequently agitated, else the whole of the carbonate of ammonia will not be decomposed and will tend to mitigate the fetor of the special odor of the putrid urine. So far from disinfecting in this case the sulphate of lime really destroys a deodorizing, or at least a masking agent, ammonia; leaving free,—purified as it were, and unadulterated, an odor, the peculiar offensiveness of which is remarkable. Sulphate of iron being substituted for gypsum in this experiment afforded a somewhat similar result, although the odor obtained was a trifle less insufferable than that of the experiments with sulphate of lime. It should be here mentioned that the odors in question were in no instance contaminated with sulphuretted hydrogen,—as was ascertained by careful trials. F. H. Storer.

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Powdered sugar.—When employed in layers upon ulcers forms crusts, beneath which the suppurations accumulate and hinder the process of healing.

Cherry-laurel water, glycerine and cellulose.—According to Antier, glycerine mixed with equal parts of cherry-laurel water forms a valuable absorbent or disinfectant to be applied as a lotion or injection. This mixture converted into pomade by mixing it with powdered almonds was also proposed as a topical application for all kinds of wounds. But in the hands of the committee neither the liquor nor the pomade by themselves or mixed with kaolin produced any effect more marked than that of lead-cerate and other anti-putrid or detergent solutions already in use.

The members of another group of disinfectants are worthy, in various degrees of consideration.

Among these charcoal appears in the front rank.—Surgeons have long regarded it as one of the best antiseptics known. Confined between pieces of linen according to the process of Malapert and Pichot it is more readily applied than when used as powder directly upon wounds; but the mixture of coal-tar and plaster, which disinfects still better and is more cleanly, is susceptible of a simpler and a more general application.

Coke of Boghead coal,—in powder as proposed by Moride* like carbon

* In view of the claim of Moride (*Comptes Rendus*, xlix, 242) as well as from its general interest the following extract from a report made to the British Secretary of War by Lewis Thompson (London Journal of Gas Lighting, Water Supply and Sanitary Improvement, 1856, v, 11) may here be cited.

Mr. Thompson states that he has instituted a set of experiments having a purely money basis as their exponent.—The articles enumerated were each employed until they practically deodorized one uniform quantity of the same mass of putrid sewage and the money value of the proportions thus used was deduced either from a broker's price-list or, where this failed to give the requisite information, by special inquiry from a wholesale dealer. The amount of sewage operated upon in each experiment was half a gallon taken from a single tank which had been recently filled out of a large and very offensive ditch or open sewer. Two indications of the progress of the disinfection were had recourse to in these experiments; one with paper dipped in sugar of lead which gradually ceased to become brown as the deodorizing agent was added in successive portions; the other had reference to the discontinuance of any offensive smell; and the attainment of this last condition was regarded as the termination of each experiment.

By this means he was enabled to draw up the subjoined table which shows at a glance the comparative cost of executing the same amount of deodorizing work with each agent on the supposition that Boghead charcoal can be had at the rate of $3·00 [=12*s.*] per ton.

Table showing the cost of Purifying one uniform Quality of Feculent Sewage by the several Articles mentioned.

Boghead charcoal (coke),	$3·00
Nitric acid,	8·50
Black oxyd of manganese,	9·25
Chlorid of lime,	10·75
Peat charcoal,	11·00
Subchlorid of iron (imperfect),	11·25
Animal charcoal,	16·75
Chlorid of manganese (imperfect),	17·50
Bichlorid of mercury,	18·00
Impure chlorid of zinc in damp powder,	26·00
Chlorid of zinc in solution as patented by Sir Wm. Burnett,	37·00
Sulphate of copper,	39·00

The sulphates of zinc, iron, and alumina; common gypsum; sulphuric, sulphur-

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when employed comparatively with coal-tar and plaster, alternately upon the same patients, proved to be less efficacious, less convenient and more disagreeable than the latter.

Mixed plaster and charcoal,—proposed by Herpin of Metz, irritates the wounds, disinfects badly, and soils everything it touches.

Carbonic acid,—proposed by the same author, appears to the committee to be too difficult of application in practice, though theoretically founded upon important analogies.

Bituminous Water of Visos—proposed by Manne, and the mud of rivers used as a poultice by Desmartis, do not appear to be susceptible of being substituted for the mixture of Corne and Demeaux.

The following substances have long ago acquired a place, each in its own way, in the class of disinfectants.

Tincture of iodine has been employed as an antiseptic by hospital surgeons since 1823. By modifying the surfaces to which it is applied, it usually improves the appearance of the pus, lessens its acridity, and is, to a certain extent, antagonistic to putrid infections. It disinfects, however, only incompletely, causes severe pain when applied to open wounds, and would be expensive if used on a large scale; finally, the odor of iodine is neither agreeable nor unattended by inconveniences.

Perchloride of iron has been used for some twelve years in hospitals as an antiseptic and as a means of modifying certain wounds, and putrid or sanguineous foci.—Without diffusing the disagreeable odor of tincture of iodine, it has, like the latter, the fault of disinfecting badly, of causing much pain, and of acting violently upon the diseased tissues, besides injuring the cloths which are soaked in it even more than is the case with the coal-tar and plaster.

Both iodine and the salt of iron just mentioned, are in fact agents of another order; they have rendered, and do still render important services. They are certainly well worth preserving, but should not be compared with the mixture of coal-tar and plaster.

Nitrate of lead,* *Creosote*, and other substances which have been proposed at one time or another, have not realized the expectations of their inventors; their price has been too great, their employment required too much care, or their action has not been sufficiently certain that they could be advantageously used in practice. There is, nevertheless, one of these which deserves special mention, viz., *chlorine*. Ever since Guyton Morveau demonstrated the true action of muriatic acid upon putrefying animal matters, the efficacy of chlorine has been tested in almost innumerable ways. Solutions of chlorine, of "chlorid of soda," and of "chlorid of lime," have rendered signal services to medicine and in the cause of public

ous, and muriatic acids; peroxyd of iron, highly dried clay, litharge, and saw-dust were found imperfect even when very large quantities were employed.

Arsenious acid and creosote on the contrary, were very active; but the danger of a subsequent evolution of arseniuretted hydrogen in the first case, and the difficulty of diffusing an oily fluid like creosote in the second, seemed to interdict the use of these substances. F. H. S.

* [An excellent, though somewhat expensive "disinfecting fluid" (Ledoyen's), which was quite extensively used in this country a few years since, consisted, according to analyses of F. E. Holyoke, of an aqueous solution of this salt.—F. H. S.]

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health, especially since Labarraque, some thirty years since, indicated an improved method of employing them. But the odor of chlorine, disagreeable in itself, is neither easily borne nor devoid of inconveniences. Wounds, moreover, hardly accommodate themselves to it any better than the sense of smell, whenever somewhat large doses of it are required.

Chlorinated Sponge.—The idea of applying sponges saturated with chlorinated solutions, directly upon purulent or gangrenous wounds, as suggested by Hervieux, appears to be excellent for certain cases. Such sponges, renewed several times per day, absorb the pus gradually as it forms better than anything else, and disinfect the wound very well. Unfortunately, chlorine rapidly alters or destroys the sponges and soon causes undue irritation. While this method, therefore, is an excellent one for cleaning certain gangrenous and sinuous wounds, it is, nevertheless, inferior in most instances to the mixture of coal-tar and plaster.

Subnitrate of bismuth—suggested by Frémy as an absorbent and disinfectant, was applied to a large number of wounds. Upon large cavernous cancers it disinfected somewhat better than Peruvian bark, charcoal, or chlorate of potash, but less than the coal-tar and plaster. By its use, however, several bad looking wounds were cleansed quite rapidly. Since it causes no pain or irritation, and since it neither soils the skin nor the clothes, the subnitrate of bismuth is preferable to a multitude of other antiseptic powders; but it is useful rather as a solidifier (*incarnatif*), or dryer, than as an absorbent or disinfectant.

In their *résumé* the committee affirm:

I. That coal-tar mixed with plaster, according to the formula of Corne, (see this Journal, xxviii, 426), can disinfect putrefying organic matters. Mixed with alvine dejections this powder destroys their odor, and leads one to hope that by its aid profound reforms in the present system of maintaining and clearing out cess-pools, &c., may some day be brought about. For this purpose, ordinary earth, coal-ashes, or sand may be substituted for the plaster, as Cabanes prefers, being at least equally efficacious.

II. In therapeutics the coal-tar and plaster has fulfilled only a part of its promises. As a disinfectant in the dissecting-room, upon the folds of bandages,—everywhere where there is infectious matter, its qualities are incontestable. This is also true as regards putrid or gangrenous foci, fetid suppurations, sanious wounds, ichorous putrilagenous cavities, hospital gangrene, &c.; but upon acute and exposed wounds, or upon ordinary wounds or ulcers, other topical applications are preferable to it.

III. Used with lint upon cloths, with pomades or cerate, it has afforded no useful result, and nothing has occurred to prove that when administered internally it has produced the least benefit.

IV. As an absorbent it leaves much to be desired, although it is not entirely devoid of action. When applied as a poultice, in particular, it absorbs very incompletely. For that matter the mixtures of coal-tar with earth or with other powders, absorb still less than the mixture of Corne and Demeaux, and are scarcely at all applicable in therapeutics. In this connection it must not be forgotten that the morbid liquids, and particularly pus, are very different from water. A substance like plaster, for example, which absorbs water strongly, might be incapable of absorbing pus. It is nevertheless true, that as an absorbent, the mixture of coal-

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tar and plaster, either as powder or as poultice, is of some use upon fetid and putrid wounds or suppurations.

V. Cellulose, glycerine and cherry-laurel water; chlorate of potash mixed with talc, clay, marl or kaolin, are neither sufficiently efficacious, nor in application are they convenient enough to be retained in practice.

VI. The mixture of saponine and coal-tar does not appear to be preferable for dressing wounds to many other liquids already known,—tincture of aloes for example. The same may be said of the mixed coal-tar and charcoal of Herpin; nor does it seem as if carbonic acid should be used, unless some improved method of applying it can be devised.

VII. The Boghead residue would be useful only in lack of coal-tar and plaster. While charcoal in porous envelopes does not mould itself to cavernous and sinuous wounds with sufficient readiness to come into general practice.

VIII. From its low price, and by its action, at once mild, absorbent, and disinfectant, as well as by its drying properties, the subnitrate of bismuth will render important services in default of the mixture of coal-tar and plaster. It is even preferable to this when the wounds are accompanied or surrounded with heat or irritation.

IX. Tincture of iodine and perchlorid of iron act rather by modifying the surfaces of wounds and of purulent foci, than as absorbents or disinfectants. They have their special applications in surgery, but agents of this sort are not comparable with the mixed coal-tar and plaster.

X. Sponges soaked in chlorinated water can also render good service upon pale, burrowing sores and upon gangrenous foci.

We have occupied ourselves, say the committee, only with the practical or experimental side of the question. A discussion of its theoretical or chemical bearings would have carried us too far. Moreover, since the authors of the different communications which have been submitted to us have themselves neglected this for the most part, it has seemed to us useless to treat of it at present; whether it be the phenic acid or rosolic, or brunolic acid, or the anilin, picolin, etc., of the coal-tar, which disinfects, is in reality of but little importance. Science will inform us of this some day no doubt; for the moment we have merely to ascertain whether or no the various disinfectants which are brought to us do really disinfect.

After citing the labors of various persons who have proposed methods of disinfection, the committee go on to say: "M. Corne, and the authors indicated above, occupied themselves only with the disinfection and the solidification of animal matter, having in view the preparation of manures. * * * * It is M. Demeaux who appears first to have had the thought of applying to fetid wounds, in surgical practice, the powder invented, or adopted and extolled, by his neighbor. In addition, it is evident that here, as is the case with so many other complex facts with which science is enriched, there is, so to say, neither invention nor priority for any one. The subject has been worked upon for more than a century—a multitude of savans having competed with each other in studying it. Little by little the evolution of the discovery has been effected. M. Corne disengaged it from its gangue a little better than his predecessors, and Demeaux, knowing, perhaps, that from time immemorial sailors and the inhabitants of certain southern countries often dress their wounds

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with tar; that tar water and pomades of tar are frequently employed in medicine, has extended its applications to therapeutics."

"Many other efforts are still necessary. In point of fact the results thus far obtained are merely rough outlines,—only first trials. So long as the world at large is not in possession of a simple, easy, and economical method, accessible to every one, which shall be capable of disinfecting immediately, and without inconvenience on the large or small scale, dejections and filth of all kinds, in dwellings as well as in privies or slaughter-houses; in dissecting-rooms and the like, as well as in the sick room, upon wounds, improvements will still be wanted; there will yet be room for new attempts. While recording those of to-day and those of yesterday upon the road already traversed, let us be careful not to diminish the ardor of the laborers in the future, who will finally endow civilization with a complete and general disinfection."

Finally, certain indispensable precautions must be followed, in order to obtain from the process of Corne and Demeaux its proper effect. It is evident, from having neglected some of these precautions, that different experimenters have been led to believe that the method is useless. Fine moulding plaster, and not the common article, should be employed. The coal-tar, which is mixed with it in the proportion of 2 to 4 parts to a hundred, by triturating or grinding, ought to impart to it a gray tint, without destroying its dry, pulverulent condition. Objects to be disinfected should be rolled in this powder until each point upon their surfaces has been brought in contact with it. Gangrenous or putrid sores should be covered with thick layers of it, by handfuls, several times per day. If one is treating pus, blood, dejections, or the like, enough of the powder should be added to form a paste of the mass, taking care to replace the first layer of powder by another as soon as it no longer absorbs any more. Mixed with oil to the consistence of a thick pap, it forms poultices of convenient application, if they are made thick and broad.

Within the limits which have been indicated the mixture of coal-tar and plaster is a good disinfectant, and may be recommended for use in domestic economy as well as in hospitals. "What we have ourselves seen leaves no uncertainty of the reality of this property, nor of the possibility of its application." * * * * * It remains only to draw from it reasonable, practical consequences, either taking the fact as it is, or by modifying and perfecting it.—*Comptes Rendus*, l, 279.

[For corroborating testimony, received by the committee from various sources, the reader is referred to their report in question. Numerous other articles upon the subject, by different authors, may also be found in vol. xlix of the *Comptes Rendus*.—F. H. S.]

7. *Decoloration of Indigo by Sesquioxyd of Iron.*—[In the May number of this Journal we took occasion to maintain that the very interesting fact of the power of ferric salts to bleach solutions of indigo was first observed by Prof. H. Wurtz. Since then we have accidentally learned that this claim was erroneous, as will appear from the following statement made by Wœhler some twenty years since. "When a solution of indigo in sulphuric acid is mixed with salts of the sesquioxyd of iron and heated, it will be decolorized precisely as it would be by nitric acid."—(Annalen der Chemie und Pharmacie, 1840, xxxiv, 235; see also Gmelin's Handbook (Cavendish Soc. Edit.), xiii, 59.—F. H. S.]

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II. GEOLOGY.

1. *On Some Points in Chemical Geology;* by T. STERRY HUNT, F.R.S. (Read before the *Geol. Society of London*, January 5th, 1859—published in the *Quarterly Journal* of the Society for November, 1859—and reprinted, with additional notes by the author, in the *Canadian Naturalist* for January, 1860.).—In this paper the author discusses a number of questions which lie at the foundation of a true history of the chemistry of the earth's crust, and gives farther developments to some of his peculiar views, which were for the most part, first enunciated in this Journal. In regard to the metamorphism of sedimentary deposits, *i. e.*, the conversion of sands, clays and marls into crystalline stratified rocks, the author, after distinguishing between local and normal metamorphism, insists upon the frequent interstratification of unchanged fossiliferous limestones among crystalline schists as evidence that heat has not been the only agent in the metamorphism, which has moreover been effected at temperatures not very elevated, and by the intervention of alkaline solutions, in the absence of which, sediments may be heated to the same degree without change.

The first announcement of this view will be found in this Journal for May, 1857 (vol. xxiii, p. 407), where the author, after describing some experiments with the alkaline silicates, expresses the opinion that "we have here the explanation of rock metamorphism in general." Farther inquiries into the action of the soluble alkaline silicates will be found in this Journal for March and May, 1858 (vol. xxv, pp. 287–437), where the subsequent experiments of Daubrée are cited in confirmation and extension of Mr. Hunt's theory of the normal metamorphism of sediments at comparatively low temperatures by the intervention of alkaline carbonates and silicates, which may be either liberated by the decomposition of the sediments themselves or derived from adjacent strata. These salts in solution permit the crystallization of feldspars and micas, or when alkaline bases are present only in smaller quantity, of kyanite, andalusite and staurotide, while by the intervention of protoxyd bases, garnet, epidote, chloritoid and chlorite are formed, and in the absence of the argillaceous element, pyroxene, olivine, serpentine and talc. In a subsequent note the author has however alluded to the probable direct formation of certain silicates of magnesia and lime, in open basins at the earth's surface and by reactions at the ordinary temperature. This Journal, March, 1860 (xxix, 284).

In the second place the author discusses the relations of plutonic to metamorphic sedimentary rocks, and concludes that the latter, becoming plastic under the influence of water and heat, may be displaced by disturbance and pressure, thus taking the form of intrusive rocks. Sediments altered *in situ* he distinguishes as indigenous, and those displaced as exotic plutonic rocks. The conclusions of Scrope, Scheerer and Elie de Beaumont, supported by the late observations of Daubrée and Sorby, as to the aqueo-igneous fusion of these rocks, are fully admitted.

In the third place the author discusses the theories of Phillips, Bunsen and Durocher, as to the origin of intrusive rocks, and rejecting the notion that these are derived from the supposed fluid interior of the earth, re-

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gards them as in all cases, fused and displaced sediments. He proceeds to show that the action of waters removing from permeable strata their soda, lime and magnesia, brings these to the composition of granitic rocks, while the finer and less permeable sediments, retaining their protoxyd bases, give rise by subsequent alterations to basic rocks with triclinic feldspars and pyroxene. Soda being preëminently the soluble alkali, has been gradually removed from disintegrated feldspathic rocks under the influence of water and carbonic acid, and the carbonate of soda thus formed has by its reaction with the lime salts of the ancient ocean, given rise to sea-salt and to the carbonate of lime with which the limestones have been built up. The aluminous silicate set free in the decomposition of the feldspars is thus the equivalent of the earthy carbonates and sea-salt which are formed. Hence we find that in the oldest known crystalline rocks, those of the Laurentian series, soda feldspars are abundant, micaceous schists rare, and argillites or silicates of alumina deficient in alkali are unknown, while in higher formations, argillites and schists with kyanite, chiastolite and staurotide abound, as well as chlorite, chloritoid, muscovite, garnet, epidote, etc., showing a great excess of aluminous silicate over the alkali required to form feldspars.

These views have already appeared in a communication from the author in this Journal (vol. xxv, p. 436) where also the action of organic matters in deoxydizing, dissolving and removing oxyd of iron from certain strata to be accumulated in others, is discussed and illustrated by a consideration of the iron deposits of various ages. The existence of beds of iron ore in the Laurentian rocks, not less than the graphite and metallic sulphurets which these contain, is by the author regarded as evidence that organic life existed at the period when these rocks were deposited. As Mr. Hunt has elsewhere explained, he supposes the condition of the cooling globe to have been one of thorough oxydation, and regards all processes of reduction or deoxydation as dependent either directly or indirectly upon organic life. In regard to iron oxyd however he remarks that its solution may sometimes be due to mineral acids, from volcanic or other sources; such solutions, and others from the oxydation of pyrites, may be decomposed by alkaline or earthy carbonates and give rise to iron deposits. A similar process with aluminous solutions will serve to explain the origin of corundum, beds of emery, and aluminous iron ores.

In regard to the agency of organic matters in the formation of iron deposits we may here remark that a reviewer in this Journal (xxv, 245) in noticing Mr. Hunt's observations on this subject contained in his Geological Report for 1856, writes as if Mr. H. had appropriated the views of Bischof. In truth neither Bischof nor Hunt ever claimed any originality in bringing forward a principle which has long been understood, and which they have only attempted to extend and develope. We may here observe that the same reviewer fails to apprehend Mr. Hunt's views on the formation of crystalline rocks, when he says that Bischof and Hunt agree in supposing all rocks to be formed by chemical agencies in the presence of water, and that therefore the latter cannot claim originality. Now upon the point in question there is little or no affinity between the views of the two writers, for the simple reason that Bischof seems never for a moment to apprehend the nature of the great problem which Mr. Hunt has under-

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taken to solve, but with Dana regards normal metamorphism as pseudomorphism on a grand scale. The ingenious speculations of Bischof and others on the possible alteration of mineral species by the action of various saline and alkaline solutions may pass for what they are worth, although we are satisfied that by far the greater part of the so-called cases of pseudomorphism in silicates are purely imaginary, and when real are but local and accidental phenomena. Bischof's notion of the pseudomorphism of silicates like feldspars and pyroxene presupposes the existence of crystalline rocks, whose generation this neptunist never attempts to explain, but takes his starting point from a plutonic basis.

The problem to be solved, as we have elsewhere insisted, is the conversion of sands, clays and marls, (consisting of silica, silicates of alumina, carbonates of lime, magnesia and oxyd of iron, derived by chemical and mechanical agencies from the ocean waters and pre-existing crystalline rocks,) into aggregations of crystalline silicates. These metamorphic rocks, once formed, are liable to alteration only by local and superficial agencies, and are not, like the tissues of a living organism, subject to incessant transformations, the pseudomorphism of Bischof and Dana. As yet, Mr. Hunt is the only one who has attempted a rational explanation, based on experiments, of the problem of the conversion of sedimentary strata into crystalline rocks, and his views, whether true or false, are to be judged by themselves, and not by comparison with those of Bischof or any other writer. Among the geologists who since the time of Hutton, have best comprehended the nature of the problem of rock metamorphism, are Boué, Virlet and Dolanouë. We hope at an early day to discuss in the pages of this Journal, the question of mineral pseudomorphism, as well as the history and theory of metamorphism.

Passing from plutonic to volcanic phenomena, Mr. Hunt proceeds to develope the views of Babbage and Herschel as to the effects of the internal heat upon deeply buried sediments. Babbage has shown that the expansion of the sedimentary rocks by heat may cause great vertical movements, while in the subsequent fusion of the heated sediments Herschel finds an explanation of volcanic phenomena. These views the author adopts, and enters into a consideration of the relations which must take place between silica and silicates, carbonates, sulphates, chlorids and organic matters in the presence of water at an elevated temperature. To azotized organic substances and to the ammonia condensed in argillaceous strata, he ascribes, with Bischof, the ammoniacal salts of volcanoes.

In considering the metamorphism of the strata, which must always precede volcanic action, we are not to lose sight of a process which will, in its results, be the reverse of that insisted upon by Babbage. It is the great contraction which must result, not only from the solidification of the porous sediments, but from the condensation attendant upon chemical combination, by which they are converted into silicates of high specific gravity, such as pyroxene, garnet, epidote, chloritoid and chiastolite. In this way, as remarked by Mr. Hunt in his lectures before the Smithsonian Institution at Washington last winter, we may realize, to a certain extent, Elie de Beaumont's notion of a shrinking of the earth's nucleus, and find an explanation of many phenomena of subsidence and corrugation, although with Herschel we are to attribute these for the most part to "the disturb-

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ance of the equilibrium of pressure consequent upon the transfer of sediments while the yielding surface reposes upon a mass of matter partly liquid and partly solid."

We conclude with the following extracts, and with two notes appended to the Canadian reprint of Mr. Hunt's papers, in one of which he calls attention to the remarkable work of Keferstein, whose ingenious views, too much in advance of his time, have hitherto been overlooked.

"The metamorphism of sediments *in situ*, their displacement in a pasty condition from igneo-aqueous fusion as plutonic rocks, and their ejection as lavas with attendant gases and vapors are then all results of the same cause, and depend upon the differences in the chemical composition of the sediments, the temperature, and the depth to which they are buried: while the unstratified nucleus of the earth, which is doubtless anhydrous, and according to the calculations of Messrs. Hopkins and Hennessey, probably solid to a great depth, intervenes in the phenomena under consideration only as a source of heat." *

"The volcanic phenomena of the present day appear, so far as I am aware, to be confined to regions covered by the more recent secondary and tertiary deposits, which we may suppose the central heat to be still penetrating (as shown by Mr. Babbage), a process which has long since ceased in the palæozoic regions. Both normal metamorphism and volcanic action are generally connected with elevations and foldings of the earth's crust, all of which phenomena we conceive to have a common cause, and to depend upon the accumulation of sediments and the subsidence consequent thereon, as maintained by Mr. James Hall in his theory of mountains. The mechanical deposits of great thickness are made up of coarse and heavy sediments, and by their alteration yield hard and resisting rocks; so that subsequent elevation and denudation will expose these contorted and altered strata in the form of mountain chains. Thus the Appalachians of North America mark the direction and extent of the great accumulation of sediments by the oceanic currents during the whole

* "The notion that volcanic phenomena have their seat in the sedimentary formations of the earth's crust, and are dependent upon the combustion of organic matters, is, as Humboldt remarks, one which belongs to the infancy of geognosy. In 1834 Christian Keferstein published his *Naturgeschichte des Erdkörpers*, in which he maintains that all crystalline non-stratified rocks, from granite to lava, are products of the transformation of sedimentary strata, in part very recent, and that there is no well-defined line to be drawn between neptunian and volcanic rocks, since they pass into each other. Volcanic phenomena according to him have their origin, not in an igneous fluid centre, nor an oxydizing metallic nucleus, but in known sedimentary formations, where they are the result of a peculiar process of fermentation, which crystallizes and arranges in new forms the elements of the sedimentary strata, with evolution of heat as an accompaniment of the chemical process. (*Naturgeschichte*, vol. i, p. 109, also *Bull. Soc. Géol. de France* (1) vol. vii, p. 197).

These remarkable conclusions were unknown to me at the time of writing this paper, and seem indeed to have been entirely overlooked by geological writers; they are, as will be seen, in many respects an anticipation of the views of Herschel and my own; although in rejecting the influence of an incandescent nucleus as a source of heat, he has, as I conceive, excluded the exciting cause of that chemical change, which he has not inaptly described as a process of fermentation, and which is the source of all volcanic and plutonic phenomena. See in this connection my paper *On the Theory of Igneous Rocks and Volcanoes*, in the Canadian Journal for May, 1858."

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palæozoic period; and the upper portions of these having been removed by subsequent denudation, we find the inferior members of the series transformed into crystalline stratified rocks."*

III. BOTANY AND ZOOLOGY.

1. *Flora of the Southern United States, containing abridged descriptions of the Flowering Plants and Ferns of Tennessee, North and South Carolina, Georgia, Alabama, Mississippi, and Florida; arranged according to the Natural System;* by A. W. CHAPMAN, M.D. (The Ferns by Daniel C. Eaton.) New York: Ivison, Phinney & Co. 1860. pp. 621, small 8vo.—The first thing that strikes our attention as we open this volume, is its neat and tasteful typography. It is a decided advance upon its counterpart, Gray's Manual for the Northern States, in this respect, and indeed is the handsomest volume of the kind we know of. It is only just to add that the book was produced by the University Press of Welch, Bigelow & Co., Cambridge. The matter of the volume is, we trust, as good as its form. It well supplies a long-felt and pressing want, and gives to schools and colleges, and to botanical students generally at the South, a work which is for that district what Gray's Manual is for the northern section of our common country. Having said this, modesty prevents more particular eulogium. If experience annually shows that the work with which this volume is compared is not yet perfect, but still requires many minor emendations, notwithstanding long pains-taking and

* "The theory that volcanic mountains have been formed by a sudden local elevation or tumefaction of previously horizontal deposits of lava and other volcanic rocks, in opposition to the view of the older geologists who supposed them to have been built up by the accumulation of successive eruptions, although supported by Humboldt, Von Buch, and Elie de Beaumont, has been from the first opposed by Cordier, Constant Prevost, Scrope and Lyell. (See Scrope, *Geol. Journal*, vol. xii, p. 326, and vol. xv, p. 500; also Lyell, *Philos. Trans.*, part 2, vol. cxlviii, p. 703, for 1858.) In these we think will be found a thorough refutation of the elevation hypothesis and a vindication of the ancient theory.

This notion of paroxysmal upheaval once admitted for volcanoes was next applied to mountains which, like the Alps and Pyrenees, are composed of neptunian strata. Against this view, however, we find De Montlosier in 1832 maintaining that such mountains are to be regarded as the remnants of former continents which have been cut away by denudation, and that the inversions and disturbances often met with in the structure of mountains are to be regarded only as local accidents. (*Bul. Soc. Geol.*, (1) vol. ii. p. 438, vol. iii, p. 215.)

Similar views were developed by Prof. James Hall in his address before the American Association for the Advancement of Science, at Montreal in August, 1858. This address has not been published, but these views are reproduced in the first volume of his *Report on the Geology of Iowa*, p. 41. Mr. Hall there insists upon the conditions which in the ancient seas, gave rise to great accumulations of sediment along certain lines, and asserts that to this great thickness of strata, whether horizontal or inclined, we are to ascribe the mountainous features of northeastern America as compared with the Mississippi valley. Mountain heights are due to original deposition and subsequent continental elevation, and not to local upheavals or foldings, which on the contrary, give rise to lines of weakness, and favor erosion, so that the lower rocks become exposed in anticlinal valleys, while the intermediate mountains are found to be capped with newer strata.

In like manner J. P. Lesley asserts that 'mountains are but fragments of the upper layers of the earth's crust,' lying in synclinals and preserved from the general denudation and translation. (*Iron Manufacturer's Guide*, 1859, p. 53.) See also his admirable little volume entitled *Coal and its Topography.*"

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repeated revisions, it may be expected that equal experience will reveal similar imperfections in the new and untried work. None but a practised teacher can tell beforehand where the pupil, or the student without a teacher will encounter obstacles, and the most experienced can only partially anticipate them. They must be found out by trial, and be corrected in new issues,—for which electrotyping offers great facilities. For the young student the Artificial Analysis of the Natural Orders is practically the key to the whole thing; a perfect key of this sort was never made at one trial; in fact most keys in such works fail very largely. So we may safely hazard the prediction that Dr. Chapman's artificial analysis will need emendation in a future edition. We are bound to add, however, that a half dozen of trials has resulted in only one failure.

This book is wanted by botanists as well as by students, and we think they will be well pleased with it. Its merits are manifold; its deficiencies are either such as are natural if not unavoidable under the circumstances of its production, or such as pertain to all works of the kind, in which knowledge is progressive, the records of this knowledge widely dispersed, and the facts to be observed and digested into order almost infinitely numerous. An introduction gives a good condensed sketch of the Elements of Botany, and a Glossary of Botanical Terms, so that the book can be used independently; though such a book as the First Lessons in Botany ought to precede and accompany its use by the student. Very appropriately is this volume dedicated to one of the worthiest botanists of the Southern States, the Rev. Dr. Curtis. Now that the Southern Atlantic states are provided with a good Flora of their own, we trust that botany will receive a new impulse, both as a scientific pursuit and as a branch of education, in that favored region. A. G.

2. *Synopsis of Dalbergieæ, a Tribe of Leguminosæ;* by GEORGE BENTHAM, Esq.—This is a (botanical) supplement to the fourth volume of the Journal of the Proceedings of the Linnæan Society, a critical account of this group of plants, and a technical synopsis of the genera and species,—of much importance therefore to the systematic botanist. Since its publication, better specimens have been examined of one of Fendler's Venezuelan plants, No. 2223, referred to on p. 17; and Mr. Bentham finds this a new generic type, *Fissicalyx.* A. G.

3. *Reports on the Natural History, Climate, and Physical Geography of Minnesota, Nebraska, Washington, and Oregon Territories; made in connection with the Survey of a Pacific Railroad Route, along the forty-seventh and forty-ninth parallels of latitude, in* 1853–4–5–6, *under the command of Governor* I. I. STEVENS, *of Washington Territory;* by G. SUCKLEY, M.D., and J. G. COOPER, M.D., Surgeons and Naturalists to the Expedition. 4to, pp. 399, 26, with 65 plates, and isothermal charts, &c. New York, Bailliere Brothers.—This is a separate issue of the 12th volume of the Pacific Railroad Exploration Reports, containing all that relates to sciences in the report of Gov. Stevens' survey of the northern proposed route, and also a preface and other additional matter by the enterprising authors, Drs. Cooper and Suckley. This issue is, we understand, rendered necessary by the cutting down of the government edition in the later volumes of the series to a much diminished number. The authors have taken this opportunity not merely to add new matter, and

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make certain emendations, but also to insert four or five pages of errata, correcting typographical errors, perhaps a tithe of those contained in the volume. This remark does not at all militate against the statement that in this volume "there are fewer errors than is usual in similar government publications." We are satisfied of the truth of this. The mass of scientific reports published by Congress and printed at Washington are marred beyond all endurance by want of proof-reading, or perhaps of proof-correction,—for in some instances proofs were furnished and revised, but the corrections were never made. The folly of the late system having now been exposed in other and more considerable respects, we may hope for the inauguration of a much better and more economical plan.

The first chapter of the present very interesting volume (whether peculiar to this edition or no is more than we can now ascertain) is separately paged, and is devoted to the meteorology and climate of Nebraska and Washington Territories. The Botanical Report, which comes next in order, consists, 1. of a very interesting and useful general sketch of the botany of the route, in reference to the character of the vegetation, geographical distribution, &c., mingled with zoological and climatal observations. 2. A catalogue of the plants collected east of the Rocky Mountains, containing three before undescribed species and one new (Chenopodiaceous) genus,—all of which are characterized by Dr. Torrey, as will be seen, although the list was drawn up by Dr. Gray. This portion is illustrated by four excellent plates, one of which is devoted to the *Endolepis Suckleyi* of Torrey, the new genus referred to. 3. Catalogue of Plants collected in Washington Territory; with observations, &c., drawn up by Dr. Cooper himself, including notes, characters, &c., supplied by Dr. Gray, Dr. Torrey, and Prof. Thurber; with two plates.

The remainder and most striking part of the volume is the Zoological Report; that on the Insects by Dr. Leconte; on the Mammals by Dr. Cooper, Dr. Suckley and Mr. Gibbs; on the Birds, by Drs. Cooper and Suckley; on the Reptiles, by Dr. Cooper; on the Fishes by Dr. Suckley; on the Mollusca by Wm. Cooper, Esq. (a veteran naturalist whom we gladly welcome back to active labor); on the Crustacea by Dr. Cooper. The zoological plates are many and truly beautiful; those of the birds are colored. The volume sells for ten dollars; and this small separate issue will doubtless be taken up at once, to complete the sets of the Pacific Railroad Reports. A. G.

4. *Potamogeton crispus* L. was introduced into the North American flora by Pursh, and said to occur from "Canada to Virginia." Dr. Torrey, in his Flora of the Northern States, mentions it as from Lake George; but as he omits it from his recent Flora of the State of New York, we infer that there was some mistake in the first instance. Prof. Tuckerman, who has paid great attention to this difficult genus, not having found *P. crispus* in this country, and not having ourselves met with it, the species was excluded from the Manual of the Botany of the Northern United States; but a remark was added in the second edition of this work, that Mr. Tuckerman had seen a specimen in some European herbarium purporting to come from Delaware. It may also be noted that this species has for many years been growing in a pool in the Botanic Garden at Cambridge,—where in fact it cannot be got rid of,—and there

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is a tradition that it was introduced into the pool by Mr. Nuttall. Last year Mr. Edward Tatnall, an intelligent botanist and horticulturist, detected the plant in question in Delaware, in the vicinity of Wilmington, and this season he finds it to occur abundantly, under circumstances which give it a good claim to be regarded as indigenous. So *Potamogeton crispus* must be restored to our flora. If really indigenous it probably occurs in other stations. Its early flowering and fruiting, compared with the other species (viz., blossoming in May) may have led to its being overlooked; but the species is probably local in this country. It is, however, so vigorous and so difficult of eradication where it is established, so likely therefore to hold its own or to extend, that, if not detected elsewhere, we may believe that it was recently imported into this country, as another water-weed, the *Anacharis* of North America, was into England, where it has spread prodigiously with a few years. A. G.

5. *Marsilea quadrifolia*, L.—Aquatic plants, especially those of low type, are in general so widely diffused geographically, that the absence from North America of the above named plant,—so common throughout the northern part of the Old World,—has always seemed rather exceptional. We have now to announce its actual occurrence here. It has just been discovered on the muddy borders of a pond in Litchfield, Connecticut, by Dr. Timothy F. Allen. This adds another instance of the apparently extremely local occurrence in this country of a common European species, of which *Scolopendrium officinarum* and *Subularia aquatica* are cases in point. As it is not likely that this Marsilea was created for Litchfield pond, or for any other few localities, if such there be, in this country, such plants must be regarded as of recent and casual introduction—which is most improbable—or else as species once diffused over the country, but now on the verge of extinction from this flora,—a view which chimes in with other inferences about geographical distribution.

A. G.

6. *Catalogue of the Acanthopterygian Fishes in the collection of the British Museum;* by Dr. A. Gunther. 8vo, pp. 524. London, 1859. —In this volume all the fishes in the British Museum, belonging to the families mentioned are described, and abstracts are given of the characters of many others, not in that collection. The work must be therefore, useful to naturalists, and especially to students, as the author has been quite diligent in collecting the indications of species from all sources. Considerable discretion must however be used in consulting it, as the author appears to have considered many species that he had not himself seen, as being very "doubtful" or as identical with some known to him. In his work, our American fresh-water fishes are especially in great disorder. None of the genera recently established by Agassiz and Girard are adopted. The *Calliuri* are placed partly in *Centrarchus* and partly in *Brythus*. The *Grystes* are distributed in *Centrarchus* and *Huro* as well as *Grystes*. If recent American works had been consulted these errors would not have occurred. Some species are regarded as identical which have no close relationship to each other, as *Pomotis falax*, B. and S., and *P. rubricanda* Storer. In many other points Dr. Gunther differs from the best Ichthyologists who have hitherto treated of the order. Thirteen genera and forty species are described as new. W. S.

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IV. ASTRONOMY.

1. *First Comet of* 1860.—(Gould's Astron. Jour. No. 134).—"A letter from Mr. Liais, Director of the Brazilian Coast Survey, to Prof. Peters, published in the *Astronomische Nachrichten*, No. 1248, announces the discovery, February 26th, at Olinda, Brazil, of a faint double comet, near the star μ Doradûs. The larger portion preceded; it was brighter on the side toward the sun, and sensibly elongated in that direction, having near the extremity a small luminous point, about as bright as a star of the 9th magnitude. The object was so faint as to render observations difficult, for which reason the diameter could not be measured; but Mr. Liais estimated it at 25 or 30 seconds in the larger, and 7 or 8 seconds in the smaller diameter. The second or smaller nebulosity appeared nearly circular, and about 4 seconds in diameter. On February 27th, at $10^h\ 25^m$ it followed the other by about 27 seconds, being about $1'\ 8''$ to the North of it. On March 3d, at $11^h\ 16^m$, the difference of position was 23 seconds in right ascension, and $46''$ in declination. On the 6th, the moonlight wholly extinguished the comet.

From observations made by Mr. Liais with the ring-micrometer, Dr. Pape, of Altona, has computed three normal places, and thence deduced elements as follows, viz.:—

Time of perihelion passage, Febr. 16·7667, Berlin m. t.

Long. of perihelion,	173° 26′ 2	App. eqx.
" " asc. node,	324 1 9	Febr. 29·6
Inclination,	79 22 6	
Log. perih. dist.,	0·07652	
Motion,	Direct.	

2. *Second Comet of* 1860.—On the 17th of April, 1860, a telescopic comet was discovered by Mr. George Rümker, of Hamburg. It was a faint, ill-defined nebulous spot of light. Having passed its perihelion at the close of the preceding February, it was when discovered receding from the sun. Its elements, approximately determined, are found to resemble those of the second comet of 1793.

3. *On the alleged intra-Mercurial Planet.*—According to the elements assigned by M. Leverrier to the planet which Dr. Lescarbault states that he observed passing across the sun March 26, 1859, there was reason to expect that the planet would be seen in like transit, sometime in March or April of the present year. In the hope of detecting such a transit the sun's disc was closely watched during these months by observers in three places at least in this country, and doubtless also by many observers in Europe. So far as we hear, the search has everywhere proved unsuccessful. But as during this period there were many hours in which the sun's disk was not and could not be under observation, the failure does not disprove the existence of such a planet. It is to be hoped that the search will be resumed hereafter, and as there is great uncertainty respecting the inclination of the planet, a thorough observation at any time, with a magnifying power of 100 or more will be valuable. If the spot seen on the sun, February 12, 1820, by Steinhübel and by Stark, (Mon. Not. Roy. Astr. Soc., March 9, 1860,) was this planet, the inclination must be quite small, and a transit across the sun may often occur.

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Lescarbault's alleged observation of March 26, 1859, is contradicted by M. Liais, in No. 1248 of the Astronomische Nachrichten. The latter states that on that day he was observing, with a view of determining the relative intensities of the different parts of the disc, at San Domingos, in the Bay of Rio Janeiro, from $11^h\ 4^m$ to $11^h\ 20^m$, and from $12^h\ 42^m$ to $1^h\ 17^m$ S. Dom. mean solar time. Allowing three hours for difference of longitude, the ingress of the spot should have been at $1^h\ 5^m$ at St. Domingo. At $1^h\ 17^m$ the spot should accordingly have been on the sun's disc for twelve minutes and would in that time have traversed an arc of $1'\ 4$. M. Liais asserts he must have seen such a spot had it been there. He shows further that such a planet as the one supposed would be easily seen during a large part of its orbit, by the telescope, and often by the naked eye of an observer within the tropics. As the planet has not been thus seen he does not believe it to exist.

V. BOOK NOTICES.

Prof. Agassiz on the Origin of Species.

1. *Contributions to the Natural History of the United States;* by L. Agassiz.—The third volume of this work, now in the press, will appear shortly. We copy from the advance sheets the following paragraphs relating to the origin of species, which has lately attracted much attention, in consequence of the publication of Darwin's book on that subject.

Individuality and Specific Differences among Acalephs.

The morphological phenomena discussed in the preceding section naturally lead to a consideration of individuality and of the extent and importance of specific differences among the Acalephs. A few years ago the prevailing opinion among naturalists was that, while genera, families, orders, classes, and any other more or less comprehensive divisions among animals were artificial devices of science to facilitate our studies, species alone had a real existence in nature. Whether the views I have presented in the first volume of this work (p. 163), where I showed that species do not exist in any different sense from genera, families, etc., have had any thing to do with the change which seems to have been brought about upon this point among scientific men, is not for me to say; but whatever be the cause, it is certainly true that, at the present day, the number of naturalists who deny the real existence of species is greatly increased. Darwin in his recent work on the "Origin of Species," has also done much to shake the belief in the real existence of species, but the views he advocates are entirely at variance with those I have attempted to establish. For many years past I have lost no opportunity of urging the idea that while species have no material existence, they yet exist as categories of thought, in the same way as genera, families, orders, classes, and branches of the animal kingdom. Darwin's fundamental idea, on the contrary, is that species, genera, families, orders, classes, and any other kind of more or less comprehensive divisions among animals do not exist at all, and are altogether artificial, differing from one another only in degree, all having originated from a successive differentiation of a primordial organic form, undergoing successively such changes as would

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at first produce a variety of species; then genera, as the difference became more extensive and deeper; then families, as the gap widened still farther between the groups, until in the end all that diversity was produced which has existed or exists now. Far from agreeing with these views, I have, on the contrary, taken the ground that all the natural divisions in the animal kingdom are primarily distinct, founded upon different categories of characters, and that all exist in the same way, that is, as categories of thought, embodied in individual living forms. I have attempted to show that branches in the animal kingdom are founded upon different plans of structure, and for that very reason have embraced from the beginning representatives between which there could be no community of origin; that classes are founded upon different modes of execution of these plans, and therefore they also embrace representatives which could have no community of origin; that orders represent the different degrees of complication in the mode of execution of each class, and therefore embrace representatives which could not have a community of origin any more than the members of different classes or branches; that families are founded upon different patterns of form, and embrace representatives equally independent in their origin; that genera are founded upon ultimate peculiarities of structure, embracing representatives, which, from the very nature of their peculiarities could have no community of origin; and that finally, species are based upon relations and proportions that exclude, as much as all the preceding distinctions, the idea of a common descent.

As the community of characters among the beings belonging to these different categories arises from the intellectual connection which shows them to be categories of thought, they cannot be the result of a gradual material differentiation of the objects themselves. The argument on which these views are founded may be summed up in the following few words: Species, genera, families, &c. exist as thoughts, individuals as facts. It is presented at full length in the first volume of this work, (p. 137–168), where I have shown that individuals alone have a definite material existence, and that they are, for the time being, the bearers not only of specific characteristics, but of all the natural features in which animal life is displayed in all its diversity; individuality being, in fact, the great mystery of organic life.

Since the arguments presented by Darwin in favor of a universal derivation from one primary form, of all the peculiarities existing now among living beings have not made the slightest impression on my mind, nor modified in any way the views I have already propounded, I may fairly refer the reader to the paragraphs alluded to above as containing sufficient evidence of their correctness, and I will here only add a single argument, which seems to leave the question where I have placed it.

It seems to me that there is much confusion of ideas in the general statement of the variability of species so often repeated lately. If species do not exist at all, as the supporters of the transmutation theory maintain, how can they vary? and if individuals alone exist, how can the differences which may be observed among them prove the variability of species? The fact seems to me to be that while species are based upon definite relations among individuals which differ in various ways among themselves,

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each individual, as a distinct being, has a definite course to run from the time of its first formation to the end of its existence, during which it never loses its identity nor changes its individuality, nor its relations to other individuals belonging to the same species, but preserves all the categories of relationship which constitute specific or generic or family affinity, or any other kind or degree of affinity. *To prove that species vary it should be proved that individuals born from common ancestors change the different categories of relationship which they bore primitively to one another.* While all that has thus far been shown is, that there exists a considerable difference among individuals of one and the same species. This may be new to those who have looked upon every individual picked up at random, as affording the means of describing satisfactorily any species; but no naturalist who has studied carefully any of the species now best known, can have failed to perceive that it requires extensive series of specimens accurately to describe a species, and that the more complete such series are, the more precise appear the limits which separate species. Surely the aim of science cannot be to furnish amateur zoölogists or collectors, a *recipe* for a ready identification of any chance specimen that may fall into their hands. And the difficulties with which we may meet in attempting to characterize species do not afford the first indication that species do not exist at all, as long as most of them can be distinguished, as such, almost at first sight. I foresee that some convert to the transmutation creed will at once object that the facility with which species may be distinguished is no evidence that they were not derived from other species. It may be so. But as long as no fact is adduced to show that any one well known species among the many thousands that are buried in the whole series of fossiliferous rocks, is actually the parent of any one of the species now living, such arguments can have no weight; and thus far the supporters of the transmutation theory have failed to produce any such facts. Instead of facts we are treated with marvelous bear, cuckoo, and other stories. Credat Judaeus Apella!

Had Mr. Darwin or his followers furnished a single fact to show that individuals change, in the course of time, in such a manner as to produce at last species different from those known before, the state of the case might be different. But it stands recorded now as before, that the animals known to the ancients are still in existence, exhibiting to this day the characters they exhibited of old. The geological record, even with all its imperfections, exaggerated to distortion, tells now, what it has told from the beginning, that the supposed intermediate forms between the species of different geological periods are imaginary beings, called up merely in support of a fanciful theory. The origin of all the diversity among living beings remains a mystery as totally unexplained as if the book of Mr. Darwin had never been written, for no theory unsupported by fact, however plausible it may appear, can be admitted in science.

It seems generally admitted that the work of Darwin is particularly remarkable for the fairness with which he presents the facts adverse to his views. It may be so; but I confess that it has made a very different impression upon me. I have been more forcibly struck by his inability to perceive when the facts are fatal to his argument, than by anything else in the whole work. His chapter on the Geological Record, in par-

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ticular, appears to me, from beginning to end, as a series of illogical deductions and misrepresentations of the modern results of Geology and Palæontology. I do not intend to argue here, one by one, the questions he has discussed. Such arguments end too often in special pleading, and any one familiar with the subject may readily perceive where the truth lies by confronting his assertions with the geological record itself. But since the question at issue is chiefly to be settled by palæontological evidence, and I have devoted the greater part of my life to the special study of the fossils, I wish to record my protest against his mode of treating this part of the subject. Not only does Darwin never perceive when the facts are fatal to his views, but when he has succeeded by an ingenious circumlocution in overleaping the facts, he would have us believe that he has lessened their importance or changed their meaning. He would thus have us believe that there have been periods during which all that had taken place during other periods was destroyed, and this solely to explain the absence of intermediate forms between the fossils found in successive deposits, for the origin of which he looks to those missing links; whilst every recent progress in Geology shows more and more fully how gradual and successive all the deposits have been which form the crust of our earth.—He would have us believe that entire faunæ have disappeared before those were preserved, the remains of which are found in the lowest fossiliferous strata; when we find everywhere non-fossiliferous strata below those that contain the oldest fossils now known. It is true, he explains their absence by the supposition that they were too delicate to be preserved; but any animals from which Crinoids, Brachiopods, Cephalopods, and Trilobites could arise, must have been sufficiently similar to them to have left, at least, traces of their presence in the lowest non-fossiliferous rocks, had they ever existed at all.—He would have us believe that the oldest organisms that existed were simple cells, or something like the lowest living beings now in existence; when such highly organized animals as Trilobites and Orthoceratites are among the oldest known.—He would have us believe that these lowest first-born became extinct in consequence of the gradual advantage some of their more favored descendants gained over the majority of their predecessors; when there exist now, and have existed at all periods in past history, as large a proportion of more simply organized beings, as of more favored types, and when such types as Lingula were among the lowest Silurian fossils, and are alive at the present day.—He would have us believe that each new species originated in consequence of some slight change in those that preceded; when every geological formation teems with types that did not exist before.—He would have us believe that animals and plants became gradually more and more numerous; when most species appear in myriads of individuals, in the first bed in which they are found. He would have us believe that animals disappear gradually; when they are as common in the uppermost bed in which they occur as in the lowest, or any intermediate bed. Species appear suddenly and disappear suddenly in successive strata. That is the fact proclaimed by Palæontology; they neither increase successively in number, nor do they gradually dwindle down; none of the fossil remains thus far observed show signs of a gradual improvement or of a slow decay.—He would have us believe that

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geological deposits took place during the periods of subsidence; when it can be proved that the whole continent of North America is formed of beds which were deposited during a series of successive upheavals. I quote North America in preference to any other part of the world, because the evidence is so complete here that it can only be overlooked by those who may mistake subsidence for the general shrinkage of the earth's surface in consequence of the cooling of its mass. In this part of the globe, fossils are as common along the successive shores of the rising deposits of the Silurian system, as anywhere along our beaches; and and each of these successive shores extends from the Atlantic States to the foot of the Rocky Mountains. The evidence goes even further; each of these successive sets of beds of the Silurian system contains peculiar fossils, neither found in the beds above nor in the beds below, and between them there are no intermediate forms. And yet Darwin affirms that "the littoral and sub-littoral deposits are continually worn away, as soon as they are brought up by the slow and gradual rising of the land within the grinding action of the coast waves." Origin of Species, p. 290.—He would also have us believe that the most perfect organs of the body of animals are the product of gradual improvement, when eyes as perfect as those of the Trilobites are preserved with the remains of these oldest animals.—He would have us believe that it required millions of years to effect any one of these changes; when far more extraordinary transformations are daily going on, under our eyes, in the shortest periods of time, during the growth of animals.—He would have us believe that animals acquire their instincts gradually; when even those that never see their parents, perform at birth the same acts, in the same way, as their progenitors.—He would have us believe that the geographical distribution of animals is the result of accidental transfers; when most species are so narrowly confined within the limits of their natural range, that even slight changes in their external relations may cause their death. And all these, and many other calls upon our credulity, are coolly made in the face of an amount of precise information, readily accessible, which would overwhelm any one who does not place his opinions above the records of an age eminently characterized for its industry, and during which, that information was laboriously accumulated by crowds of faithful laborers.

It would be superfluous to discuss in detail the arguments by which Mr. Darwin attempts to explain the diversity among animals. Suffice it to say, that he has lost sight of the most striking of the features, and the one which pervades the whole, namely, that there runs throughout Nature unmistakable evidence of thought, corresponding to the mental operations of our own mind, and therefore intelligible to us as thinking beings, and unaccountable on any other basis than that they owe their existence to the working of intelligence; and no theory that overlooks this element can be true to nature.

There are naturalists who seem to look upon the idea of creation, that is, a manifestation of an intellectual power by material means, as a kind of bigotry; forgetting, no doubt, that whenever they carry out a thought of their own, they do something akin to creating, unless they look upon their own elucubrations as something in which their individuality is not concerned, but arising without an intervention of their mind, in conse-

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quence of the working of some "bundles of forces," about which they know nothing themselves. And yet such men are ready to admit that matter is omnipotent, and consider a disbelief in the omnipotence of matter as tantamout to imbecility; for, what is the assumed power of matter to produce all finite beings, but omnipotence? And what is the outcry raised against those who cannot admit it, but an insinuation that they are *non-compos?* The book of Mr. Darwin is free of all such uncharitable sentiments towards his fellow-laborers in the field of science; nevertheless his mistake lies in a similar assumption that the most complicated system of combined thoughts can be the result of accidental causes; for he ought to know, as every physicist will concede, that all the influences to which he would ascribe the origin of species are accidental in their very nature, and he must know, as every naturalist familiar with the modern progress of science does know, that the organized beings which live now, and have lived in former geological periods, constitute an organic whole, intelligibly and methodically combined in all its parts. As a zoölogist he must know in particular, that the animal kingdom is built upon four different plans of structure, and that the reproduction and growth of animals takes place according to four different modes of development, and that unless it is shown that these four plans of structure, and these four modes of development, are transmutable one into the other, no transmutation theory can account for the origin of species. The fallacy of Mr. Darwin's theory of the origin of species by means of natural selection, may be traced in the first few pages of his book, where he overlooks the difference between the voluntary and deliberate acts of selection applied methodically by man to the breeding of domesticated animals and the growing of cultivated plants, and the chance influences which may effect animals and plants in the state of nature. To call these influences "natural selection," is a misnomer which will not alter the conditions under which they may produce the desired results. Selection implies design; the powers to which Darwin refers the order of species, can design nothing. Selection is no doubt the essential principle on which the raising of breeds is founded, and the subject of breeds is presented in its true light by Mr. Darwin; but this process of raising breeds by the selection of favorable subjects, is in no way similar to that which regulates specific differences. Nothing is more remote from the truth than the attempted parallelism between the breeds of domesticated animals and the species of wild ones. Did there exist such a parallelism, as Darwin maintains, the difference among the domesticated breeds should be akin to the differences among wild species, and afford a clue to determine their relative degree of affinity by a comparison with the pedigrees of well-known domesticated races. Again, if there were any such parallelism, the distinctive characteristics of different breeds should be akin to the differences which exist between fossil species of earlier periods and those of the same genera now living. Now let any one familiar with the fossil species of the genera Bos and Canis, compare them with the races of our cattle and of our dogs, and he will find no correspondence whatever between them; for the simple reason that they do not owe their existence to the same causes. It must therefore be distinctly stated that Mr. Darwin has failed to establish a connection between the mode of raising domesticated

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breeds, and the cause or causes to which wild animals owe their specific differences.

It is true, Mr. Darwin states that the close affinity existing among animals can only be explained by a community of descent, and he goes so far as to represent these affinities as evidence of such a genealogical relationship; but I apprehend that the meaning of the words he uses has misled him into the belief that he had found the clue to phenomena which he does not even seem correctly to understand. There is nothing parallel between the relations of animals belonging to the same genus or the same family, and the relations between the progeny of common ancestors. In the one case we have the result of a physiological law regulating reproduction, and in other affinities which no observation has thus far shown to be in any way connected with reproduction. The most closely allied species of the same genus or the different species of closely allied genera, or the different genera of one and the same natural family, embrace representatives which at some period or other of their growth resemble one another more closely than the nearest blood relations; and yet we know that they are only stages of development of different species distinct from one another at every period of their life. The embryo of our common fresh water turtle, *Chrysemis picta*, and the embryo of our snapping turtle, *Chelydra serpentina*, resemble one another far more than the different species of Chrysemis in their adult state, and yet not a single fact can be adduced to show that any one egg of an animal has ever produced an individual of any species but its own. A young snake resembles a young turtle or a young bird much more than any two species of snakes resemble one another; and yet they go on reproducing their kinds, and nothing but their kinds. So that no degree of affinity, however close, can, in the present state of our science, be urged as exhibiting any evidence of community of descent, while the power that imparted all their peculiarities to the primitive eggs of all the species now living side by side, could also impart similar peculiarities with similar relations, and all degrees of relationship, to any number of other species that have existed. Until, therefore it can be shown that any one species has the ability to delegate such specified peculiarities and relations to any other species or set of species, it is not logical to assume that such a power is inherent in any animal, or that it constitutes part of its nature.* We must look to the original power that imparted life to the first being for the origin of all other beings, however mysterious and inaccessible

* The difficulty of ascertaining the natural limits of some species, and the mistakes made by naturalists when describing individual peculiarities as specific, has nothing to do with the question of the origin of species, and yet Darwin places great weight, in support of his theory, upon the differences which exist among naturalists in their views of species. Some of the metals are difficult to distinguish, and have frequently been mistaken, and the specific differences of some may be questioned; but what could that have to do with the question of the origin of metals, in the minds of those who may doubt the original difference of metals? Nothing more than the blunders of some naturalists in identifying species with the origin of species of animals and plants. The great mischief in our science now lies in the self-complacent confidence with which certain zoölogists look upon a few insignificant lines, called diagnoses, which they have the presumption to offer as characteristics of species, or, what is still worse, as checks upon others to secure to themselves a nominal priority. Such a treatment of scientific subjects is unworthy of our age.

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the modes by which all this diversity has been produced may remain for us. The production of a plausible explanation is no explanation at all, if it does not cover the whole ground.

All attempts to explain the origin of species may be brought under two categories: viz. 1st, some naturalists admitting that all organized beings are created, that is to say, endowed from the beginning of their existence with all their characteristics, while 2d, others assume that they arise spontaneously. This classification of the different theories of the origin of species, may appear objectionable to the supporters of the transmutation theory; but I can perceive no essential difference between their views and the old idea that animals may have arisen spontaneously. They differ only in the modes by which the spontaneous appearance is assumed to be effected; some believe that physical agents may so influence organized beings as to modify them—this is the view of DeMaillet and the Vestiges of Creation; others believe that the organized beings themselves change in consequence of their own acts, by changing their mode of life, etc., this is the view of Lamarck; others still assume that animals and plants tend necessarily to improve, in consequence of the struggle for life, in which the favored races are supposed to survive; this is the view lately propounded by Darwin. I believe these theories will, in the end, all share the fate of the theory of spontaneous generations so called, as the facts of nature shall be confronted more closely with the theoretical assumptions. The theories of DeMaillet, Oken, and Lamarck are already abandoned by those who have adopted the transmutation theory of Darwin; and unless Darwin and his followers succeed in showing that the struggle for life tends to something beyond favoring the existence of certain individuals over that of other individuals, they will soon find that they are following a shadow. The assertion of Darwin, which has crept into the title of his work, is, that favored *races* are preserved, while all his facts go only to substantiate the assertion, that favored *individuals* have a better chance in the struggle for life than others. But who has ever overlooked the fact that myriads of individuals of every species constantly die before coming to maturity? What ought to be shown, if the transmutation theory is to stand, is that these favored individuals diverge from their specific type, and neither Darwin nor any body else has furnished a single fact to show that they go on diverging. The criterion of a true theory consists in the facility with which it accounts for facts accumulated in the course of long-continued investigations and for which the existing theories afforded no explanation. It can certainly not be said that Darwin's theory will stand by that test. It would be easy to invent other theories that might account for the diversity of species quite as well, if not better than Darwin's preservation of favored races. The difficulty would only be to prove that they agree with the facts of Nature. It might be assumed, for instance, that any one primary being contained the possibilities of all those that have followed, in the same manner as the egg of any animal possesses all the elements of the full-grown individual; but this would only remove the difficulty one step further back. It would tell us nothing about the nature of the operation by which the change is introduced. Since the knowledge we now have, that similar

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metamorphoses go on in the eggs of all living beings has not yet put us on the track of the forces by which the changes they undergo are brought about, it is not likely that by mere guesses we shall arrive at any satisfactory explanation of the very origin of these beings themselves.

Whatever views are correct concerning the origin of species, one thing is certain, that as long as they exist they continue to produce generation after generation, individuals which differ from one another only in such peculiarities as relate to their individuality. The great defect in Darwin's treatment of the subject of species lies in the total absence of any statement respecting the features that constitute individuality. Surely, if individuals may vary within the limits assumed by Darwin, he was bound first to show that individuality does not consist of a sum of hereditary characteristics, combined with variable elements, not necessarily transmitted in their integrity, but only of variable elements. That the latter is not the case, stands recorded in every accurate monograph of all the types of the animal kingdom upon which minute embryological investigations have been made. It is known, that every individual egg undergoes a series of definite changes before it reaches its mature condition; that every germ formed in the egg passes through a series of metamorphoses before it assumes the structural features of the adult; that in this development the differences of sex may very early become distinct; and that all this is accomplished in a comparatively very short time, extremely short, indeed, in comparison to the immeasurable periods required by Darwin's theory to produce any change among species; and yet all this takes place without any deviation from the original type of the species, though under circumstances which would seem most untavorable to the maintenance of the type. Whatever minor differences may exist between the products of this succession of generations are all *individual peculiarities,* in no way connected with the essential features of the species, and therefore as transient as the individuals; while the specific characters are forever fixed. A single example will prove this. All the robins of North America now living have been for a short time in existence; not one of them was alive a century ago, when Linnæus for the first time made known that species under the name of Turdus migratorius, and not one of the specimens observed by Linnæus and his cotemporaries was alive when the Pilgrims of the Mayflower first set foot upon the Rock of Plymouth. Where was the species at these different periods, and where is it now? Certainly nowhere but in the individuals alive for the time being; but not in any single one of them, for that one must be either a male or a female, and not the species; not in a pair of them, for the species exhibits its peculiarities in its mode of breeding, in its nest, in its eggs, in its young, as much as in the appearance of the adult; not in all the individuals of any particular district, for the geographical distribution of a species over its whole area, forms also part of its specific characters.* A species is only known when its whole history

* We are so much accustomed to see animals reproducing themselves, generation after generation, that the fact no longer attracts our attention, and the mystery involved in it no longer excites our admiration. But there is certainly no more marvellous law in all nature than that which regulates this regular succession. And upon this law the maintenance of species depends; for observation teaches us that all that is not individual peculiarity is unceasingly and integrally reproduced while all that constitutes individuality, as such, constantly disappears.

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has been ascertained, and that history is recorded in the life of individuals through successive generations. The same kind of argument might be adduced from every existing species, and with still greater force by a reference to those species already known to the ancients.

Let it not be objected that the individuals of successive generations have presented marked differences among themselves; for these differences, with all the monstrosities that may have occurred, during these countless generations, have passed away with the individuals, as individual peculiarities, and the specific characteristics alone have been preserved, together with all that distinguishes the genus, the family, the order, the class, and the branch to which the individual belonged. Moreover all this has been maintained through a succession of repeated changes, amounting in each individual to the whole range of transformations, through which an individual passes, from the time it is individualized as an egg, to the time it is itself capable of reproducing its kind, and, perhaps, with all the intervening phases of an unequal production of males and females, of sterile individuals, of dwarfs, of giants, etc., etc., during which there were millions of chances for a deviation from the type. Does this not prove that while individuals are perishable, they transmit, generation after generation, all that is specific or generic, or, in one word, *typical* in them, to the exclusion of every *individual peculiarity* which passes away with them, and that, therefore, while individuals alone have a material existence, species, genera, families, orders, classes, and branches of the animal kingdom exist only as categories of thought in the Supreme Intelligence, but as such have as truly an independent existence and are as unvarying as thought itself after it has once been expressed.

Returning, after this digression, to the question of individuality among Acalephs, we meet here phenomena far more complicated than among higher animals. Individuality, as far as it depends upon material isolation, is complete and absolute in all the higher animals, and there maintained by genetic transmission, generation after generation. Individuality, in that sense, exists only in comparatively few of the Radiates. Among Acalephs it is ascertained only for the Ctenophoræ and some Discophoræ. In others, the individuals born from eggs end by dividing into a number of distinct individuals. In others still, the successive individuals derived from a primary one, remain connected to form compound communities. We must therefore, distinguish different kinds and different degrees of individuality, and may call *hereditary individuality* that kind of independent existence manifested in the successive evolutions of a single egg, producing a single individual, as is observed in all the higher animals. We may call *derivative* or *consecutive individuality* that kind of independence resulting from an individualization of parts of the product of a single egg. We have derivative individuals among the Nudibranchiate Mollusks, whose eggs produce singly, by a process of complete segmentation, several independent individuals. We observe a similar phenomenon among those Acalephs the young of which (Scyphostoma) ends in producing, by transverse division (Strobila), a number of independent free Medusæ (Ephyræ). We have it also among the Hydroids which produce free Medusæ. Next, we must distinguish *secondary individuality*, which is inherent to those individuals arising as

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buds from other individuals, and remaining connected with them.—This condition prevails in all the immovable Polyparia and Hydraria, and I say intentionally in the immovable ones; for, in the movable communities, such as Renilla, Pennatula, etc., among Polyps, and all the Siphonophoræ among Acalephs, we must still further distinguish another kind of individuality, which I know not how to call properly, unless the name of *complex individuality* may be applied to it. In complex individuality a new element is introduced, that is not noticeable in the former case. The individuals of the community are not only connected together, but, under given circumstances, they act together as if they were one individual, while at the same time each individual may perform acts of its own.

As to the specific differences observed among Acalephs, there is as great a diversity between them as between their individuals. In some types of this class the species are very uniform; all the individuals belonging to one and the same species resembling one another very closely, and exhibiting hardly any difference among themselves, except such as arises from age. This identity of the individuals of one and the same species is particularly striking among the Ctenophoræ. In this order there are not even sexual differences among the individuals, as they are all hermaphrodites. In the Discophoræ proper a somewhat greater diversity prevails. In the first place we notice male and female individuals, and the difference between the sexes is quite striking in some genera, as, for instance, in Aurelia. Next there occur frequent deviations, among them, in the normal number of their parts; their body consisting frequently of one or two spheromeres more than usual, sometimes, even, of double normal number, or of a few less. And yet, year after year, the same Discophoræ reappear upon our shores, with the same range of differences among their individuals. Among Hydroids polymorphism prevails to a greater or less extent, besides the differences arising from sex. Few species have only one kind of individuals. Mostly the cycle of individual differences embraces two distinct types of individuals, one recalling the peculiarities of common Hydræ, the other those of Medusæ; but even the Hydra type of one and the same species may exhibit more or less diversity, there being frequently two kinds of Hydræ united in one and the same community, and sometimes even a larger number of heterogeneous Hydræ. And this is equally true, though to a less extent, of the Medusa type. Yet among Siphonophoræ there are generally at least two kinds of Medusæ in one and the same community. But notwithstanding this polymorphism among the individuals of one and the same community, genetically connected together, each successive generation reproduces the same kinds of heterogeneous individuals, and nothing but individuals linked together in the same way. Surely we have here a much greater diversity of individuals, born one from the other, than is exhibited by the most diversified breeds of our domesticated animals; and yet all these heterogeneous individuals remain true to their species, in once case as in the other, and do not afford the slightest evidence of a transmutation of species.

Would the supporters of the fanciful theories lately propounded, only extend their studies a little beyond the range of domesticated animals, would they investigate the alternate generations of the Acalephs,

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the extraordinary modes of development of the Helminth, the reproduction of the Salpæ, etc., etc., they would soon learn that there are, in the world, far more astonishing phenomena, strictly circumscribed between the natural limits of unvarying species, than the slight differences produced by the intervention of men, among domesticated animals, and, perhaps, cease to be so confident as they seem to be, that these differences are trustworthy indications of the variability of species. For my own part I must emphatically declare that I do not know a single fact tending to show that species do vary in any way, while it is true that the individuals of one and the same species are more or less polymorphous. The circumstance, that naturalists may find it dificult to trace the natural limits of any one particular species, or the mistakes they may make in their attempts to distinguish them, has nothing whatsoever to do with the question of their origin.

There is another feature of the species of Acalephs which deserves particularly to be noticed. All these animals are periodical in their appearance, and last for a short period, in their perfect state of development. In our latitude most Medusæ make their appearance, as Ephyræ, early in the Spring, and rapidly enlarge to their full size. In September and October they lay their eggs and disappear; the young hatched from the eggs move about, as Planulæ, for a short time, and then become attached, as Scyphostomes, and pass the winter in undergoing their Strobila metamorphosis. The Ctenophoræ appear also very early, and lay their eggs in the autumn, passing the winter as young, and growing to their full size towards the beginning of the summer. Among the Hydroids there is more diversity in their periodicity. Hydraria are found all the year round; but the Medusæ buds, the free Medusæ, and the Medusaria make their appearance in different seasons, in different species. Some bring forth Medusæ buds and free Medusæ or Medusaria during winter; others, and in our latitude this is the case with by far the largest number of the Hydroids, produce their Medusæ brood in the spring; a few breed later, in the summer or in the autumn; so that, notwithstanding the regularity of their periodical return, Acalephs may be studied, in some condition or other, during the whole year.

When considering Individuality and Specific Differences, as manifested in the class of Acalephs, I have taken an opportunity of showing, upon general grounds how futile the arguments are upon which the theory of transmutation of species is founded. Having now shown that that class is circumscribed within definite limits, I may be permitted to add here a few more objections to that theory, based chiefly upon special grounds, connected with the characteristics of classes. If there is any thing striking in the features which distinguish classes, it is the definiteness of their structural peculiarities; and this definiteness goes on increasing, with new and additional qualifications, as we pass from the class characters to those which mark the orders, the families, the genera, and the species. Granting, for the sake of argument, that organized beings living at a later period may have originated by a gradual change of those of earlier periods, one of the most characteristic features of all organized beings remains totally unexplained by the various

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theories brought forward to explain that change; the definiteness of their respective groups, be they ever so comprehensive, or ever so limited, combined with the greatest inequality in their numeric relations. There exist a few thousand Mammalia and Reptiles, and at least three times their number of Birds and Fishes. There may be twenty thousand Mollusks; but there are over a hundred thousand Insects, and only a few thousand Radiates. And yet the limits of the class of Insects are as well defined as those of any other class, with the only exception of the class of Birds which is unquestionably the most definite in its natural boundaries. Now the supporters of the transmutation theory may shape their views in whatever way they please to suit the requirements of the theory, instead of building the theory upon the facts of Nature, they never can make it appear that the definiteness of the characters of the class of Birds is the result of a common descent of all Birds, for the first Bird must have been brother or cousin to some other animal that was not a Bird, since there are other animals besides Birds in this world, to no one of which any bird bears as close a relation as it bears to its own class. The same argument applies to every other class; and as to the facts, they are fatal to such an assumption, for Geology teaches us that among the oldest inhabitants of our globe known, there are representatives of nine distinct classes of animals, which by no possibility can be descendants of one another, since they are cotemporaries.

The same line of argument and the same class of facts forbid the assumption that either the representatives of one and the same order, or those of one of the same family, or those of one of the same genus should be considered as lineal descendants of a common stock; for orders, families and genera are based upon different categories of characters, and not upon more or less extensive characters of the same kind, as I have shown years ago (Vol. I, p. 150 to 163), and numbers of different kinds of representatives of these various groups, make their appearance simultaneously in all the successive geological periods. There appear together Corals and Echinoderms of different families and of different genera in each successive geological formation, and this is equally true for Bryozoa, Brachiopods and Lamellibranchiata, for Trilobites and the other Crustacea, in fact for the representatives of all the classes of the animal kingdom, making due allowance for the period of the first appearance of each; and at all times and in all classes the representatives of these different kinds of groups are found to present the same definiteness in their characteristics and limitation. Were the transmutation theory true, the geological record should exhibit an uninterrupted succession of types blending gradually into one another. The fact is that throughout all geological times each period is characterized by definite specific types, belonging to definite genera, and these to definite families, referable to definite orders, constituting definite classes and definite branches, built upon definite plans. Until the facts of Nature are shown to have been mistaken by those who have collected them, and that they have a different meaning from that now generally assigned to them, I shall therefore consider the transmutation theory as a scientific mistake, untrue in its facts, unscientific in its method, and mischievous in its tendency.

Cambridge, June 30, 1860.

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2. *Anleitung zur Organischen und Gasanalyse von* J. SCHIEL. Erlangen, 1860. 260 pp., 8vo.—This introduction to elementary organic and gas analysis, by our adopted countryman Dr. Schiel of St. Louis, contains a very full, yet concise, account of all of the approved methods, and is an excellent hand-book of these subjects. Many of the refinements of research which the text-books do not notice, and newly discovered processes or apparatus that are beginning to displace those hitherto in vogue, we find satisfactorily described in these pages. The best and simplest methods for taking the specific gravity of liquids, for determining melting and boiling points are given, and the admirable process of Simpson for the quantitative determination of nitrogen as well as that of Natanson for taking vapor densities are fully described.

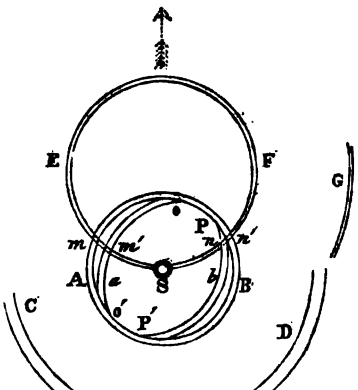
The chapters on Gas Analysis present in a clear manner the important parts of this subject. An instructive section of the book is occupied with an exposition of the mathematical principles which apply to the discussion of experimental results and the correction of errors of observation.

We believe that this treatise will be of great service in the hands of students, and by its careful selection and full description of the methods that unite simplicity and accuracy, will help to inspire them with the *animus* of scientific research.

VI. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *Parhelia seen at Weld, Franklin Co., Me.;* by STILLMAN MASTERMAN, (in a letter to the Editors).—*Messrs. Editors:*—I send you the following account of a *parhelion* seen by myself, at this place on the 21st instant, at $10^h\,45^m$ A. M.

A light haze was spread over the sky at the time, for the most part so thin as to be scarcely visible, but at a few points coming out in lenticular patches of well-marked *cirrus*. When first noticed at the time above stated, the meteor presented the appearance delineated in the accompanying drawing. The circle A B was about 45° in diameter, having the sun in its centre; and was very brilliant, having the colors of the primary rainbow, but with the red next to the sun. It was surrounded by a bright corona of white, three or four degrees in width: the width of the colored ring A B being equal to the apparent diameter of the solar orb. Two elliptical arcs *a* and *b* were included in A B, having their middles 2° distant from the inner edge of the latter and their extremities falling within it at *o o'* and P P'. In color and width *a* and *b* were precisely like A B and of a like brightness. Concentric with A B was the arc C D, being a little less than a semicircle, on a diameter of 95°, colored like A B with its red next to the



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sun, but of twice the width of the latter, and of dazzling brilliancy. The circle E F, with a diameter of about 60°, had its circumference in the sun and its centre vertically above him, [the arrow denoting the position of the vertical passing through the sun,] and consisted of a ring of pure white, quite brilliant, and having a width equal to two-thirds the apparent diameter of the sun. The arc G was concentric with E F, and bore the same proportion in radius to the latter that C D did to A B. It was white like E F, but not nearly so bright. The *parhelia* at *m m'* and *n n'* were equal to the united brilliancy of the rings intersecting at those points.

At $11^h\ 10^m$, all the rings had vanished but A B, which had contracted in diameter and width, and a small portion of C D immediately below the sun.

At $11^h\ 30^m$, A B had regained its former dimensions, with increased brilliancy. C D had entirely vanished.

At $12^h\ 0^m$, A B had contracted to 30° diameter, and become faint. All traces of the meteor had entirely vanished at $1^h\ 30^m$ P. M. The *cirrus* had in the mean time become gradually converted into *cirro-cumulus*, and the thin haze had completely vanished.

Weld, Franklin Co., Maine, May 28th, 1860.

2. *Tolles' improved Microscope Objectives.*—It has been generally supposed that the further improvement of object-glasses for the microscope was to be sought in the production of flint glass of high dispersive power. I am happy to inform your readers that Mr. R. B. Tolles of Canastota, N. Y., has boldly discarded the use of flint glass of unusual dispersive power and has discovered a method of constructing glasses of as large aperture as have ever yet been made by any artist, using only such glass of uniform density as is made for ordinary optical purposes.

I have recently received from Mr. Tolles a *one-third inch objective*, constructed on his plan, which has an aperture of 100° and is remarkable for the great amount of light transmitted even at the extreme borders of the lens. This objective is well corrected for chromatic and spherical aberration and gives excellent definition of blood corpuscles as well as of other tests appropriate for such an objective. This is a remarkably fine glass for ordinary investigations in minute anatomy and pathology. Mr. Tolles makes a one-fourth inch objective of 140° to 160° aperture, and a one-eighth inch of 160° to 175° aperture. I am inclined to think Mr. Tolles' glasses are unsurpassed by any object-glasses for the microscope that have yet been made in this country or elsewhere. M. C. White.

New Haven, June 10, 1860.

3. *Application of Photography in construction of Micrometers.*—The successful application of photography in the construction of micrometers, has been made by Mr. Clarence Morfit of the U. S. Assay Office, New York. It is merely the reduction of a large scale of exact dimensions and divisions to a definite size suitable for microscopic instruments. A scale of ten inches divided into inches and tenths of an inch has been reduced in this manner to one-twentieth of an inch, thus making its smallest divisions equal to one two-thousandth part of an inch square. The method is simple, accurate and economical. Moreover, the micrometer has the advantage of giving the exact measurement of the object in frac-

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tions of an inch and at the same time determines the power of the microscope itself.

4. *Geological Survey of California.*—The Act of the California legislature (approved April 24, 1860) appoints Prof. J. D. Whitney State Geologist with a salary of $6000 per annum. He appoints his own assistants with the approval of the Governor. Twenty thousand dollars are appropriated to the payment of expenses incurred in the survey. The provisions of the Act respecting the annual and final Reports are eminently judicious, especially that part which directs the Governor and Secretary of State to cause the volumes of the geological survey to be sold for the benefit of the Common School fund of the State.

No state geological survey was ever more auspiciously inaugurated, wisely provided for, or fraught with more interesting scientific and practical problems. It is understood that Prof. Whitney will commence his Californian labors in the coming autumn.

5. *Total Solar Eclipse of July* 18, 1860.—The American Nautical Almanac Commission has sent three observers to the Cumberland House, British America, to take note of this eclipse. This is a station of the Hudson's Bay Company on the West side of Pine Island Lake, in lat. 54° N., long. 102° 40′ W.

Another party of observers, consisting of Prof. Stephen Alexander, of Princeton, N. J., President F. A. P. Barnard, of Oxford, Mississippi, Prof. C. S. Venable, of North Carolina, Prof. A. W. Smith of the Naval Academy, Annapolis, Md., and Lieut. E. D. Ashe, R. N., of the Quebec Observatory, embarked June 28th at New York in the U. S. steamer Bibb, Lt. Commanding Alex. Murray, to proceed to Cape Chadleigh, Labrador.

6. *Newport Meeting of the American Association for the Advancement of Science.*—The next meeting of the Association will be held at Newport, R. I., commencing on Wednesday the first of August. The officers of the meeting are: President, Isaac Lea, Esq. of Philadelphia; Vice President, Dr. B. A. Gould of Cambridge; General Secretary, Prof. Joseph LeConte of Columbia, S. Ca.; Treasurer, Dr. A. L. Elwyn of Philadelphia.

It will be remembered that Prof. Henry, at the request of the Association, will deliver a discourse commemorative of the life and scientific labors of Dr. Robert Hare. Prof. Bache will also by appointment give an address on the Gulf Stream; while Prof. Leidy was requested to prepare a discourse on the extinct Reptilia and Mammalia of North America.

There are many reasons why this may be looked forward to as one of the most attractive and promising meetings of the Association ever held.

7. *Letter from John McCrady, Esq., Charleston, on the* Lingula pyramidata *described by Mr. W. Stimpson*, vol. xxix, p. 444.—Dear Stimpson: I believe you are connected with the Zoological department of Silliman's Journal during the absence of Prof. Dana. You have also, I see, redescribed there our Lingula, long known to naturalists but not before described. It was found more than ten years ago on our coast by the Rev. Thomas J. Young. A living specimen was also found by Mr. Burkhardt on Sullivan's Island when Prof. Agassiz had a laboratory there. I have never seen a specimen during all the time I have resided on Sullivan's Island. I am very glad you have distinguished and described it, since I suppose the Beaufort species can hardly be different from the South Carolinian.

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I have found quite lately something very interesting in connection with this *Lingula pyramidata*. You know that up to this time nothing has been known to science of the development of the Brachiopoda. I have within a few days discovered an embryo unlike anything known, and which has at once so many affinities with the Bryozoa and the Brachiopoda, that I believe it to be an embryo Brachiopod, and very probably the young of *Lingula pyramidata*. Imagine an equilateral, thin, hyaline, straight-hinged bivalve shell, elliptical in outline, and with valves very flat. Through this perfectly transparent shell is visible a lining membrane or rather the borders of such a membrane, which is the mantle. Within this and near the hinge, a large flask-shaped body containing a digestive cavity surrounded by a dark mass. This cavity extends into the neck of the bottle-shaped cavity (œsophagus) and terminates towards the gap of the shell in a mouth. From the opposite or basal end of the digestive cavity goes off a pretty long intestine which turns first to the left, makes several convolutions and terminates in an anus on the right side between the two valves of the shell. The mouth lies on a somewhat triangular prolongation of the body-wall which rests with its apex towards the gap of the shell in the dorsal valve. The borders right and left of this homologue of the arms are fringed each about six cirrhi, the hindermost being the longest.

The animal thus constituted, when quiet withdraws its whole body, cirrhi and all, within its bivalve shell, which is tightly closed; but when in motion the shell is distinctly opened and the gap of the valves is plainly visible even to the naked eye. Through their aperture are thrust out the cirrhi, about twelve in number, which then arrange themselves in circular funnel-like manner precisely as in a Bryozoan polyp, and by a motion plainly ciliary, and with its cirrhi or tentacula thus extended the embryo swims through the water with considerable rapidity.

The cirrhi of this embryo I take to be the homologues of the cirrhi of Brachiopoda, and of Cristatella, and of the tentacula of Bryozoa generally. The rest of the structure, especially the anus on the right and the shape of the shell, point, I think, to Lingula embryo with cirrhi extended about a line in length. There is no trace of a peduncle. It appears to me that this must set at rest all difficulty about the approximation of Bryozoa and Brachiopoda, as proposed by Agassiz and others. A somewhat fuller description of this young animal, with a figure, will be published in the forthcoming issues of the Elliott Society of Charleston.

Charleston, S. C., June 18th, 1860.

8. *The Fusion and Casting of Platinum* on a considerable scale has been accomplished by Messrs. Deville and Debray. At the sitting of the *Académie des Sciences* at Paris, June 4, they exhibited (1.) Two ingots of platinum weighing together 25 kilograms, fused in the same fire and cast in an ingot mould of cast iron. The surface of the metal shows evidence of perfect fluidity and carries the impression of characters engraved on the surface of the mould.

(2.) A toothed wheel, of platinum cast in ordinary founders' sand was also shown. This was cast in the mode common for cast iron in a two-part flask with a sprue and vent holes as usual.

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The metal used they obtained by the dry mode from the crude platinum and platinum-money which the Russian Government had placed at their disposal; the details of the process being reserved for a second communication to the Academy.—*Comptes Rendus*, June 14, 1, 138.

9. *New Arctic Expedition by* Dr. I. I. Hayes.—Dr. Hayes, well known as the companion of Kane and the author of "An Arctic Boat Voyage," leaves Boston in a small vessel with seventeen companions for Smith's Sound, early in July, designing to test the views respecting the open sea which he has already expressed in detail in this Journal in an article entitled "Observations on the practicability of reaching the North Pole."* This new expedition of Dr. Hayes is equipped by private liberality, and goes out under the auspices of the American Geographical Society.

In this connexion we may mention the proposal of Mr. Parker Snow of London, well known for his Arctic researches, to set on foot a new exploration to search for further traces of the Franklin Expedition. Mr. Snow has prepared a paper setting forth his plans and motives in detail, a copy of which we have received and placed before the American Geographical Society at a late sitting, by whom it has been officially communicated to Dr. Hayes, just on the eve of his departure.

10. *Constitution and By-Laws of and List of Officers and Members of the Chicago Academy of Sciences.* Chicago, Illinois, 1859.—This flourishing young association of naturalists was organized in 1857, and incorporated March, 1859. Its establishment affords another gratifying proof of the rapid progress of science at the West.

11. *Personal.*—Prof. A. D. Bache, Superintendent of the U. S. Coast Survey, was recently chosen a Foreign Member of the Royal Society of London. Of the fifty foreign members, three are now Americans, viz., Professors Peirce, Agassiz, and Bache.

Prof. Dana has been in Switzerland since early in May, and will remain among the mountains until his return, toward the close of summer. His health is slowly but steadily improving.

Prof. Benjamin Peirce, of Cambridge, has sought relief from his too severe labors in a voyage to Europe by packet. His numerous friends will be pained to learn that his health was much impaired.

Prof. J. P. Cooke, of Cambridge, lately read before the Chemical Society of London a paper on the compounds of antimony and zinc ($SbZn^2$ $SbZn^3$), designed to show that crystalline form is not a necessary indication of definite chemical composition. A copy of this paper from the Author reached us at too late a date for publication in the present number of this Journal.

The gold medals of the London Geographical Society have been awarded (May 28), at the suggestion of Sir R. I. Murchison, to Lady Franklin and Sir Leopold McClintock;—to the first in consideration of the deeds of her husband and in recognition of her noble-minded devotion; and to the second for his discovery of the fate of the Erebus and Terror and the accompanying additions to Arctic geography.

Lady Franklin is announced as expected soon to arrive in the United States as the guest of Henry Grinnell, Esq., Vice-President of the American Geographical Society.

* Vol. xxvi, No. 78, 305, Nov. 1858.

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12. *To our Correspondents* we owe an apology for the unavoidable postponement to a succeeding Number of several valuable papers, crowded out by the unexpected length of the Articles in the present issue. Even the addition of a full sheet has not sufficed to embrace our customary lists of new books, or a considerable mass of scientific intelligence, proceedings of societies, bibliography, &c., already in type.

13. *Obituary.*—The Rev. BADEN POWELL, Professor of Geometry in the University of Oxford and equally distinguished as a mathematician and physicist, died there in June.

CHARLES GOODYEAR, widely known as the discoverer of "*vulcanization*" of Caoutchouc died in New York, July 1st, aged fifty-nine years.

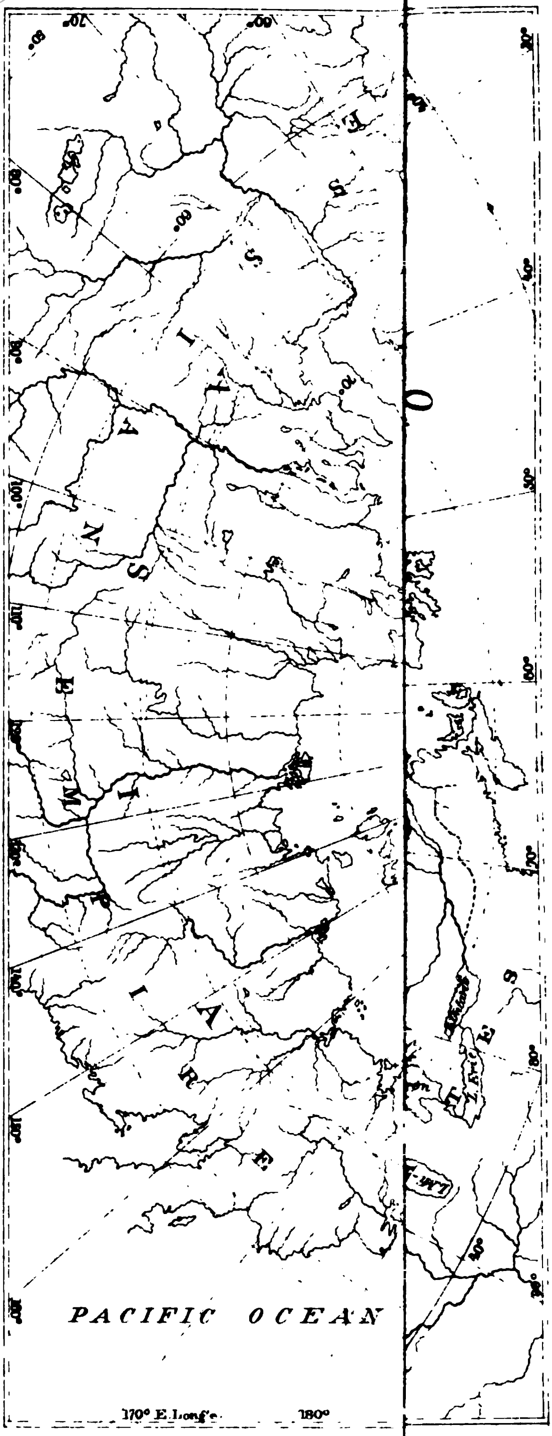
CANADIAN NATURALIST AND GEOLOGIST, Dec., 1859. Vol. IV.—p. 411, Notes on land and sea birds observed around Quebec; *J. M. LeMoine.*—p. 414, Chemical Geology; *Hunt.*—p. 426, Fossils of the Chazy limestone, with new species; *Billings.*—p. 494, A list of the Coleoptera found in the vicinity of Montreal; *D'Urban.*—Feb. 1860, Vol. V.—p. 1, Devonian plants of Canada; *Dawson.*—p. 14, List of plants found near Prescott, C. W.; *Billings.*—p. 24, Tubicolous Marine Worms of the Gulf of St. Lawrence; *Dawson.*—p. 30, Marine Algæ; *Kemp.*—p. 69, Palæozoic fossils; *Billings.* April.—p. 81, Natural History of the Valley of the River Rouge; *D'Urban.*—p. 100, Review of Darwin; *Dawson.*—p. 120, Sketch of the life of Mr. David Douglas.—p. 132, On the Silurian and Devonian rocks of Nova Scotia; *Dawson.*—p. 144, New fossils from the Silurian of Nova Scotia.

PROCEEDINGS PHILADELPHIA ACAD. NAT. SCI., 1860.—p. 81, Notice of the death of Dr. Hallowell.—p. 82, Habits of an Ocelot kept on ship-board; *Wilson.*—p. 85, Corrected numbers of Unionidæ; *Lea.*—On Hyalonema mirabilis from Japan; *Leidy.*—p. 86, Donation of 28,000 specimens of birds, by Dr. T. B. Wilson.—p. 88, On the coloring matter of the Nacre in Unionæ; *Lea.*—p. 89, New South American Unionidæ; *Lea.*—p. 92, New Unionæ and Melaniæ; *Lea.*—p. 93, New Cretaceous fossils from New Jersey; *Gabb.*—p. 96, Experiments in chemical geology; *Lesley.*—On Trichina spiralis; *Leidy.*—p. 97, On the palpi of S. American Anodons; *Lea.*—On a gneiss boulder in Orange Co., N. Y.; *Lesley.*—p. 98, New botanical locality; *Leidy.*—On the Albertite of New Brunswick; *Rogers.*—Experiments in Binocular Vision; *Rogers.*—p. 100, Conspectus piscium, etc., the Sicydianæ collected on the North Pacific Expedition; *Theo. Gill.*—p. 102, Monograph of the genus Labrosomus, Sw.; *Gill.*—p. 108, Monograph of the genus Labrax, Cuv.; *Gill.* (Mr. Gill finds our American species generically distinct from the European type. Our striped basse he places in the genus *Roccus*, and the white perch in *Morone*—names adopted from Mitchell.)—p. 120, Monograph of the Philypni; *Gill.*—p. 126, Notice of geological discoveries made by Capt. J. H. Simpson; *Meek* and *Engelmann.*—p. 132, Catalogue of Birds collected on the Isthmus of Darien by the expedition under Lt. Michler, with notes and descriptions of new species; *Cassin.*

JOURNAL OF THE ACADEMY OF NATURAL SCIENCES OF PHILADELPHIA, New Series, Vol. IV, Pt. III, March, 1860.—Descriptions of Edotic Unionidæ, 13 plates; *I. Lea.*—New Cretaceous and Eocene fossils from Mississippi and Alabama (2 plates); *T. A. Conrad.*—New Cretaceous and Triassic fossils (a plate); *Gabb.*—Reflections upon the nature of the temporary star of the year 1572,—an application of the nebular hypothesis; *Wilcocks.* W. S.

PROCEEDINGS BOSTON SOC. NAT. HIST., 1860.—p. 226, On two Birds from Bogota—*Turdus minimus* (Lafresnaye) and *Vireo Bogotensis* (Bryant); *Dr. Henry Bryant.*—p. 227, Fossiliferous slate and sandstone (Devonian) from the Dennis river, Maine; *Prof. W. B. Rogers.*—p. 228, Spines of Siluroid fishes from a whale's blubber; *Capt. Atwood.*—p. 229, "Cocoa-nut pearl;" *Dr. C. F. Winslow.*—p. 229, Discussion on the subject of Vision; *Dr. J. B. Jeffries, Prof. W. B. Rogers,* and *Dr. Gould*—p. 231, Discussion on the theory of Darwin; *Agassiz, Emerson,* and *Prof. W. B. Rogers.*—p. 235, Description of Hoplocampa rubi, by the late *Dr. T. W. Harris;* with remarks on its history by *Noyes Darling, Esq.; Mr. Scudder.*—p. 236, Geological map of Vermont; *Mr. C. H. Hitchcock,* with remarks by *Prof. W. B. Rogers.*—p. 239, Section of an elephant's tooth as an example of *Osteo-dentine; Dr. White.*—p. 240, Laws of fracture of a thick glass tube; *Prof. W. B. Rogers.*

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PACIFIC OCEAN
170° E. Long.
180°

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THE

AMERICAN

JOURNAL OF SCIENCE AND ARTS.

[SECOND SERIES.]

ART. XV.—*On the Nebular Hypothesis;* by Professor DANIEL KIRKWOOD, Bloomington, Indiana.

THE records of our planet's physical history, from the dawn of organized existence down to the epoch of man's creation, have, for the most part, been brought to light since the commencement of the nineteenth century. Within this brief period the immense antiquity of our globe, the former high temperature of its surface, and, in short, all the main facts and doctrines of geology, have been discovered or confirmed by a laborious examination of the planetary crust. *But the earth has also a pre-geologic history*—a history as yet undeveloped; the grand outlines of which must be derived chiefly from *celestial* phenomena. "The testimony of the rocks" in regard to the one, may be more explicit than that of the *stars* in regard to the other. It must be borne in mind, however, that this department of research—the tracing of astronomical facts not accounted for by gravitation, to their source in the origin of the system—has hitherto received but little attention from men of science. Cogent arguments, it is true, were adduced by Laplace and Pontécoulant in favor of the nebular hypothesis, but very little has since been accomplished, tending either to invalidate or confirm it.

The present article is designed as a popular rather than a scientific discussion of this interesting subject, and we trust its interest will not be abated by the fact that a portion of the matter has been before presented by us anonymously in a Quarterly Review. We shall in the first place present a brief account of the origin and nature of Laplace's theory; secondly, a connected

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view of the principal phenomena by which it is sustained; and thirdly, consider the most prominent of the objections which have been urged against it.

As a group, our solar system is comparatively isolated in space; the distance of the nearest fixed star being at least seven thousand times that of Neptune, the most remote known planet. Besides the central or controlling orb, it contains, so far as known at present, sixty-seven primary planets, twenty-one satellites, three planetary rings, and nearly eight hundred comets. In taking the most cursory view of this system of bodies, we cannot fail to notice the following interesting facts in regard to the motions of its various members:—

1. The sun rotates on his axis from west to east.

2. The primary planets all move nearly in the plane of the sun's equator.

3. The orbital motions of all the planets, primary and secondary, except the satellites of Uranus and perhaps those of Neptune, are in the same *direction* with the sun's rotation.

4. The direction of the rotary motions of all the planets, primary and secondary, in so far as has been observed, is identical with that of their orbital revolutions; viz., from west to east.

5. The rings of Saturn revolve about the planet in the same direction.

6. The planetary orbits are all nearly circular.

7. The *cometary*, is distinguished from the *planetary* portion of the system by several striking characteristics: the orbits of comets are very eccentric and inclined to each other, and to the ecliptic at all possible angles. The motions of a large proportion of comets are *from east to west*. The physical constitution of the latter class of bodies is also very different from that of the former; the matter of which comets are composed being so exceedingly attenuated, at least in many instances, that fixed stars have been distinctly visible through what appeared to be the densest portion of their substance.

None of these facts are accounted for by the law of gravitation. The sun's attraction can have no influence whatever in determining either the *direction* of a planet's motion, or the eccentricity of its orbit. In other words, this power would sustain a planetary body moving from east to west, as well as from west to east; in an orbit having any possible degree of inclination to the plane of the sun's equator, no less than in one coincident with it; or, in a very eccentric ellipse, as well as in one differing but little from a circle. The consideration of the coincidences which we have enumerated led LaPlace to conclude that their explanation must be referred to the *mode* of our system's formation—a conclusion which he regarded as strongly confirmed by the contemporary researches of Sir William Herschel. Of the numer-

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ous nebulæ discovered and described by that eminent observer, a large proportion could not, even by his powerful telescope, be resolved into stars. In regard to many of these, it was not doubted that glasses of superior power would show them to be extremely remote sidereal clusters. On the other hand, a considerable number were examined which gave no indications of resolvability. These were supposed to be a species of self-luminous, nebulous matter—the chaotic elements of future stars. The great number of these irresolvable nebulæ, scattered over the heavens and apparently indicating the various stages of central condensation, very naturally suggested the idea that the solar system, and perhaps every other system in the universe, originally existed in a similar state. The sun was supposed by Laplace to have been an exceedingly diffused, rotating nebula, of spherical or spheroidal form, extending beyond the orbit of the most distant planet; the planets as yet having no separate existence. This immense sphere of vapor, in consequence of the radiation of heat and the continued action of gravity, became gradually more dense, which condensation was necessarily attended by an increased angular velocity of rotation. At length a point was thus reached where the centrifugal force of the equatorial parts was equal to the central attraction. The condensation of the interior meanwhile continuing, this zone was detached, but necessarily continued to revolve around the central mass with the same velocity that it had at the epoch of its separation. If perfectly uniform throughout its entire circumference, which would be highly improbable, it would continue its motion in an unbroken ring, like that of Saturn; if not, it would probably collect into several masses, having orbits nearly identical. "These masses should assume a spheroidal form, with a rotary motion in the direction of that of their revolution, because their inferior particles have a less real velocity than the superior; they have therefore constituted so many planets in a state of vapor. But if one of them was sufficiently powerful, to unite successively by its attraction, all the others about its centre, the ring of vapors would be changed into one spheroidal mass, circulating about the sun, with a motion of rotation in the same direction with that of revolution."* Such, according to the theory of Laplace, is the history of the formation of the most remote planet of our system. That of every other, both primary and secondary, would be precisely similar.

If it be said that the small eccentricities of the planetary orbits, the approximate coincidence of their planes with that of the solar equator, and the uniformity of direction in which the planets move, are *ultimate facts* that the *final cause* of these arrange-

* Harte's Translation of Laplace's System of the World, vol. ii, note vii.

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ments was the stability of the system; and that we transgress the legitimate domain of scientific research in *attempting* their explanation;—we ask by what rule in philosophy the arrangements in question are determined to be ultimate facts. Even granting their final cause to have been the stability of the system, we are by no means to conclude that they are necessarily unsusceptible of explanation. "Final causes," says Whewell,* "are to be excluded *from physical inquiry*; that is, we are not to assume that we know the object of the Creator's design, and put this assumed purpose in the place of a physical cause. We are not to think it a sufficient account of the clouds that they are for watering the earth—to take Bacon's examples—or 'that the solidness of the earth is for the station and mansion of living creatures.' The physical philosopher has it for his business to trace clouds to the laws of evaporation and condensation; to determine the magnitude and mode of action of the forces of cohesion and crystallization by which the materials of the earth are made solid and firm. This he does, making no use of the notion of final causes; and it is precisely because he has thus established his theories independently of any assumption of an end, that the end, when, after all, it returns upon him, and cannot be evaded, becomes an irresistible evidence of an Intelligent Legislator. He finds that the effects, of which the use is obvious, are produced by most simple and comprehensive laws; and when he has obtained this view, he is struck by the beauty of the means, by the refined and skillful manner in which the useful effects are brought about;—points different from those to which his researches were directed."

As the question, then, to which the cosmogony of Laplace proposes a solution, is a legitimate one, we shall proceed to consider some of the evidences by which the theory is supported.

1. *The nebular hypothesis furnishes a very simple explanation of the motions and arrangements of the planetary system.* In the first place, it is evident that the separation of a ring would take place at the equator of the revolving mass, where of course the centrifugal force would be greatest. These concentric rings—and consequently the resulting planets—would all revolve *in nearly the same plane.* It is evident also that the central orb must have a revolution on its axis *in the same direction with the progressive motions of the planets.* Again: at the breaking up of a ring, the particles of nebulous matter more distant from the sun would have a greater absolute velocity than those nearer to it, which would produce the observed *unity of direction in the rotary and orbital revolutions.* The motions of the satellites are explained in like manner. The hypothesis, moreover, accounts satisfactorily for the fact that the orbits of the planets are all nearly circular.

* Bridgewater Treatise, vol. ii, p. 180.

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And finally, it presents an obvious explanation of the rings of Saturn. It would almost seem, indeed, as if these wonderful annuli had been left by the Architect of Nature, as an index to the creative process.

The argument derived from the motions of the various members of the solar system is not new, having been forcibly stated by Laplace, Pontécoulant, Nichol, and other astronomers. Its full weight and importance, however, have not, we think, been duly appreciated. That a common physical cause has determined these motions, must be admitted by every philosophic mind. But apart from the nebular hypothesis, no such cause, adequate both in mode and measure, has ever been suggested;—indeed none, it seems to us, is conceivable. The phenomena which we have enumerated *demand* an explanation, and this demand is met by the nebular hypothesis. It will be found, therefore, when closely examined, that the evidence afforded by the celestial motions is sufficient to give the theory of Laplace a very high degree of probability.

2. The fact that this theory of the genesis of the solar system may be extended to the binary and multiple systems among the so-called fixed stars, may be urged as no inconsiderable evidence in its favor. Unity, no less than variety, is characteristic of nature's works. All the diversified and apparently disconnected phenomena of the universe have their roots in a few general laws. Whatever, therefore, leads us higher in the process of generalization may be presumed to have some foundation in truth.

3. Numerous geological facts appear to harmonize with the hypothesis under consideration. Fossil organic remains, and their absence in the earlier rocks, both indicate that the temperature of the earth's surface was formerly much higher than at present, and that the decrease was slow and gradual. In places, for instance, where ferns do not now exceed three feet in height, those of former periods are found to have been from forty to fifty, or even seventy feet high. Now in regard to existing plants, it is well-known that their *number*, as well as their *size and luxuriance of growth*, gradually increase as we advance from high latitudes towards the equator. In both these respects a similar increase is observable in descending through the successive strata of the ancient world, until we reach the oldest Secondary rocks. These facts are doubtless to be referred to the same cause, namely, a gradual change of temperature. A comparison of fossil animals with those now existing, leads to a similar result. This high temperature of the earth's surface during the earlier stages of its physical history, is attributed by most geologists to a central heat,* which diffused itself through-

* A different theory in regard to the ancient high temperature of the earth has been developed by the celebrated Poisson. Starting with the fact—established be-

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out the entire mass. That the interior of our planet is in a state of igneous fluidity, or has, at least, an extremely high temperature, is now very generally admitted. As we descend from the surface we find a regular increase of heat, varying indeed for different localities, partly no doubt, on account of the different conducting powers of the rocks which constitute the crust, but averaging about one degree for every fifty feet.* This has been ascertained from a great variety of experiments upon rocks and springs in mines, and upon the water issuing from Artesian wells. It is true, the depth to which man has been able to penetrate, is comparatively small; but assuming the same rate of increase to continue, it has been calculated that the point at which all known rocks would be in a state of fusion must be considerably less than one hundred miles from the surface, and possibly less than fifty. Reasons are not wanting for regarding this molten mass as the seat of present volcanic activity,† as well as the source of that great upheaving power by which not only mountains, but islands and continents have been elevated.

Geologists are generally agreed that there is conclusive evidence of the primitive igneous fluidity of the unstratified rocks of the earth's crust. Indeed it can scarcely be doubted that the solidification of the outer portions of our globe was a mere cooling by radiation from the surface. "There is no small reason," says Professor Hitchcock, "to suppose that the globe underwent numerous changes previous to the time when animals were placed upon it; that, in fact, the time was when the whole matter of

yond doubt by modern observations—that the solar system has a progressive motion in space, he supposes that in the sweep of its mighty orbit it has passed through regions of very different temperatures, and that the heat of former periods—the residuum of which is still found at great depths beneath the surface—was received *ab extra* while in a portion of space much richer in stars, and having therefore a higher temperature, than that through which it is at present passing. "The physical doubts which have reasonably been entertained against this extraordinary cosmical view, (which attributes to the regions of space that which probably is more dependent on the first transition of matter condensing from the gaseo-fluid into the solid state,) will be found collected in Poggendorff's *Annalen*, bd. xxxix, S. 93—100." Humboldt's Cosmos, vol. i. p. 165.

* See Proceedings of the American Association for the Advancement of Science, Tenth Meeting, Part II. p. 102.

† "The similarity of lava, wherever found, and the close agreement as to composition and physical characters of the basalt of ancient epochs and of that still bursting through and intersecting the walls of modern volcanoes, are proofs that all such eruptions have a common origin, and are due, as well as the accompanying physical phenomena of earthquakes, to forces acting on the still liquid portion of the earth." *Portlock's Rudimentary Geology*, p. 70.

The researches of Hopkins and Hennessy have led to different results in regard to the thickness of the earth's crust. See Trans. of the Roy. Soc. for 1839, p. 381; for 1840, p. 193; for 1842, p. 43; for 1851, Part II, p. 495. Also, the Journal for March, 1852, p. 271, and for May, 1853, p. 126. For "some suggestions in Explanation of the primitive Incandescent condition of the Earth and the other Planets," see Monthly Notices of the Roy. Astro. Soc. for January 13th, 1854.

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the earth was in a melted and not improbably also in a gaseous state."* All this, it will be perceived, is in striking accordance with the Nebular Hypothesis.

4. Whatever may be the nature of the elevating force, it is not confined to our globe. The lunar and planetary mountains afford evidence of its action in the other members of our system—a fact which seems indicative of their common origin.

5. The spheroidal figure of the planets points to a great and significant fact in regard to their primitive constitution—the fact that they have *all*, at former epochs in their history, been in a liquid, if not in a gaseous state. That the polar flattening of the earth is not conclusive evidence of its primitive fluidity, has been affirmed, we are aware, by more than one writer of scientific eminence. If we suppose our planet to have been originally solid and perfectly spherical—its surface being entirely covered with water—the effect of its axial revolution would be the accumulation of water in the equatorial region, and a consequent recession from the poles. Sir John Herschel remarks,† that the gradual abrasion of these polar continents and their slow deposition in the deep equatorial ocean would eventually reduce the solid earth itself to the form of an oblate spheroid. Recently, however, H. Hennessy, an eminent mathematician, has subjected this hypothesis to the best of scientific scrutiny. The learned and original researches of this gentleman have shown that the ultimate ellipticity, in case the earth were at first a *solid* sphere, as supposed by Herschel, would be $\frac{1}{231}$, while that found by actual measurement is $\frac{1}{299}$ to $\frac{1}{304}$.‡ The theory of primitive fluidity may therefore be regarded as fully established. It is worthy of remark that the oblateness of Mars is much greater than would have resulted from its observed velocity of rotation, supposing the planet to have been originally fluid, homogeneous, and of spherical form. How is this anomaly to be accounted for? On the nebular hypothesis we have only to suppose that in the process of transition from the gaseous to the liquid and solid form, "the liquid surface of some planets was solidified before they could assume the figure appertaining to their velocity of rotation."§

6. The Nebular Hypothesis affords the most probable explanation of the phenomena of comets. Laplace supposed these bodies to have had their origin in portions of nebulous matter which had been left about the points of equal attraction between the sun and neighboring stars—the occasional preponderance of the solar influence causing portions of these outstanding nebulosities to enter our system from different quarters of the heavens. The orbits of such bodies would be very eccentric, and might have

* Religion of Geology, p. 22. † Outlines, Arts 226, 227. ‡ Mädler, p. 50, this vol.
§ Humboldt's Cosmos, vol. iv, (Bohn's Edition,) p. 427.

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any degree of inclination to the plane of the ecliptic. Their motion, moreover, might be either direct or retrograde. Thus the great characteristics which distinguish the members of the cometary from those of the planetary system are necessary consequences of this theory of their origin.

"The attraction of the planets," says Laplace, "and perhaps also the resistance of the ethereal medium, ought to change several cometary orbits into ellipses, of which the greater axes are much less than the radius of the solar activity. It is probable that such a change was produced on the orbit of the comet of 1759, the greater axis of which was not more than thirty-five times the distance of the sun from the earth. A still greater change was produced in the orbits of the comets of 1770, and of 1805."*

7. The evidence afforded by the telescope in regard to the present physical constitution of the different members of the solar system appears to be confirmatory of the nebular hypothesis. If the sun and planets have condensed from a gaseous state, the former, not only on account of the comparative recentness of its formation, but especially because of its great relative magnitude would retain an intensely high temperature for an indefinite period after the planets had cooled down. *The present heat, therefore, of the central orb, and the existence of a very extensive gaseous envelope, together with the various phenomena of the solar spots, would result as consequences from the theory of Laplace.*

The Moon.—Although the diameter of the earth is nearly four times that of her satellite, the mountain elevations of the latter are nearly equal to those of the former. The cup-shaped cavities, which cover a great part of the lunar surface, are very different in appearance from volcanoes on the earth. The largest are from fifty to one hundred and fifty miles in diameter, and some are from three to four miles deep. In no case, however, is there any decisive evidence of *present* volcanic activity, though doubtless the phenomena justify the inference that at some remote epoch in the past history of our satellite, its crust has been agitated and shattered by up-heaving eruptive forces, compared with which the similar agencies in our own planet, at least those in operation during the historical period, sink into insignificance.

In contemplating these striking characteristics of the moon's surface—the traces of ancient igneous activity over the whole visible hemisphere, and the apparent absence of organization—it is natural to inquire—what is their physical import? If our satellite, in accordance with the nebular hypothesis, has solidified from a gaseous state, the earth having also undergone the same process, the latter, on account of its superior magnitude, would require a much longer period than the former to cool down from

* System of the World, Harte's Translation, vol. ii, p. 364.

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a condition of igneous fluidity. The moon may therefore be farther advanced in its physical history than the earth. On this subject Mr. Nasmyth, of Manchester, England, who has devoted much time and attention to observations on the lunar surface, remarks as follows:—"Having in my travels, seen the actual results of volcanic action, extinct and active, I think I can comprehend what I observe on the moon, and trace the analogy where it is applicable. * * * I may express one of my most definite and strong convictions in a few words, namely, *that I do not believe there is one of the countless thousands of volcanoes, whose craters bespatter the lunar surface, in action, or which has been in action, for thousands of ages past.* I am vain enough to think I have got the right view of the true nature of volcanic action; a view which close observation of the phenomenon, in all its phases, has impressed on me; namely, *that volcanic action is an expiring phenomenon*, having for its cause and source great cosmical principles, quite independent from any mere chemical action; and, in that view, I consider molten lava, and the heat of volcanic action, to be nothing less than the residue of that *igneous* state through which *all* the planets have passed, in their cosmical history, from the earliest moment of their creation to the present time. And, in this view, if our globe be permitted to exist, in its present condition, for ages to come, volcanic action, as an active phenomenon will dwindle away, and finally cease to exist,—the solid crust of the earth so increasing in thickness as to prevent the issue of any of the yet remaining molten matter from its interior.

"The moon, from its small mass, and proportionally great surface, must have cooled down much more rapidly than the earth; and all have been dead, tranquil and silent, for countless ages, ere we had passed over our rampant volcanic era, of which our most tremendous modern volcanoes are but mole-hills in comparison."

Jupiter and Saturn.—What indications do the belts of Jupiter and Saturn afford in regard to the present physical condition of these planets? When our own planet was in a molten state, where was the water which now constitutes our oceans? It must necessarily have existed in the form of vapor, entirely surrounding the earth, and at a considerable distance from it. Nor could this water descend to, and remain permanently upon the surface, until long after the solidification of the crust by cooling. During this "pre-oceanic" period, our globe, it is likely, would have appeared to a distant spectator, very much as Jupiter and Saturn with their cloudy belts now appear to us. Now it has been justly remarked, that "the length of time which would be required for such enormous planets as Jupiter and Saturn to cool down from their original molten and incandescent condition to such a

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temperature as would be fitted to permit their oceanic matter permanently to descend and remain upon the surface, *would be vastly longer than in the case of such a comparatively small planet as the earth.*"

Thus, in accordance with the nebular hypothesis, it would seem that Jupiter and Saturn, on account of their vast dimensions, still retain so much of their primitive heat as to produce the vaporous envelopes by which they are surrounded.

Variable Stars.—The well-known phenomena of variable stars, the explanation of which has greatly perplexed astronomers, is ingeniously accounted for, according to the nebular hypothesis, by Mr. A. D. Wackerbarth, as follows: "Suppose a nebula such as that from which the earth, sun, and planets, are supposed to have arisen, existing in space, such a nebula would probably be composed of elements more or less the same as those whereof our own planet is formed.* Some, indeed, of the latter we might suppose wanting, and others present which we possess not here; but on the whole, let us suppose that the chief elements of the earth are found in our nebula, which would thus form an immense spheroid of nebulous matter revolving round its own minor axis; or rather, if that matter were not quite homogeneously distributed, on an axis passing through its centre of gravity. We may suppose or not as we please, that this nebula has a nucleus, as many nebula appear to have, and many not to have, any such portion; but in the former case we must suppose some little difference in the constitution of the particles towards the centre, or position of the said nucleus. Our nebula, thus composed, may wander a longer or shorter period in space peacefully; but now let us suppose a disturbance such as that which broke up the nebulosity of the mass which forms the planetary system, and condensed it into separate globes. Such disturbance might come from without or from within; there are forces in nature to account for either. We have supposed all or many of the elements present; but in a nebulous form they would be in a finely divided state, and many of them, perhaps all the baser metals, have such affinity to oxygen, as when in that state, to take fire on coming in contact therewith; so that any cause, which would bring them and the oxygen into contact, would cause fearful explosions, and set the whole nebula in

* It is a remarkable fact that no *new* elements have been found in meteoric stones, which are now regarded as planetary bodies. "I would ask," says Humboldt, "why the elementary substances which compose one group of cosmical bodies, or one planetary system, may not in a great measure be identical? Why should we not adopt this view, since we may conjecture that these planetary bodies, like all the larger or smaller agglomerated masses revolving round the sun, have been thrown off from the once far more expanded solar atmosphere, and been formed from vaporous rings describing their orbits round the central body?" (Cosmos, vol. i, p. 120.) Mr. Wackerbarth extends this hypothesis in regard to the identity of the elementary substances to *stellar systems*.

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ignition, condensing it into burning masses, each by the violent explosions casting out smaller fused and burning masses into space, to revolve as burning globes around itself, or the centre of gravity of the whole. But what would be the condition of one of these fused and blazing masses? Hydrogen is present as well as oxygen; and ignition must therefore immediately cause the production of water. An ocean is thus poured down on the incandescent globe, to be cast up again as steam into a damp atmosphere formed by the nitrogen and the watery vapor. Here it cools, and is again poured down in torrents on the glowing mass; and this process must continue until the globe has sufficiently cooled for the water to be able to rest upon its surface. Now, I imagine that the appearance of a globe in such a state viewed at a stellar distance, must be variable; that when the water is, in the form of steam, driven up into the atmosphere, the burning mass must glow with greater brilliancy; but when, the steam condensing, a boiling ocean is poured upon it, the violence of the conflagration must for a time abate, and thus the object assume a less brilliant appearance, until the fire has succeeded in reconverting the water into steam, and driving it up again into the atmosphere.

"Another circumstance may be mentioned as possible, namely, some bodies may be at present in some parts extinct; while other parts of them are yet fused and burning. Would not the revolution of such a body present the phenomena of a variable star?"*

8. *The Satellites.*—It is a remarkable coincidence in regard to the motions of the moon that her rotation is completed in a period precisely equal to that of her orbital revolution. The same is true of Jupiter's satellites, at least *some* of Saturn's, and probably, indeed, of all secondaries. Such coincidences are not to be ascribed to chance. Either, therefore, we must regard them as *ultimate facts*, or refer them to the operation of those primitive natural forces by which other phenomena of the heavenly bodies were produced. Few correct thinkers, we presume, would be inclined to adopt the former alternative. "The craving of the philosophic mind is for *explanation*, *i. e.*, for the breaking up of complex phenomena into familiar sequences, or equally familiar transitional changes, or contemporary manifestations."† There can be no doubt that the equality in question is due to the operation of known physical forces. Sir Isaac Newton's explanation,‡ it is well-known, assumes the original fluidity of the satellites. The attraction of the earth on the primitive fluid mass of the moon would produce an elongation of the hemisphere in the

* Monthly Notices of the Roy. Astr. Soc. for May 9th, 1856.

† Edinburgh Review, No. 175. ‡ Principia, B. iii, Prop. xxxvii, Cor.

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direction of the primary. The force of gravity of this tidal elevation would maintain the greatest axis in the direction of the attracting body, thus producing the observed coincidence. The same applies to the other secondary planets.

This equality, we believe, is best explained by the nebular hypothesis. Newton's explanation pre-supposes the two motions to have been originally so adjusted as to differ *very little* from exact coincidence. But if the moon once existed in a state of vapor, its volume was much greater than at present, and the gravity of particles at its surface proportionally less; while from the fact of their gaseous condition they would yield with the greatest facility to any force impressed upon them. The equality between the two motions might thus become established long before our satellite had contracted to its present dimensions.

But why, it may be asked, should this isochronism obtain universally in the subordinate systems, while there is not the least approximation to it in the case of any primary planet? It has been suggested that "the integrity of the motions of the ring, when it resulted in satellites, may have arisen from the comparative maturity of the system—then approaching the close of the first epoch in its history."* Without entering here into any discussion of this question, we may remark that in the case of the secondary planets, *the epoch of solidification would evidently be reached, and consequently the acceleration of the rotary velocity arrested, at a comparatively early period of their existence.* "The motions of the three first satellites of Jupiter present a phenomenon still more extraordinary than the preceding; which consists in this, that the mean longitude of the first, minus three times that of the second, plus twice that of the third, is constantly equal to two right angles. There is the ratio of infinity to one, that this equality is not the effect of chance. But in order to produce it, it is sufficient, if at the commencement, the mean motions of these three bodies approached very near to the relation which renders the mean motion of the first, minus three times that of the second, plus twice that of the third, equal to nothing. Then their mutual attraction, rendered this ratio rigorously exact, and it has moreover made the mean longitude of the first, minus three times that of the second, plus twice that of the third, equal to a semi-circumference. At the same time, it gave rise to a periodic inequality, which depends on the small quantity, by which the mean motions originally deviated from the relation which we have just announced. Notwithstanding all the care Delambre took in his observations, he could not recognize this inequality, which, while it evinces its extreme smallness, also indicates, with a high degree of probability, the existence of a cause which makes it disappear. In our hypothesis,

* Dr. Nichol.

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the satellites of Jupiter, immediately after their formation, did not move in a perfect vacuum; the less condensible molecules of the primitive atmospheres of the sun and planet would then constitute a rare medium, the resistance of which being different for each of the stars, might make the mean motions to approach by degrees to the ratio in question; and when these movements had thus attained the conditions requisite, in order that the mutual attraction of the three satellites might render the relation accurately true, it perpetually diminished the inequality which this relation originated, and eventually rendered it insensible. We cannot better illustrate these effects than by comparing them to the motion of a pendulum, which, actuated by a great velocity, moves in a medium, the resistance of which is inconsiderable. It will first describe a great number of circumferences; but at length, its motion of circulation perpetually decreasing, it will be converted into an oscillatory motion, which itself diminishing more and more, by the resistance of the medium, will eventually be totally destroyed, and then the pendulum, having attained a state of repose, will remain at rest forever."*

9. *The Asteroids.*—Our data perhaps may not yet be sufficient to afford a satisfactory explanation of the phenomena of the asteroids. The question, however, in regard to their origin, and the bearing of this inquiry on the cosmogony of our system, are subjects of more than ordinary interest. The following facts appear to indicate the most plausible theory of their origin:—

(1.) These bodies occupy a chasm—observed before their discovery—in the order of the planetary distances; an order which indicated the existence of a *single planet* where *fifty-eight* have already been detected.

(2.) The members of the group are characterized by certain peculiarities which are doubtless indicative of an intimate mutual relationship. They are extremely diminutive in size; the volume of the largest probably not exceeding the ten-thousandth part of that of the earth. The eccentricities and inclinations of their orbits are generally much greater than those of the other planets. Their orbits interlace. "The strongest evidence," says D'Arrest, "of the intimate connection of the whole group of small planets appears to be, that if the orbits are supposed to be represented materially as hoops, they all hang together in such a manner that the entire group may be suspended by any given one." The occasional rapid variation in the apparent magnitude of several members of the group—changes not attributable to variations of distance—would seem to indicate some peculiarity in their physical constitution.

(3.) The asteroids already discovered amount to one for about every six degrees of longitude. Their number appears to *in-*

* Harte's Translation of Laplace's System of the World, vol. ii, p. 367.

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crease with the *decrease* of their apparent magnitude. In all probability hundreds of these bodies will have been discovered by the close of the present century; and the existence of immense numbers too small to be detected, cannot reasonably be doubted. In fact, the zone is even yet almost entitled to the appellation of a *primary planetary ring.*

(4.) Should the rings of Saturn, (now regarded by some astronomers, as fluid,) from any cause whatever become separated into parts, and collect about distinct nuclei, a ring of *secondary asteroids* would be formed, analogous to the zone between Mars and Jupiter.

(5.) It has been affirmed by an eminent astronomer that the rings of Saturn are sustained by the direct action of the satellites; that no other planet of the system has such an arrangement of secondaries as to secure the stability of a ring; and that the only place in the solar system where a primary ring would be long sustained, is the region occupied by the small planets.

(6.) The amount of perturbation in this point of the system is extraordinary in consequence of its proximity to the enormous mass of Jupiter.

Now according to the nebular hypothesis each of the principal planets originally existed as a gaseous ring. The observed order would require such an annulus between Mars and Jupiter, and this is the precise situation in which a ring would be the longest sustained by the exterior planets. Upon the breaking up of the ring, however, *a zone of small planets would naturally be formed* unless some one portion of the vaporous mass should have a preponderating influence so as ultimately to absorb all the rest. In short, it is believed that the nebular hypothesis will explain the various phenomena of these bodies, which without it seem inexplicable.

10. *The Zodiacal Light.*—Cassini regarded the zodiacal light as produced by an innumerable multitude of small planetary bodies revolving in a ring about the sun. This hypothesis was generally accepted until 1855, when another was proposed by the Rev. George Jones, of the United States Navy. Mr. Jones thinks his own observations of the phenomena wholly incompatible with the hypothesis of a nebulous ring *with the sun for its centre*, and also with that of a nebulous planetary body revolving round the sun. He is led, therefore, to the alternative of a *nebulous ring round the earth, interior to the moon's orbit.* This theory has been favorably received by the astronomers of our country. Objections of some weight, however, have been urged against it by Pres. F. A. P. Barnard,* of the University of Mississippi, and it will not perhaps be generally adopted without further confirmation. But it is generally admitted by the advocates

* This Journal for March, 1856.

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of each hypothesis that the appearance is produced by a *continuous zone of infinitesimal asteroids.* Now if the abandoned rings of either the *solar* or the primitive *terrestrial* atmosphere contained any matter of too volatile a nature to coalesce in the formation of solid planetary bodies, such molecules ought to exhibit the appearance of a flattened ring, of a vapory or nebulous form, like that actually presented by the zodiacal light. Whether we regard it, therefore, as a primary or secondary ring, the theory of Laplace affords the most plausible explanation of its origin.

11. *The Rotations of the Planets.*—Is there or is there not any bond of union between the periods of rotation of the different members of our system? That these rotations should be mere isolated facts, independent of all order and relationship, seems, to say the least, extremely improbable. Even in the process of crystallization the atoms of matter arrange themselves in regular mathematical forms; the chemical combination of elements occurs only in definite proportions; the *orbital* motions of the planets are governed by invariable laws; in short, order and harmony appear to characterize all the operations of nature around us, or in the language of an ancient philosopher, "God geometrizes." Is there any probability that we have an exception to this general prevalence of law, in the *rotations* of the heavenly bodies?

The first attempt, it is believed, to develop a connection between these apparently independent elements, was undertaken by the writer in 1839. The result of these researches was the discovery of an interesting planetary harmony, which, in 1849, was communicated to the American Association for the Advancement of Science.* It is not claimed for this Analogy that it has been fully established as a physical law. It appears, however, to harmonize too closely with the known elements of the solar system, to be regarded as merely an accidental coincidence. Now the immediate dependence of one of the elements of this analogy—viz., the diameter of the sphere of attraction—on the nebular hypothesis, is obvious. It is evident also that coincidence between the periods of rotation and revolution is a necessary characteristic of planetary rings revolving around a central mass, and that on the transformation of a ring into a planet the same equality would obtain when the *diameter* of the latter was equal to the *breadth* of the former. Any further condensation, however, would *increase* the rotary velocity, and thus shorten the period of axial revolution. Hence *the ratio of the angular velocity of rotation to that of orbital revolution, or what is the same thing, the number of a planet's days in its year, would be a function of the diameter of the sphere of attraction.*

12. *The distribution of the satellites.*—Another interesting con-

* See Proceedings of the Am. Assoc., for 1849, p, 207.

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sideration may here be suggested. Planets of greater magnitude and having greater spheres of attraction would probably throw off the greater number of rings. An explanation may thus be given of the striking fact in the constitution of the solar system, that the exterior planets have numerous satellites. Saturn, which has the greatest sphere of attraction, has the greatest number of satellites; Uranus, the next in order in this respect, has the next greatest number; while Jupiter, which is the *third* in regard to the former, is *third* also in the latter. Mars, it is true, has a greater sphere of attraction than the earth; it may be remarked, however, that owing to the small size of the planet, and its proximity to Jupiter, a satellite would not be *stable* unless very near the primary.

13. *Recapitulation—Simplicity of the Nebular Hypothesis.*—When the number and variety of the phenomena explained by the nebular hypothesis are duly considered, the weight of evidence in its favor must be admitted, we think, by every unbiassed mind. The rotation of the sun; the unity of direction of the planetary motions; the approximate coincidence of the planes in which the planets move, with the plane of the solar equator; the general agreement between the direction of the axial and orbital revolutions; the small eccentricities of the planetary orbits; the rings of Saturn; the central heat of the earth; the oblate form and primitive fluidity of the planets; the origin of comets; telescopic revelations in regard to the physical constitution of the sun, moon, and some of the planets; the phenomena of variable stars; the equality between the periods of rotation and orbital revolution of the satellites; the extraordinary phenomena presented by the first three satellites of Jupiter; the zone of asteroids between Mars and Jupiter; the zodiacal light; the analogy between the periods of rotation of the primary planets; the distribution of the satellites;—all, as we have seen, are accounted for by the nebular cosmogony, while for many of them at least, no other explanation has ever been offered. Adopting this hypothesis, all the motions of the solar system are derived from a *single impulse* communicated by the Creator to the primitive nebula: rejecting it, each motion of every member demands the *separate* operation of his power. Now, "if there be two modes of explaining any phenomenon of nature, then, *cæteris paribus*, that is the most probable which is the most simple. For by what we observe in the creation around, we are forced upon the conviction that the Almighty acts in this respect with that *economy* of creative energy, which, although infinitely more perfect in its degree, has nevertheless its visible type in that husbandry of *our* resources, that disposition to economy in *our* efforts, which impels us always to avail ourselves of the simplest possible means of effecting all that we wish to do.

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"Thus, when in reasoning upon any hypothesis, we are forced back upon secondary causes, it is sound philosophy to judge of the probability of that hypothesis, according to the simplicity or complication of the causes to which we are thus compelled ultimately to refer it. If, for instance, there be two hypotheses, by one of which we shall be compelled to fall back upon a double operation of the hand of the Almighty, whereas the other resolves it into a single effort of his will, then is the latter hypothesis, according to the analogy of nature, more probable than the former, and that infinitely."

Objections to the Nebular Hypothesis.

1. *The Satellites of Uranus.*—It has been objected to the nebular hypothesis that it cannot be reconciled with the retrograde motion of the satellites of Uranus. We reply that in every instance, so far as we know, the motions of secondary planets are performed in planes nearly perpendicular to the axis of the primary, but that in no case, with the single exception of Jupiter, is the axis of a primary planet nearly perpendicular to the plane of its own orbit. The earth's axis is inclined more than twenty-three degrees; that of Mars, as well as that of Saturn, nearly thirty; while that of Venus, if we may rely on the observations of Schroeter, is inclined more than seventy degrees. Now if this amount of inclination does not invalidate the hypothesis in question, ought the greater one of one hundred degrees—which gives the satellites of Uranus their backward motion—to be regarded as wholly inconsistent with it? These inclinations, as well as the slight eccentricities of the orbits, and their deviations from the plane of the sun's equator, may be ascribed partly to irregularities in the density and temperature of the planets in a state of vapor, and to the operation of molecular forces during the process of condensation. The following suggestion of Laplace is also worthy of note:—"If any comets have penetrated the atmospheres of the sun and planets at their formation, they must have described spirals, and consequently fallen on the bodies, and in consequence of their fall, caused the planes of the orbits and of the equators of the planets to deviate from the plane of the solar equator."* "The probabilities of the case," says Professor Nichol, "certainly favor the idea, that it [the anomalous motion of the Uranian satellites] is a *disturbance*, an *interference* with the *order* of the system, by some *foreign* law or occurrence."†

But while the hypothesis requires *in general* that the direction of the planetary rotations, as well as that of the orbital motions of their satellites, should be the same as that of their revolution

* System of the World, Harte's Translation, vol. ii, p. 364.

† Thoughts on some Important Points Relating to the System of the World. First American Edition, p. 109.

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round the sun, an *opposite* direction is not, as some have supposed, incompatible with it. Perhaps, indeed, it is not more improbable than a permanent ring, like that of Saturn, or a zone of small planets like the asteroids. We have supposed that on the breaking up of a nebulous ring its matter collected into several masses, the orbits of which were not greatly different, and that they revolved separately during an indefinite period, but ultimately united, except in the case of the asteroids, in *one* planetary body. Those primitive masses, owing to perturbations, &c., would meet in different directions, and it is by no means impossible that the circumstances of the collision and coalescence of the nebulous fragments may have been such as to overcome the natural tendency to rotate in the direction of the orbital motion.

Again: the form of the primary ring may have influenced the original *direction* of the planet's rotation, and hence that of its *secondary* rings and the resulting satellites. If the thickness of a ring was greatest in the direction at right angles to the plane of its motion, or if the diameter of its mass when it first assumed the form of a gaseous planet, was greatest in that direction, the plane of rotation would probably, for obvious reasons, be inclined to that of the orbit; the amount of inclination depending upon the ratio of the diameters.*

2. *The Revelations of Lord Rosse's Telescope.*—The opponents of the nebular hypothesis affirm that it derived its chief support from the supposed existence of irresolvable nebulæ; that is, of widely diffused vaporiform matter not yet aggregated into stars, but slowly undergoing the process of condensation. Such, until 1846, was believed to be the constitution of a large proportion of those mysterious objects. The great nebula in Orion, for instance, was thought to present decided indications of irresolvability. Lord Rosse's telescope, however, has shown it to consist at least in some portions, of minute stellar points, in extremely close proximity; in the language of Dr. Nichol, "every wisp, every wrinkle, is verily a sand-heap of stars." This discovery has been considered by some as satisfactory evidence that all nebulæ are resolvable. The sublime conception of Laplace, it has been dogmatically affirmed, can hereafter be regarded as but a "splendid vision." We reply that the *principal* evidence in favor of the theory, is that afforded by the phenomena of the solar system itself, and that *this* would not be in the least invalidated should every nebula within the reach of the telescope be resolved into stars. But no results hitherto obtained justify the conclusion that all nebulæ are composed of stars. A large proportion remain still unresolved, even under the highest power of

* This idea has also been suggested by an anonymous writer in the Westminster Review.

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the "Parsonstown Leviathan." As this, however, is a point much insisted on by the opponents of the nebular cosmogony, we shall endeavor to place it in its proper light.

The hypothesis that certain nebulæ consist of cosmical vapor gradually condensing around stellar nuclei, was proposed by Sir William Herschel; that of the genesis of the solar system from a primitive nebula was suggested by Laplace. The former resulted from a critical examination of the nebulæ themselves; the latter, from a philosophical discussion of *the phenomena of the solar system.* Laplace, it is true, adopted the Herschelian theory of nebular matter, and regarded it as highly favorable to his own hypothesis. Popular writers of the present day, however, look upon the former as constituting the entire ground of Laplace's speculations. "The theory," says a distinguished author,* "proceeds on the assumption of the existence and wide diffusion of a nebulous fluid." The achievements of Lord Rosse's telescope are accordingly claimed as almost decisive evidence against Laplace's cosmogony.

Now if the mere fact that we have no certain knowledge of the *present* existence of nebular vapor warrants the inference that the *primitive* condition of the solar system was not gaseous, by parity of reasoning it would follow that unless some of the *members* of the solar system are *now* in a fluid state, our own globe has always been *solid.* But the *form* of the earth proves its ancient fluidity; and so, in like manner, various phenomena of the planetary system indicate a primitive nebular state.

When it is remembered, moreover, that the analogy of the solar system is decidedly against the assumption that the different appearances of the stars are owing to a gradation of distances; that portions of our own sideral cluster, the milky-way, *have never been resolved;* and that while Lord Rosse's telescope has separated particular *parts* of some nebulæ into stellar points, it has left other parts *unresolved*, and revealed very faint nebulous ramifications which *cannot* be composed of stars, unless we regard the components as extremely minute, the nebular hypothesis does not appear to have been materially weakened by Lord Rosse's discoveries.†

Finally, we may remark that comets and the zodiacal light *demonstrate* the existence of cosmical vapor similar to that which the theory assumes.

* James Buchanan, D.D., LL.D.

† "Regarding the *nebulæ* as spherical in form, and not as vastly long vistas foreshortened by having their ends turned towards the earth—which would be improbable, seeing there are two of them close together—the brightness of objects in their nearer portions cannot be much exaggerated, nor those in its remote, much enfeebled by difference of distance. It must, therefore, be an admitted fact that stars of the seventh and eighth magnitudes and irresolvable nebulæ may coëxist within limits of distance comparatively small, and that all inferences in regard to relative distance drawn from relative magnitudes must be received with caution."—Bartlett's Spherical Astronomy, p. 222.

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3. *The alleged atheistic tendency of the Nebular Hypothesis.*—The most prominent objection to the nebular hypothesis is its alleged *atheistic tendency.* It has been characterized as an attempt to exclude God from his own universe, by substituting *natural law* for his direct agency in the work of creation. The force of this objection must evidently depend upon the question—What are we to understand by the "laws of nature?" As the question is one in regard to which much misapprehension appears to have existed, its brief consideration may not be out of place.

In the nebular theory, the process of condensation, the separation of rings and their conversion into spheroids, the acceleration of rotary velocity, solidification, &c., are supposed to have occurred in accordance with the known laws of gravitation, centrifugal force, cohesion, and chemical combination. The hypothesis does *not* assume, however, that these laws were self-originated and independent material forces. It has no conflict with Divine revelation. It assumes the existence of chaotic matter—has nothing to do with its origination. Its advocates therefore can consistently grant that matter was created by a Being of infinite power, and that the *quantities* of the various elements were determined, their collocations arranged, and their respective properties appointed by a Being of infinite wisdom. *Laws of nature are formal expressions of the ordinary modes in which the Divine will is manifested in natural phenomena.* McCosh* specifies three different senses in which the phrase is understood, but they are all embraced in the preceding definition. "A law never acts;" but it invariably points to an intelligent agent or designer. Gravitation, for instance, is neither an essentially inherent property of matter, nor an absolute *cause* of motion. The Newtonian law by which it is expressed, so far from being independent of an All-wise and Omnipotent Author, is simply the rule by which the Creator governs the material universe. The assertion therefore that this law "is probably the only efficient principle of the creation of the physical world, as it is of its preservation,"† is obviously absurd.

The fact then of the existence of a *law*, necessarily implies the existence of a *law-giver;* hence the objection which we have stated is destitute of foundation. It must follow also as a necessary consequence that the nebular hypothesis is utterly *incompatible* with the very system to the support of which a false philosophy has attempted to pervert it. Moreover, if the power of the Deity is manifested in accordance with a uniform system—a "law of nature"—in *sustaining* and *governing* the material universe, why should it be regarded as derogating from his perfections to suppose the same power to have been exerted in a similar way in the process of its formation?

* On the Divine Government, B. II., chap. i, sec. 1.
† Pontécoulant.

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But some writers, among whom we may mention in particular the author of the "Vestiges," have attempted to connect the nebular hypothesis with the Lamarckian theory of development. Each of these hypotheses, however, is complete in itself. The arguments which lie against the latter have no logical bearing whatever against the former. It is not necessary to our purpose therefore to discuss the Lamarckian theory; this has been ably done by Miller, in his "Footprints of the Creator;" Buchanan, in his "Modern Atheism;" and by other distinguished writers on both sides of the Atlantic. We have no hesitation, however, in affirming that its complete refutation leaves the nebular hypothesis untouched, and that the demonstration of the latter would afford no evidence of the truth of the former. To regard them as interlinked, dependent, and essential parts of a great atheistic scheme, is to mistake entirely their mutual relationship.

The foregoing are, we believe, the most weighty objections that have been urged against the nebular hypothesis. The first, or that derived from the retrograde motions of the satellites of Uranus, has doubtless the greatest force; the most eminent astronomers, however, have not deemed it insuperable.

But if we admit, in general, the truth of the nebular hypothesis, the question still remains whether the special form of it proposed by Laplace is that which obtained in the formation of our system, or whether certain modifications ought not to be admitted. Did the chaotic matter, for instance, advance with regularity through all the stages of condensation, or were its contractions sudden and violent? The former supposition, which is that of Laplace, has been generally adopted by writers on the subject; but the latter, as has been remarked by Professor Nichol, seems more in harmony with the known operations of nature around us. When gases pass to the liquid form it is not generally by gradual condensation: on the other hand, such changes are characterized by rapid and energetic action. The same is true, at least in some cases, when bodies pass from a liquid to a solid state.

It will be seen, we think, from the foregoing discussion, that the confirmation of the nebular hypothesis would tend to reduce the apparently isolated phenomena of the solar system to the domain of law, and show that, in a cosmical point of view, the material world, in each of its diversified operations, exhibits a *uniformity of action*, adapted to develop our intellectual powers, and direct our thoughts to the great First Cause—the ever-present, all-sustaining Governor of the Universe.

Bloomington, Indiana, 1860.

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ART. XVI.—*On a new Theory of Light, proposed by John Smith, M.A.;* by OGDEN N. ROOD, Professor of Chemistry in Troy University.

SEVERAL months ago, when attempting, by means of a revolving disc, to measure the time occupied by the explosion of small charges of gunpowder, the following observation was made: The flame of a burning-fluid lamp was viewed through a rotating disc provided with four radial slits, and it was found that a certain rapidity of rotation caused the lower part of the flame to assume a green hue, while by a diminished rate the whole flame was colored deep purplish red; a lower rate gave a violet tint, alternating with pure white.

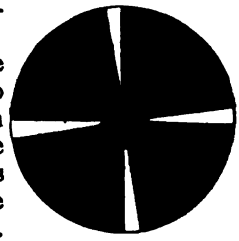
It was evident that these appearances depended much on the state of the eye, for they often could be perceived only after it had become a little fatigued by the blinding effect consequent on the comparatively slow succession of the impressions of light; in addition to this, I found that my colleague, Prof. Vincent, though recognizing the green tint, was unable to perceive the red hue, with which my eyes at that very moment were dazzled.

I considered these appearances, therefore, as *subjective*, and laid the matter aside for future experiment. As, however, similar phenomena have been observed in England, and have been thought to have not only an objective existence, but to be worthy materials on which to build a new theory of light, it may not be amiss to enter into a slight examination of their nature.

In Mr. Smith's experiments, of which I have been able to obtain only the account given in the March number of this Journal, bright white light is allowed to act on the eye during a certain fraction of a second; it is succeeded by shadow or darkness, which lasts also during a certain short interval of time, when the operation is repeated anew, &c.

This pulsation of light and shade the author effects in a variety of ways: the result is color—a yellowish green, purple, pink, &c. Fechner, to whom we are indebted for extensive researches on sight, several years ago observed that white discs having black spiral figures painted on them, when set in rotation exhibited colors which he considered subjective.*

1.



That these colors are really subjective, the following simple experiment may serve to show: A blackened disc nine inches in diameter, was cut with four slits of the shape seen in the wood cut; the width of the slits at the circumference was $\frac{5}{16}$ of an inch; the disc was made to rotate before a bright cloud.

* Pogg. Ann. vol. xlv, p. 227.

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A rate of ten revolutions per second caused the cloud in a short time to appear of a deep red color, having in it a tinge of purple, or, according to Mr. Smith, the disc transmitted pink light; it was now viewed through a plate of orange-tinted glass. Previous experiment with a small telescope provided with a micrometer, and a flint glass prism, had shown that this orange colored glass readily transmitted the red, orange, yellow, and a portion of the green, but that it was opaque to the blue and violet rays; it was therefore fairly to be expected that if the disc was really transmitting red light, the plate of glass would do the same. The result was different; through the glass the disc appeared of a bright greenish-blue color. This experiment is very easy to make, and the effect is brilliant.

Plates of glass of other tints were now employed; the results are given below.

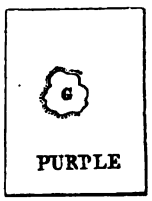
Medium.	*Tints of the Disc*
Yellow glass,	Violet,
Green "	Green, neutral, or faint red,
Red "	Red, neutral, or faint green.

As the green glass was nearly opaque to red rays, the effect of its use ought to have been darkness. It is evident by an inspection of this table that the disc really transmitted *white*, and not red light, which becoming colored by its passage through the plates of glass, induced in the retina, from time to time, the sensation of the complementary tint, more or less mingled with the *original* impression.

Having now shown that, contrary to Mr. Smith's supposition, the light transmitted by the revolving disc is really white, let us notice some of its effects on the eye.

For this purpose I caused perforated discs to revolve at uniform rates by means of clock-work; the arrangement being similar to that employed by Plateau. A blackened disc five inches in diameter and perforated with four slits 7° 12′ in the width was set in rotation, and the bright sky viewed through it; the eye of the observer being immediately behind the disc. With a rate of 11½ revolutions per second, the appearance of the window was as in fig. 2; a central spot was colored bluish-green, the rest of the field was purple, or reddish purple, according to the state of the eye. The green spot remained always in the axis of vision, and moved with each change of it. With the exception of fluctuations in the outline of the spot, this appearance remained tolerably constant, as long as the rate of revolution continued the same. The spot or shadow was fringed with a narrow, faint blue border, indicated by the dotted line.

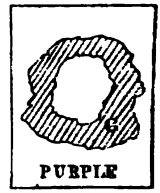
2.



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Upon increasing the rate of revolution, the bluish-green spot expanded into an irregularly shaped ring, and continued to expand, filling the field, till the rate had become as high as 15 rotations per second, when often the field for an instant became of a greenish tint which was succeeded by a bluish tint; upon increasing the speed this also vanished. Still higher rates cease to produce any of these peculiar effects on the eye.

3.



Upon slowly reducing the rate to 9 revolutions per second, the green spot contracted in dimensions, and assumed a yellow tint, while the field often became at the same time tinted deep crimson. With a rate still lower, the appearance of the field is variable and the tints flickering; it assumes sometimes a purple, a yellow, or a yellowish green tint.

This experiment I repeated a great number of times, with the same general result, and though it sometimes happened that the eye became insensible to these colors, from repetition, momentary rest in darkness restored this power for a short time. Thus it occurred that the tints were sometimes seen with great distinctness, while at others they could hardly be distinguished.

Upon a dark cloudy day to produce these effects it was found necessary to increase the width of the slits to 20°; from whence it was manifest that lack of intensity in the light might be made up by its longer duration.

It would appear then, from these experiments, that light from a bright cloud, if allowed to act on the eye repeatedly during from $\frac{1}{7}\frac{1}{5}\frac{1}{0}$ to $\frac{1}{7}\frac{1}{5}\frac{1}{0}$ of a second, developes subjective colors; that, however, the development of the subjective tint is dependent not so much on the length of time which the eye is exposed, as upon the interval of *rest* or shade which follows each exposure, may be shown in the following manner: In the experiment where with $11\frac{1}{3}$ revolutions a reddish purple was produced, the exposure lasted $\frac{1}{3}\frac{1}{7}\frac{1}{5}$ of a second; the interval of rest or shade was $\frac{1}{3}\frac{1}{5}$ of a second; now a disc was cut similar to fig. 1, but having eight slits, each 7° 12′ in width, when it was found that 5·5 instead of 11·5 revolutions per second produced the purplish-red tint; here the exposure was twice as long, but the interval of rest or shade nearly the same. With sixteen slits, $2\frac{3}{7}\frac{3}{5}$ revolutions produced the same tint, the exposure being of course four times as long, but the interval of rest nearly the same. Determinations of the length of this interval are given below:—

Length of the intervals of shade required for the production of

Blue,.....$\frac{1}{6}\frac{1}{5}$ sec.
Purplish-red,.....$\frac{1}{3}\frac{1}{0}$ "

To ascertain exactly what portions of these intervals elapse

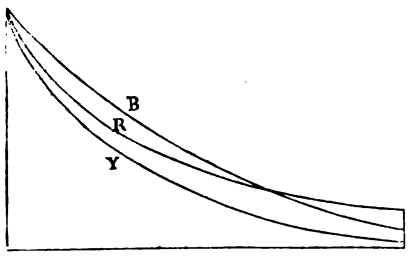
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before these tints make their appearance, or how long the tints are actually seen by the eye, is another matter, and would no doubt require an elaborate series of experiments, though it would seem probable that at least half of the time of the above given intervals passes before the subjective color makes its appearance.

The table does not apply to the axial portion of the retina, which is almost always differently affected. That the change in susceptibility in the retina is progressive outwards, is shown by the gradual expansion of the green ring; that it varies from second to second, is seen in the fluctuations of the outline of this ring.

The occurrence and sequence of these subjective colors may easily be explained by supposing that during the interval of rest or shadow the action of the yellow rays diminishes more rapidly than that of the red, the red more rapidly again than that of the blue. If this takes place as indicated by the curves below, it is easy to understand the production of the tints, for if the moment after the blue has been developed white light be again presented to the eye, it is evident that neither purple nor red will be seen, sufficient time not having been allowed for their production.

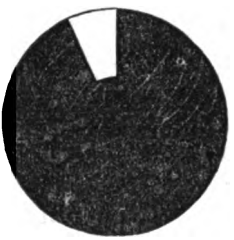
4.



The same reasoning applies to the axial portion of the retina, which, owing to its greater sensibility to such impressions of light, requires a somewhat longer interval of rest before the reaction occurs.

If the impression be too strong, that is, if the light be too bright or the eye too long exposed to it, these peculiar effects are not observed, and during such short intervals of rest as $\frac{1}{7\,5}$ or $\frac{1}{8\,5}$ of a second the white clouds seen through the discs suffer no change in tint; but if a blackened disc twelve inches in diameter be cut as seen in the figure, with an aperture of 30°, and made to rotate before a white cloud at a rate of only one revolution per second, the eye placed as near it as possible, will most distinctly see, in the interval of darkness, an image of the sky, of a bluish-green tint. Whence it follows that an exposure of the eye to white light lasting $\frac{1}{7\,5}$ of a second, induces in it for a considerably longer time the sensation of this color.

5.



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In general these experiments seem to point out that after momentary exposure to white light subjective colors are induced in the eye, whose tint and duration are dependent on the strength of the impression received, as well as upon the length of time allowed for rest; these sensations of color apparently having a relation to the colors observed after looking at the sun, similar to that which a temporary disorder bears to a chronic affection.

Troy University, March 20, 1860.

ART. XVII.—*On the Meteor of November* 15th, 1859; by Prof. H. A. NEWTON, Yale College.

IN the forenoon of November 15th, 1859, about half-past nine o'clock, a meteor or meteorite was seen to descend towards the earth by many persons at widely different places. Throughout the southern part of New Jersey a tremendous explosion was heard immediately after its appearance. Mr. Benjamin V. Marsh of Philadelphia has published in the Journal of the Franklin Institute, a very valuable collection of statements of persons who saw it from various positions, from Newburyport, Mass., to Petersburg, Va. In the January and March numbers of this Journal Prof. Loomis has also given a selection from the accounts of several persons who were so fortunate as to witness it.

The physical circumstances attending its passage through the atmosphere, its explosion, and its entire or partial combustion are of interest and the accounts referred to are of great value in studying them. My present purpose, however, is to determine as accurately as possible the *path* and *velocity* of the meteorite. The result of my investigation has been to establish almost beyond a doubt the conclusion, that *this body was not a member of the solar system but came to us from the stellar regions.*

In several instances have the paths of meteors or shooting stars been determined and velocities been computed which would necessitate with respect to them the same conclusion. But in general the data are so vague that the result lacks the certainty which is afforded by the meteor of November. This is especially true of the velocities given by M. Petit of Toulouse. In not a single instance have the results given by him seemed to me worthy of confidence. I have thought, therefore, that a careful discussion of this meteor was desirable.

The observations from Alexandria, Va., and New Haven, Ct., seem for several reasons the best for determining its path through the atmosphere. Mr. Marsh quotes the following from a letter of Mr. Caleb S. Hallowell of Alexandria.

"Abram Martin, a student particularly well qualified for such observations, was fortunately, standing perfectly still with the

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meteorite in full view during its entire visibility, and he seems to have completely daguereotyped its path and appearance on his mind. By my direction he made at the time a careful note of the spot where it fell, and as to the point in the heavens from whence it originated, he, on two *independent* and *distinct* occasions pointed the telescope of the theodolite very nearly to the same spot, as given below.

	1st obe'n.	2nd o' s'n.	Mean.
Apparent altitude at moment of appearance, .	40°	39½°	39½°
Azimuth, " " "	N. 83 E.	N. 85 E.	N. 84 E.
Apparent altitude at moment of disappearance, behind houses, ...	11°	10°	10½°
Azimuth, " " "	N. 77 E.	N. 76 E.	N. 76½ E.

The time of flight was estimated at two seconds. The determination of the point from which it originated was made some weeks after the 15th of November.

In New Haven it was seen by Judge W. W. Boardman, who was standing at the time in the west door of the Exchange Building, Church Street. He was conversing with a friend, and looking S.E., when his attention was caught by the meteor down the street.

It shot obliquely down across the street and disappeared behind the cupola of the Catholic church, which is 700 or 800 feet distant. The course of the street is about S. 29° W. He says it must have been seen as far east as the line of the street. Mr. E. C. Herrick, from conversation with Judge Boardman some days after the meteor was seen, concluded that the most probable angle of its path with the vertical was 33°, though it might vary from this several degrees. This inclination was obtained, first by calculation from the apparent altitude at the moment of appearance, and second, by holding up a rod at the inclination which Judge Boardman said equalled that of the meteor's path and measuring this inclination. The perpendicular lines of the buildings would help to impress on his mind the direction. The point of disappearance was, in azimuth S. 35° 34′ W., at an altitude of about 6°. The time of flight was estimated as one second, or between one and two seconds.

The position of Judge Boardman was N. lat. 41° 18′ 18″, W. long. 72° 55′ 10″. That of Alexandria is N. lat. 38° 49′, W. long. 77° 4′. There are indications which go to show that the path of the meteor was not a straight line, especially near its lower extremity. But the part seen by Judge Boardman must have been very nearly straight, and may be so considered without serious error. The plane passing through New Haven, and the point of the heavens S. 35° 34′ W., altitude 6°, and making there an angle of 33° with the vertical, meets the plane passing through Alexandria and the two points given in the last column of the table of Mr. Hallowell in a straight line, which I shall consider the path of the meteor. This line cuts the earth

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in W. long. 75° 10′ 30″, and N. lat. 39° 13′, a place a mile north of False Egg Island Point, just on the eastern shore of Delaware Bay. The azimuth of the point from which the meteor would approach the earth, at that place, was N. 88° 55′ E., and the altitude was 49° 35′.

Observations at New York, and also in the neighborhood of Dennisville, N. J., prove that the azimuths of the points of disappearance at each station are quite exact. The only important question is, whether the inclinations reported are reliable. To test their general accuracy, I have collected such other observations as seem at all reliable, and can be compared with them.

The inclination of 33°, at New Haven, is confirmed by observers at Salem, Mass., Waterbury, Ct., New York city and Newark, N. J. These places were all N.E. of the meteor and nearly in the same azimuth as New Haven. In the third column of the table below, is given the reported inclination, and in the fourth, the angle at which the path above assigned to the meteor would be seen to be inclined to the vertical circle at the horizon.

	Place	Observed inclin.	Calculated inclination	Obs. – cal.	Cal. – obs.	Observers.
1	Salem,	40°	31° 21′	8° 39′		Francis F. Wallis.
2	Waterbury,	30	34		4°	Wilder Smith.
3	New York,	10	35 27		25 27′	Mr. Gould.
4	" "	20	35 27		15 27	Mr. Latham.
5	" "	35	35 27		27	J. P. Pirrson.
6	" "	45	35 27	9 33		Mr. Bradley.
7	Newark,	45	36 33	8 27		Henry J. Mills.

The observations that appear to me from the circumstances of the observer most worthy of confidence next to that at New Haven are taken in order, the fifth, the second, and the seventh. These in fact agree best with the New Haven inclination. The third I feel disposed to leave out of consideration. Perhaps the fourth should be likewise discarded. The numbers in the fourth column, refer to the horizon, the third in general to points above it. They are not therefore exactly comparable. The numbers in the fifth and sixth columns appear large, but if we consider only those observations that appear worthy of confidence, I think the accordance is as great as can usually be expected for a daylight meteor. The probable error would be quite small. The errors are pretty nearly the same in excess as in deficiency. The assumed angle of inclination therefore cannot be far from the truth.

The inclination of the meteor's path as actually seen at Alexandria, must have been nearly the same as that resulting from the mean of the observations reported by Mr. Hallowell. The arc passing through the two given positions is at the horizon inclined 11° 34′ to the vertical. The meteor was seen by three persons at Washington, two of whom say it descended vertically.

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The third says it apparently descended to the ground a little north of the point of its first appearance.

The computed trajectory would be seen at Washington inclined 9° 30′ to the vertical circle.

Mr. Nickerson, four miles west of Dover, says the column of smoke was nearly or quite vertical.

Mrs. Cowgill, six or seven miles west of Dover, says that if it varied at all from the vertical, it disappeared to the north of the point where it was first seen.

The inclinations at these two places of the computed trajectory, and the vertical, are 7° and 6½° respectively.

A line but little inclined to the vertical would be easily considered strictly vertical. But one would not readily mistake the direction of inclination. For my present purpose, moreover, an error of several degrees in the inclination at Alexandria would be of little importance.

The length of the visible track of the meteor is the next thing to be computed. Judge Boardman is confident that he saw it as far east as the line of Church street. It was probably visible sometime before he saw it. If S. 29° W. be assumed as the direction in which it was first seen we have a visible arc of about 12°, corresponding on the real line of the meteor to a distance of 36, or more exactly 35·91 miles. The motion was nearly at right angles to the line of vision. One extremity of this line was in N. lat. 39° 13′ 33″, W. long. 74° 24′ at an altitude of 49¼ miles. The other extremity was in N. lat. 39° 13′ 15″, and W. long. 74° 49′ 40″, at an altitude of about 22 miles. After the meteor disappeared to Judge Boardman, it must have passed several miles before it exploded and vanished. Many accounts agree in this, that it disappeared above the horizon. Some say it burst into fragments, each leaving behind a train of light. At Tuckerton, N. J., it disappeared at 10° altitude. Other accounts make me think this not far from the truth. This would make it explode at an altitude of a little less than 2°, as seen from New Haven. It would add 5° to the 12° of the visible arc and about 14 miles to the track of the meteor, making it in all, about 50 miles long from the point assumed, as that first seen by Judge Boardman.

Prof. Loomis, from the reports of several persons who saw the meteor in New York city, says, "the length of its visible path, was variously estimated from 15° to 25°. The entire period of its visibility did not exceed one or two seconds." By most of these observers, both the upper and lower portions of the meteor's path were probably not seen. At first it would not attract attention and it usually disappeared behind buildings. They probably saw in the average nearly the same amount of the meteor's path as Judge Boardman. This at New York city, subtends an arc of about 18°.

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The meteor was no doubt seen at Alexandria much sooner than at New Haven. I am inclined to think that the reported altitude of its first appearance is less correct than the inclination. Taken strictly, the visible arc corresponds to a distance of 522 miles on the meteor's path. But a change of one degree in the place of first appearance might diminish this nearly 80 miles. When first seen at New Haven, it had an altitude of 17° 25′ at Alexandria. If the altitudes had been estimated, and not measured, the 17° 25′ might be considered 40°. But the method employed, that of directing an instrument to that point in the heavens, seems well fitted to give tolerably correct results. Though we cannot determine the distance passed over by the meteor while in view, yet it is evident that Mr. Martin saw it much sooner than Judge Boardman. If we were to allow an error of 15° in the place of first appearance, the length of the visible path of the meteor would be at the least 90·8 miles. If in addition, an error of 5° be allowed in the direction of the meteor's path, the length would not be less than 70 miles. If even the meteor's path was 10°, and the place of first appearance 15° in error, the visible path would be 57 miles.

These last errors seem very much greater than can be allowed. Mr. Hallowell says, "I cannot believe the body could possibly have had a less altitude than 35° at the time it was first seen." Ten degrees change in the direction of the meteor's path would carry it about 8° towards the vertical as seen from New Haven and New York city, a change which I think the observations would hardly allow.

The Washington observers, to some extent confirm the Alexandria observation. One says it appeared first at an elevation of 50°. Dr. Mackie says that the meteor had a luminous train extending vertically 15° to 20°. When first seen, its base was about 30° from the horizon. The point where it was first seen, by Judge Boardman, would have, at Washington, an altitude of 17° 40′.

The *time* of *flight* is the most difficult element to determine. My purpose is not to compute the actual velocity. I wish rather to prove that it was much more than 21 miles per second, when the body entered the atmosphere.

Judge Boardman estimates the time at one second, and says that it could not have been as great as two seconds. He estimated it, by supposing a body to pass with the same velocity over a similar distance, and noting the interval. The person to whom he was speaking, was not able to get a view of the meteor. The most probable velocity from this observation, would be 36 miles: the least velocity over 18 miles a second. The New York city observers, reported by Prof. Loomis, saw about the same amount of the meteor's path, as Judge Boardman. "The entire period of visibility did not exceed one or two seconds." The velocity then

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would be not less than 18 miles a second, and probably it was much greater.

Mr. Mills estimated the time at two seconds. The arc passed over seems to have been 15° or 20°. This would give a velocity of about 18 miles a second, if his estimate of time is correct.

Dr. Mackie of Washington, says: "It was perhaps two seconds in view, for I had time, after seeing it first, to grasp my companion's arm, and point to it, before it disappeared." The altitude of its first appearance being estimated, and not measured, the velocity cannot be easily determined from this observation. But considering how liable an observer is to over-estimate the time of flight, I think the velocity, so far as indicated by his observation, is *much greater* than 20 miles a second.

The time of flight at Alexandria was estimated nearly in the same manner as at New Haven. The velocity which is indicated by this observation depends on the amount of error we can allow in the determination of the point of first appearance. Taken strictly, we have a velocity of 260 miles a second. Though I should not be unwilling to admit such a velocity, if we had valid proof of it, yet the present observation cannot be considered as furnishing it. Allowing a possible error as great as mentioned (p. 190) we have a velocity of 45, or 35, or 28½ miles per second. Only one person, that I am aware of, gives a period of time exceeding two seconds. Mr. Wallis of Salem, Mass., says it was in sight from five to eight seconds.

Besides these specific estimates of time, we have other reasons of greater or less weight for calling it very short.

Mr. Marsh in his paper argues with great reason, that "the extreme shortness of the time occupied in its flight is proved, not merely by the estimates of several observers, but by the failure of people in the vicinity of the explosion to distinguish the source of the sudden flash of light seen by them, and by the impression of even the most distant observers, that it fell very near to them." The latter reason, especially, has much weight.

The light is always called a "flash of light," by some a *sudden* or *instantaneous* flash.

A large number of observers state that they were unable to call the attention of those standing by them to the meteor. It seems that only those looking towards that part of the heavens, saw it.

In a letter dated June 13th, Mr. Marsh says, "all I have since heard from parties I have conversed with tends to confirm the shortest estimates, the impression generally being that it was instantaneous or nearly so."

In reasoning from these data, two considerations should be kept in mind.

1st. The natural tendency is to make the time of flight too great, and hence the velocity, too small.

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2nd. From the moment the meteor entered the atmosphere, it would lose velocity. The resistance which the air offers to so rapid a motion, is enormous. If meteorites be admitted to come in general from meteors, it may be added that they rarely enter the ground more than two or three feet. They do not strike the earth with a velocity at all comparable to that which meteors are known to have, in the higher regions. They lose almost all their velocity in passing through the atmosphere.

A careful examination of all these observations leads me to believe that the actual velocity was as great as 36 miles a second. If we consider the resistance of the air, and then make as large an allowance for errors of observation as can reasonably be made, it seems almost impossible that it could have entered the atmosphere with a velocity less than twenty-one miles. The parts of the earth directly under the meteor, were by the earth's motion in its orbit, and on its axis, moving in a line inclined 89° 31′ to the path of the meteor, with the velocity of 19·023 miles. If the velocity of the meteor in this path was 21, its velocity relative to the sun would then be a little more than $28\frac{1}{4}$ miles. If the meteor had been moving in a parabolic orbit around the sun, it would have had from the combined action of the earth and sun, a velocity of 27·9 miles a second. If, therefore, as I think, can hardly be doubted, the meteor entered the atmosphere with a velocity not less than 21 miles, *it must have been moving in a hyperbolic orbit.*

We have been accustomed to consider the solar system as filled with small planetoids, millions of which, each day, come into the atmosphere, and are burnt up, causing the *shooting stars.* Now we find that we must, in all probability, add one, and no doubt innumerable other similar bodies to the stellar spaces. It opens a new view of creation.

It must not hence be imagined, that the meteors and shooting stars all come from the stellar spaces. The periodicity of the August and November meteors, shows plainly that they are from permanent members of the solar system.

This meteorite did not come from the moon. If we could suppose a lunar volcano to throw out a body with such an enormous velocity, that body must come to the earth, nearly from the direction of the moon. But the moon was at that time about 120° from the direction of the meteor's path.

The recent researches, respecting the transformation of motion into heat, throw some light on the subject of shooting stars. When these bodies come into the atmosphere, the motion they lose is transformed into motion of the air, heat, light, sound, and probably other forms of energy. If it was all transformed into heat, it would be easy to compute the amount due to the loss of a given velocity. If they have a motion of their own, and their directions are subject to no law, it is easily seen that the average

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velocity is much greater than 19 miles a second. A body weighing one pound, and moving 25 miles a second, has momentum sufficient to raise $(25\times5280)^2\div2g=271{,}500{,}000$ pounds one foot. By Joule's equivalent the raising of 772 pounds one foot, corresponds to the heat necessary to raise one pound of water one degree Fahrenheit. If the capacity of the meteoric substance for heat is 0·2, (that of iron is 0·12,) the loss of a velocity of 25 miles would be equivalent to heating $(271{,}500{,}000\div0{\cdot}2)\div772=1{,}760{,}000$ pounds of the substance one degree Fahrenheit, if the whole of the motion was transformed into heat. A very small fraction of this heat would doubtless suffice to burn up, or dissipate, any substance whatever.

It is often urged, that the shooting stars cannot be solid bodies, since of the millions that daily enter the atmosphere, so few come to the ground. The above calculation shows that the heat generated may be ample to vaporize or dissipate them.

The shooting stars need not in general be large bodies. The apparent size is due to irradiation, and indicates, not amount of matter, but rather amount and intensity of light. Thus the stars though often spoken of as mere points have disks. The diameter of stars of the first magnitude was estimated at 2′ by Tycho Brahe. The telescope has shown that this disk is spurious. If these stars are equal in size to the sun, Tycho's estimate makes their diameters 50,000 times too great.

It has been estimated that the light of the sun's surface is four or five times as great as that of the same surface of the lime in the calcium light. It is also estimated that the light of the sun is 20,000,000,000 times that of Sirius. A simple calculation shows that an inch globe as brilliant as the calcium light, would give at over 100 miles distance as brilliant a light as a star of the first magnitude. The estimates which are used as the basis of calculation are confessedly very vague, yet they show that a very small body may furnish as much light as a shooting star. Such a body would naturally burn up without passing through the atmosphere.

I can therefore see no reason, as some persons do, to make a marked distinction between the different classes of meteors. Those which furnish meteorites, those which explode with a loud report, and those of all degrees of brilliancy which are not heard to explode, all seem to belong to one class, and to differ from each other no more than substances on the earth. That some are solid and others aëriform is not impossible. Differences of chemical constitution, size, velocity, and orbit exist, and these may account for the variety of appearance.

Note.—Since the above was in type, the meteor of July 20 seems to furnish better data for proving that meteors sometimes come from the stellar spaces.

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ART. XVIII.—*Crystalline form not necessarily an indication of definite Chemical Composition: or, on the possible variation of constitution in a mineral species independent of the Phenomena of Isomorphism.* By JOSIAH P. COOKE, Jr., A.A.S., Professor of Chemistry and Mineralogy in Harvard College.*

IN a memoir presented to the American Academy of Arts and Sciences in September, 1855,† I described two new compounds of zinc and antimony which I named *stibiolzincyle* and *stibiotrizincyle*, on account of their analogy in composition to the metallic radicals of organic chemistry. The symbols of these compounds are $Sb\,Zn^2$ and $Sb\,Zn^3$; and they are distinguished by the high perfection of their crystalline forms, the last being still further characterized by a most remarkable property of decomposing water quite rapidly at 100° C. I stated in the same memoir that crystals of these two compounds could be obtained containing proportions of zinc and antimony differing very widely from those required by the law of definite proportions; and I also traced out the relation between the composition of the crystals, and that of the menstruum in which they are formed. It is my object in the present paper to consider the bearing of these facts, already fully described, on the idea of mineral species, and to offer a few suggestions which I hope may be of service in determining the true chemical formulæ of many minerals, and thus in simplifying the science of mineralogy. But in order to render myself intelligible, it will be necessary to recapitulate very briefly the facts in question, referring to the original memoir for the full details.

The crystals both $Sb\,Zn^2$ and $Sb\,Zn^3$ can be obtained with great readiness. It is only necessary to melt together the two metals in the atomic proportions, and when the metals are fully alloyed, to proceed exactly as in crystallizing sulphur. The melted mass is allowed to cool until a crust forms on the surface, which then is broken, and the liquid metal remaining in the interior poured out. On subsequently breaking the crucible, the interior is found lined with magnificent metallic crystals, which, when not tarnished by oxydation have a silver-white lustre. In the course of my investigations on these compounds, crystallizations were made, or attempted, of alloys, differing in composition by one half to five per cent, according to circumstances, from the alloy containing 95 per cent of zinc, to that containing 95 per cent of antimony; *but only two crystalline forms were observed*, that of $Sb\,Zn^2$ and that of $Sb\,Zn^3$. The crystals of the

* Communicated by the Author.

† Transactions of the American Academy of Arts and Sciences, New Series, vol. v, p. 337. This Jour. [2], xx, 222.

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two compounds both belong to the trimetric system; but they differ from each other, not only in their crystallographic elements, but also in their whole "habitus." Stibiotrizincyle crystallizes in long acicular prisms, which group themselves together into larger prismatic aggregates; while stibiobizincyle crystallizes in broad plates, which twin together on an octahedral face, and form a very characteristic cellular structure. This very striking difference in the character of the crystals proved to be an important circumstance in the investigation, as it enabled me to distinguish with certainty between the two compounds, even when the faces of the crystals were so imperfect that a measurement of angles was impossible.

The most remarkable result of the investigation, and the one to which I wish to direct especial attention, is the fact that each of the two crystalline forms was found to be constant under very wide variations in the per-centage composition of the crystals. As this is a point of great importance, it will be necessary to enter more into detail, considering in the first place the crystals of $Sb\,Zn^3$. The crystals of this compound are obtained in the greatest perfection from an alloy containing the two metals in just the proportions represented by the formula, namely, 42·8 parts of zinc, and 57·2 parts of antimony. They are then comparatively large, generally aggregated, and, as the three analyses cited in the accompanying Table indicate, they have the same composition as the alloy.

Composition of the alloy by synthesis.		Composition of the crystals by analysis.		
Per cent of Zn.	Per cent of Sb.	Per cent of Zn.	Per cent of Sb.	Sum.
42·80	58·20	43·15	56·93	100·08
"	"	43·06	56·50	99·56
"	"	42·83	57·24	100·07

On increasing gradually the amount of zinc in the alloy up to 48·7, the crystals continued to have the composition of the alloy; and the only difference which could be observed in their character was that they were smaller, and more frequently isolated. Between these limits the whole mass of the alloy exhibited a strong tendency to crystallization; and by pouring it, as it cooled, from one vessel to another, it could be crystallized to the last drop. On increasing the amount of zinc in the alloy to 50·7 per cent, the amount of zinc found in the crystals was uniformly less than it was in the alloy; but no closer relation between the two could be detected, owing, undoubtedly, to the unavoidable irregularity in the crystallization of the alloys which contained more than 50 per cent of zinc. This arose from a peculiar pasty condition which the liquid mass assumed at the point of crystallization. Definite crystals, however, were obtained from an alloy of 60 per cent zinc containing 55 per cent; above this the crys-

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tals became less and less abundant, and gradually faded out, although the alloy of 86 per cent of zinc exhibited a radiated crystalline texture; and a trace of this structure could still be discovered even in the alloy containing only 4 per cent of antimony. It was very interesting to trace the gradual fading out of the crystalline structure, as the character of the phenomenon was entirely analogous to that which may be noticed in many crystalline rocks.

Finding that the crystalline form of $Sb\,Zn^3$ was constant under so great an increase of the proportion of zinc in the crystals, it might be supposed that, on returning to the alloy of 42·8 per cent of zinc and increasing the amount of antimony, we should obtain crystals containing an excess of antimony; but so far is this from being true, that the slightest excess of antimony entirely changes the character of the crystallization. On crystallizing an alloy containing 41·8 per cent of zinc, not a trace of any prismatic crystals could be seen; but in their place there was found a confused mass of thin metallic scales, which, as will soon be shown, are imperfect crystals of $Sb\,Zn^2$. Thus it appears that, although perfectly formed crystals of $Sb\,Zn^3$ can be obtained containing 55 per cent of zinc (that is, 12 per cent above the typical proportions), they cannot be made to take up the slightest excess of antimony.

Let us pass now to the crystals of $Sb\,Zn^2$. In order to obtain crystals having the exact typical constitution, it was found necessary to crystallize an alloy at least as low as 31·5 per cent of zinc. At this point large compound crystals are obtained corresponding to the large crystals of $Sb\,Zn^3$; and the same was true of alloys down to 27 per cent of zinc. Between these two limits (namely, alloys of 31·5 and 27 per cent of zinc) the crystals formed were found to have the theoretical composition of $Sb\,Zn^2$, indicating of course a tendency towards this point; but on increasing or diminishing the amount of zinc in the alloy beyond these limits, the composition of the crystals immediately began to vary in the same direction as that of the alloy. The crystals of $Sb\,Zn^2$ containing an excess of zinc are smaller and more frequently isolated than those having the exact theoretical composition. A similar fact, it will be remembered, is true of the crystals of $Sb\,Zn^3$.

At the alloy of 33 per cent of zinc, the definite crystals of $Sb\,Zn^2$ begin to disappear, and are succeeded by thin metallic scales, which are obviously imperfect crystals of the same form. This was established, not only by the obvious law of continuity noticed in the different specimens (the perfect crystals gradually passing into the scales), but also by the peculiar mode of twining, which was the same with the scales as with the large crystals, forming the peculiar cellular structure already referred to. Moreover, the angle between two scales thus united was found

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to be equal to the basal angle of the perfect crystals, at least as nearly as could be measured. These scales continue up to the alloy of 41·8 per cent of zinc, becoming, however, less abundant and less distinct. Several specimens of them were analyzed; but no regularity could be detected in their composition, except that they all contained a much larger amount of zinc than the alloys in which they were formed.

Crystals of $Sb\,Zn^2$ containing an excess of antimony were readily obtained from alloys containing less than 27 per cent of zinc. They became more and more imperfect as the excess of antimony increased, and finally faded out altogether in the alloys below 20 per cent of zinc. It is evident, therefore, that definite and perfect crystals of $Sb\,Zn^2$ can be obtained with a large excess either of zinc or antimony above the theoretical composition. It is also evident that, of the two compounds, $Sb\,Zn^2$ is the most stable,—first, because it is formed to the exclusion of $Sb\,Zn^3$ in all alloys containing less zinc than the amount corresponding to the typical composition of the last compound; and secondly, because the crystals retain the typical composition under quite a wide variation (viz. between 31·5 and 27 per cent) in the composition of the alloy.

The facts above stated are fully illustrated by the following Table, which gives the results of a large number of analyses of crystals of both compounds formed in alloys containing different proportions of the two metals:—

Analyses of the Crystals formed in the Alloys of Zinc and Antimony.

Stibiotrizincyle.					Stibiobizincyle.				
Composition of the alloys by synthesis.		Composition of the crystals by analysis.			Composition of the alloys by synthesis.		Composition of the crystals by analysis.		
Per cent of Zn.	Per cent of Sb.	Per cent of Zn.	Per cent of Sb	Sum.	Per cent. of Zn.	Per cent of Sb.	Per cent of Zn.	Per ce t of Sb.	Sum.
70·40	29 60	64·15	35·77	99·92	33·00	67·00	35·37	64·57	99·94
66·50	33·50	61·00	39·00	*100 00	33·00	67·00	35·40	64·60	†100·00
64·50	35·50	53·50	41·44	99·94	32·50	67·50	34·62	64·92	99 54
....	....	55·49	44·42	99 91	32·50	67·50	34·61	65·39	†100·00
60·60	39·40	55·00	45 09	100·09	31·50	68·50	33·95	66 09	100·04
58·60	41·40	50·39	49·29	99·68	29·50	70·50	33 62	66·38	†100·00
56·60	43·40	49·92	50 05	99·97	29·50	70·50	33·62	66·38	†100 00
54·70	45·30	48·26	51 42	99 68	27·50	72·50	33·85	65·81	99 66
52·70	47·30	47·47	52·53	†100·00	26·50	73·50	32·08	67·60	99·68
....	....	....	....	....	26 00	74·00	30·74	69·06	99·80
50·70	49·30	[illegible]	53·11	†100·00	25·50	74·50	30·43	69 51	99·94
50·70	49·30	46·45	53 55	†100·00	25·00	75·00	29·88	70·20	100 08
48·70	51·30	46·66	51·34	†100·00	24·50	75·50	28·76	71·24	100·00
46·70	53·30	46·77	53·23	†100·00	23·50	76·50	27·93	71·85	99·78
44·80	55·20	44·26	55·73	†100·00	22·50	77·50	26·62	73·27	99 89
43·80	56·20	44·04	55·96	†100·00	21·50	78·50	24·83	74·74	99·57
42·80	58·20	43·15	56·93	100·08	20·12	79·88	20·58	79·42	100·00
42·80	58·20	43 06	56·50	99·56					
42·80	58·20	42·83	57·24	100·07					

* In this analysis the antimony only was determined.
† In this analysis the zinc only was determined.

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The relation between the composition of the crystals $Sb\,Zn^2$ and that of the alloy in which they are formed, is discussed at length in the memoir already referred to. It is there shown to be a very simple function of the mass of metal which is in excess in the alloy, and of the force which determines the union of the elements in definite proportions. The whole order of these phenomena seem to the Author to point to the existence of a power in the mass of metal which is in excess in the alloy, to disturb the action of the force, whatever it may be, which tends to unite the elements in definite proportions. There is, in the first place, a strong tendency in the elements to unite and form crystals having the exact typical composition; and secondly, this tendency is only overcome by a certain excess of either metal in the alloy. Then, again, the crystals of one compound obviously interfere with those of the other. This certainly has the appearance of one force interfering with the action of another,—the force of mass (if I may so call it) perturbing the action of the chemical force. But it is not my object at present to enter into a discussion on the cause of this variation. Moreover, since such a discussion must be based on purely hypothetical grounds, we could not expect to arrive at any definite conclusion. The facts will be viewed differently according to the theory which may be adopted in regard to that long-controverted subject, the essential constitution of matter. Leaving, however, all theoretical considerations aside, there are certain practical bearings of the *observed facts* on the science of mineralogy which are of immediate application.

Here are two beautifully crystallized products, as well crystallized as any that occur in nature, and yet the different specimens of the crystals differ from each other so widely in composition that any single analysis might lead to an entirely erroneous conclusion in regard to the general formula of the substance. Were a chemist to analyse accidentally solely the crystals obtained from an alloy containing 58·6 per cent of zinc, he would at once determine that the formula of the compound was $Sb\,Zn^4$; and by a like accident he might be led to any other formula between this and $Sb\,Zn^2$: in fact, by an analysis of a number of specimens of needle-shaped crystals obtained from alloys of copper and tin, Rieffel was led to several just such improbable formulæ; and in my own investigations it was not until I had analysed a whole series of crystals, that the real nature of the phenomena became apparent, and the true constitution of the compounds determined. If, then, such great variations in composition are compatible with a definite crystalline form in these furnace products, may not similar variations occur in the crystalline minerals formed in nature?

It is not necessary to make an extended investigation in order

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to answer this question; for the materials at our hands are sufficient to give us a satisfactory reply.

There is a compound of antimony and silver called discrasite, which occurs in many localities crystallized in trimetric prisms homœomorphous with $Sb Zn^3$. The formula of the mineral is therefore probably $Sb Ag^3$, which would require 71·5 per cent. of silver; but the per cent as given by analysis varies between 75·25 and 78 per cent, and one analysis gives the per cent as high as 85. Further analyses of this mineral are required in order to determine its constitution, but there can be no doubt that it varies in composition like $Sb Zn^3$.

Silver-glance is another highly crystalline mineral. Theoretically it should contain 87·1 per cent of silver and 12·9 per cent of sulphur; but in a specimen analysed by Klaproth, the proportions were 85 and 15.

Again, the analyses of pyrrhotine (magnetic pyrites) give results varying between 38·78 per cent sulphur, 60·52 per cent iron (variety from Bodenmais), and 43·63 sulphur, 56·37 iron (variety from Barèges). The constitution of the mineral is still uncertain; but its true formula is probably Fe S, which would require 36·4 per cent sulphur and 63·6 per cent iron. Lastly, the analyses of antimony-glance give results varying between

Antimony 74·06,	and	Antimony 73·5,
Sulphur 25·94,		Sulphur 26·5.

The true formula of this mineral is undoubtedly $Sb S^3$, which would require only 72·88 per cent of antimony.

Similar examples might be greatly multiplied. Those just cited were selected at random from the first few pages of Dana's 'System of Mineralogy.' They are all examples of binary compounds which occur almost chemically pure in nature; so that the phenomena in question are not complicated by those of isomorphism.

When we pass to minerals of more complex constitution, the same phenomena can be made evident, although not quite so easily, on account of the introduction of the phenomena of substitution by isomorphous elements. It will not, however, be necessary for me to cite examples; for it is a fact perfectly well known to all mineralogists, that, after making allowances for the substitution of isomorphous elements, the various analyses of such minerals as mica, hornblende, garnet, and tourmaline differ very greatly from each other,—a difference, moreover, which no mere error of analysis will explain, and which must therefore be referred to an actual variation in composition. In the silicates this variation in composition is made evident by the variation of what is termed the "oxygen ratios;" and it is well known to mineralogists that in many species this variation is very large.

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For example, in mica the following ratios between the oxygen in the base and acid have been observed in merely the Muscovite variety:—13 : 16, 13$\frac{1}{3}$: 16, and 14$\frac{2}{3}$: 16; and similarly wide variations might be pointed out in other well known species. It is in consequence of such variations as these that the general chemical formulæ of some of the best known mineral species, such as mica and tourmaline, are still uncertain; and in other cases, where the true formulæ is probably known, the constitution of the mineral has been determined quite as much from other considerations as from the chemical analyses.

Sufficient has been said, I think, to show that variations in composition similar to those which I have observed in zinc and antimony occur in many minerals; and I trust that the results of my investigation will serve to throw light on this whole class of phenomena, which have so greatly perplexed mineralogists, and rendered all strictly chemical classifications of mineral species so unsatisfactory. This investigation has shown that a definite crystalline form is compatible with quite a wide variation of composition, and has in this way pointed out an explanation of the variation observed in the mineral kingdom. But more than this, the investigation has also indicated a method by which, amidst all this variation, the true constitution of the mineral can be determined.

In the compounds of zinc and antimony, although the definite crystalline form was compatible with a wide variation in the proportions of the constituent elements, yet the point corresponding to the typical composition was marked by several unmistakeable properties, which clearly enough indicated the true formulæ of the compounds. These properties are discussed at length in my original memoir, and need therefore only to be alluded to in this connection.

It has already been stated that the crystals, both of $Sb\,Zn^3$ and $Sb\,Zn^2$, having the theoretical composition are, as a rule, larger and more generally aggregated than those containing an excess of either metal. Moreover, in $Sb\,Zn^2$ the general character of the crystals appears to be modified by the change of composition, although the crystallographic elements remain the same. Thus in the crystals having the theoretical composition, the octahedral planes are greatly developed, giving to the crystals the general appearance of a truncated octahedron.* But as the crystals take up an excess either of antimony or zinc, the basal planes become more and more dominant, and the crystals are at last reduced to thin plates. In fact, so marked are these changes, that, after a little experience, a person could tell the approximate composition of the crystals from their general appear-

* See figure accompanying my original memoir.

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ance. Similar changes in the appearance of many minerals are familiar to the mineralogist. They are seen in calcite, heavy spar, Anglesite, and others, and may serve as guides in tracing variations of composition.

Again, the specific gravity of the crystals, both of $SbZn^2$ and $SbZn^3$, was taken with great care through the whole series, and the results are tabulated below. The union of the two elements is attended with an increase of volume, and this increase is at a maximum at the points corresponding to the theoretical composition. These points would therefore be marked in a set of crystals by being points of minimum specific gravity; and they could be determined with great accuracy by means of this property, even in a series of alloys of the two metals which had not been crystallized. This fact is illustrated by the following Table, reprinted from the original memoir.

Specific Gravities of Crystals formed in the Alloys of Zinc and Antimony.

Composition of the alloys.		Composition of the crystals.		Spec. grav. of crystals by experiment.	Mean spec. grav. of zinc and antimony.	Expansion in crystallizing.
Per cent of Zn.	Per cent of Sb.	Per cent of Zn.	Per cent of Sb.			
10 00	.....	....	....	7·153	7·153	0·000
*96·00	4·00	....	....	7·069	7·133	0·064
*86 20	13·80	....	....	6 898	7·082	0·184
*76·30	23·70	....	....	6·769	7·032	0 263
70·40	29 60	64·20	35·80	6·699	6 975	0·276
66 50	33 50	61·00	39·00	6·628	6·959	0·331
64 50	35 50	58·56	41·44	6·596	6·948	0 352
62 50	37·50	55·53	44·47	6·506	6·933	0·427
60·60	39·40	55 00	45·00	6·440	6·931	0·491
58 60	41·40	50 39	49·61	6·396	6 909	0·513
56·60	43·40	49·95	50·05	6·388	6·906	0·518
48·70	51 30	48·66	51·34	6·404	6·900	0·496
46·70	53·30	46 77	53·23	6·376	6·891	0·515
44·80	55·20	44·26	55·74	6 341	6 879	0·538
†42·30	57·20	43·09	56·91	6·327	6·874	0·547
*40·00	60·00	....	....	6 386	6·860	0 474
*35·00	65·00	....	....	6 404	6·837	0·433
33 00	67 00	35·37	64·63	6·401	6·838	0·437
‡29·50	70·50	33·62	66·38	6·384	6 830	0·446
‡27·50	72·50	33·85	66·15	6·382	6 831	0·448
26·50	73 50	32 08	67·92	6·400	6 822	0·422
26·00	74 00	31·07	68·93	6·418	6·818	0·400
25·50	74·50	30·43	69 57	6 428	6 816	0·388
24·50	75·50	28·76	71·24	6·449	6 807	0·358
22·50	77·50	26·62	73 38	6·453	6·798	0 345
21·50	78·50	24·83	75·17	6·467	6·790	0·323
*15 00	85 00	....	....	5 564	6·744	0·180
*10 00	90 00	....	....	6·603	6 721	0·118
*5 00	95 00	....	....	6 655	6·698	0 043
....	100·00	....	....	6 677	6·677	0 000

* Alloys not crystallized. † Point of typical composition of $SbZn^3$.
‡ Point of typical composition of $SbZn^2$.

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The point of typical composition in the case of the crystals of $SbZn^3$ was still further marked in a most decided manner by a very remarkable property. It has already been stated that this compound has the power of decomposing water with rapidity at 100° C.; but this is true only of those crystals which have approximately the theoretical composition. During the course of my investigation I determined the quantity of hydrogen evolved by alloys of different composition during a given time, taking care, of course, that the circumstances should be the same in all cases; and I found that with the alloy containing 43 per cent of zinc, there is an immense maximum, confined at most between 2 per cent on either side, the alloy of 43 per cent yielding over nine times as much gas as an alloy of 50 per cent, although the crystals of the last were fully as definite as those of the first.*

It is evident from the above facts, that the points corresponding to the theoretical composition of the two compounds of zinc and antimony, are also *points of maxima and minima* of various properties. Now I have no doubt that the same truth will be found to hold in the mineral kingdom. In a mineral like tourmaline or mica, for example, the specimen having the exact theoretical composition may probably be discovered by examining a large number of specimens, and discussing their various physical properties. All the physical properties may be of value in this connection, such as lustre, hardness, specific gravity, specific heat, &c.; and no mechanical rules can be laid down. Much must depend on the discretion of the observer; and in any cases such properties will be selected as are best adapted to the circumstances of the case. In comparing different crystals of the same mineral, it is obviously important to select such as have been formed in a different matrix; for it is only with such that we should be led to expect great variations of composition. It is also evident that the phenomena would be complicated when there has been a substitution of isomorphous elements; and until the effect of such substitution on the physical properties can be traced, it will be necessary to select specimens of as uniform a constitution as possible.

With one other consideration I will close this paper. The principle which has been here discussed must modify materially our notion of a mineral species. The idea of a mineral species has hitherto involved chiefly two distinct characters:—first, a definite crystalline form; second, a constant general formula; and any important variation in either of these characters has been regarded as equivalent to a change of species. Rutile and anatase are regarded as different species, because their crystalline forms are slightly different, although both minerals have identically the same constitution; and again, magnetite and Franklin-

* See Table in the memoir before cited.

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ite, which have the same form, are regarded as different species, because they have a slightly different composition. It is true that the actual composition of a mineral may vary very greatly by the substitution of isomorphous elements, and yet, if the general formula remains constant, the species may not be changed. But the extent to which such substitution can be carried without changing the species is not so well settled among mineralogists as could be desired, and the same rule is not applied to all species. The difference between the *varieties* of garnet, for example, is as great as that between the *species* magnetite and Franklinite. Leaving, however, this point undetermined, all mineralogists have agreed that any *essential change* in the *general formula* was inconsistent with the idea of the *same species*. The result, however, of my investigation is to show that the general formula of a mineral species may vary also, or, as I should rather say, the *general formula* is not necessarily the actual formula of each given specimen, but only the *typical formula* of the species towards which the mineral tends, and which it would unquestionably reach if it could be several times recrystallized.

According to this view, the general formula represents not the *actual constitution* of the mineral, but only a certain *typical composition*, which perhaps is never realized with any actual specimen. The fact that the composition of a mineral species may be modified by the substitution of isomorphous elements, was first established by Mitscherlich, and has long been an admitted principle in mineralogy. We must now, as I think, still further expand our idea of a mineral species, and admit that its composition may be modified by an actual variation in the proportions of its constituents. Thus it is that in mineralogy, as in other sciences, we are led to admit the truth of that maxim which every advance in true knowledge seems to verify, "Natura non facit saltus."

While the results of my investigations thus serve to render the idea of a mineral species less definite than before, I cannot but hope that they will tend ultimately to simplify the whole subject of mineralogy; for not only may we expect to reduce the number of mineral species, but also, by simplifying the general formulæ of those which remain, to classify the whole with a greater precision than is now possible. To do this, however, implies a careful revision of the whole subject-matter of mineralogy on the principles above given,—a labor of which few can appreciate the extent, except those who are familiar with the methods of physical research. The work cannot be done by any one person; and it is the chief object of the present paper to call the attention of mineralogists to the importance of the subject.

I have not thought it necessary to dwell in this paper on the obvious distinction between the phenomena here in considera-

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tion, and those of isomorphism. It was shown in my previous memoir, that the variation in the composition of the crystals of $Sb Zn^3$ and $Sb Zn^2$ could not be explained by this principle; and the distinction between the two classes of phenomena has been still further illustrated by a recent investigation on the crystals formed in alloys of copper and zinc, made in my laboratory by Mr. F. H. Storer. These crystals, which are undoubtedly mixtures of isomorphous elements, give no indications whatever of points of typical composition,—thus illustrating not only the characters of an isomorphous mixture, but also the distinction between such a mixture and a true chemical compound. Admitting, then, the possibility of a variation of composition in a mineral species, independent of the phenomena of isomorphism, it becomes of importance to distinguish this new class of phenomena by a separate term; and I would propose for this purpose the word *Allomerism*. By this word I would designate *a variation in the proportions of the constituents of a crystallized compound without any essential change in the crystalline form*. If, then, we also use the word *typical* to indicate the condition of *definite composition*, we may speak of those specimens of a mineral species which contain an excess of one or the other constituent, as *allomeric variations from the typical composition*. The degree of allomerism would then be measured by the excess of the *allomeric constituent* above the typical composition. Thus the crystals of $Sb Zn^3$ containing 42·3 per cent of zinc would be said to have the typical composition; while those containing 55 per cent of zinc would be distinguished as an allomeric variety, the degree of allomerism in this instance amounting to 12 per cent, and zinc being the allomeric constituent. In the case of the mineral Discrasite, it is probable that no specimen having the typical composition has yet been analyzed. Those specimens whose analyses are given in Dana's 'System of Mineralogy,' are all probably *allomeric varieties* of the mineral, silver being the allomeric constituent, and the degree of allomerism varying from 4 to 7 per cent. It is unnecessary, however, to multiply examples, as the above are sufficient to illustrate the use of the term.

ART. XIX.—*Notices of several American Meteorites;* by CHARLES UPHAM SHEPARD.

1. *Nebraska Iron.*—This very interesting mass, first noticed in a late number of the Proceedings of the St. Louis Academy of Sciences, was brought to St. Louis by the fur traders in the employ of C. P. Chouteau, Esq., about two years ago, and by him presented to the museum of the Academy. It was found near the Missouri River, between Council Bluff and Fort Union. It

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originally weighed about thirty-five pounds, but is now reduced to twenty-nine. Its shape was an oblong, compressed oval, not unlike that of the Chesterville, South Carolina, iron-mass, which has been compared by me to the form of a thick, blunt edged fresh water clam (*Unio*). Its surface is as black and smooth as that of the Braunau Iron, from which however it differs in being more even and smooth, though it is not destitute of the usual indentations belonging to meteorites, but these are by no means uniform in their occurrence over the entire surface. The crust is everywhere extremely thin, amounting to scarcely more than a mere varnish; and what is very remarkable, is often insufficient to hide the Widmannstättian figures with which the body must have been covered before it entered our atmosphere. The lines are not equally displayed throughout, and indeed will generally require a single lens in order to be distinctly seen. Nor have they the same beautiful regularity as when obtained by etching upon a polished surface from the interior. They are moreover curiously knotted, so as to resemble, under the microscope, the blunted teeth of a fine saw blade. The configuration upon the etched plates of this iron resembles slightly that of the Texas mass, though the bars are much more rectilinear, and in this respect approach nearer to the new African irons described by me (those from Namaqua Land and Orange River). I observe, however, in this as well as in most other irons, that the fullest regularity of internal structure does not prevail, until some little depth from the outer surface or crust is reached. The Nebraska iron is quite free from earthy, plumbaginous or pyritic matter. Prof. Litton, of the St. Louis University, has recognized in it the presence of nickel; and is understood to be now engaged with its complete analysis. Its specific gravity is 7·735. The character of the surface renders it certain that this mass must be of very recent fall. I am indebted to the liberality of the Academy, and the kind offices of Nathaniel Holmes, Esq., its Secretary, for a fine slice of the mass, from which I have been enabled to make the foregoing observations.

2. *Forsyth* (Taney County, Missouri) *Iron.*—My first information of this locality was derived, while on a visit to southeastern Missouri in April last, from N. Aubushon, Esq., of Ironton. He stated that a small specimen of curiously knitted, malleable ore, of a white color resembling silver, had been sent him two or three years ago by a person residing near the locality. Mr. Aubushon forwarded it to an assayer at Ducktown, Tenn., from whom he learned that it was composed of iron and nickel. On visiting St. Louis soon after, I was informed by Prof. Swallow, the State Geologist, that a specimen had also been transmitted to him by letter from the same place; and that Prof. Litton had found it to be composed of similar constituents. Prof. Swallow

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presented me a small fragment of his specimen, upon which I am able to offer a few remarks, awaiting the results of Prof. Litton's analysis, for fuller information.

The mass evidently belongs to the rather rare group of amygdaloidal meteoric irons, in which, like those of Steinbach (Saxony) and Hainholz (Westphalia), the peridotic ingredient preponderates over the nickelic iron. Its specific gravity is 4·46. The iron is remarkable for its whiteness, while the peridot is of a well marked green color, and distinctly crystalline. No pyrites is visible in the very small fragments examined. It is reported that two considerable masses of this meteorite were found buried in the soil upon a hill-side; and that they are at present secreted under the belief that they contain silver.

3. *Bethlehem* (New York) *Meteoric Stone*, of Aug. 11, 1859.—The only stone found from the great explosion heard over a large district of northwestern Massachusetts, and extending into the state of New York as far as ten miles west of the cities of Albany and Troy, was the little fragment, less in size than a pigeon's egg, of which an outline is here subjoined.



I am imdebted to David A. Wells, Esq., the editor of the American Scientific Annual, for several interesting particulars concerning its discovery and properties. He was good enough to visit, at my request, the residence of Mr. Garritt Vanderpool (situated seven miles from Albany and one mile west of Bethehem church), where the stone fell, and to ascertain on the spot the facts respecting its descent. Mr. Vanderpool was at work near his house, and heard the explosion in common with other members of his family. About two minutes after, as it appeared to him, a stone, coming in an oblique course, struck the side of a waggon-house, glanced off, hit a log upon the ground, bounded again, and rolled into the grass. A dog lying in the doorway of the waggon-house sprang up, darted out and seized it, but dropped it immediately, probably on account of its warmth and sulphureous smell. Mr. Wells had two opportunities of inspecting the stone before it was sold to the State Cabinet in Albany. It was far from being entire when first picked up, no doubt having been broken by its contact with the house. On the second inspection, he noticed that one corner had been broken away, and other portions much marred through the use of knife blades upon its surface by the curious, who, in this rude way, had been led to investigate its peculiarities. About "one-half of it however," he observes, "is covered with the peculiar dark colored crust of meteorites, and has a burnt appearance. This is so well marked that it at once establishes its identity as a meteoric stone. The other sides present-

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ing the appearance originally bright and of a fresh fracture were clear, but are now soiled from handling. The color is a light steel-grey, with metallic particles interspersed. The structure is granular."

Through the recommendation of His Excellency, Gov. Morgan, to the officers having in charge the state cabinet, a small fragment of the stone including a portion of the crust, was most obligingly transmitted to me by Mr. Woolworth, accompanied by the following note:

"Albany, Nov. 11, 1859.

Prof. CHARLES U. SHEPARD,

Dear Sir:—I am directed by Gov. Morgan, as Chairman of the Committee of the Regents of the University on the State Cabinet of Natural History, to send you the inclosed portion of the aërolite lately found near this city. The Committee had hopes of finding other parts of the stone than the one first discovered, but have not been successful. They regret they cannot send you more, but could not do so without destroying the specimen they possess. Hoping it may be sufficient for your purposes, I am, very truly, yours, &c.,

J. B. WOOLWORTH, *Sec'ry, &c.*

I am likewise much indebted to Henry A. Homes, Esq, the State Librarian, for his good offices in facilitating my early acquisition of the specimen which enables me to compare it with those I possess from other localities.

The crust of the Bethlehem stone is very peculiar. It is double the thickness of any in my collection, equalling that of thick pasteboard. It is perfectly black, and very open in its texture. The outer surface is rough, being nowhere perfectly fused, but only semi-vitrified. Without being fragile or carbonaceous, it nevertheless resembles in color, lustre, and porousness, certain surfaces of mineral charcoal. The interior of the stone is equally peculiar, being loosely granular, the particles being uniform in character, small, highly crystalline, and nearly transparent. They possess a brilliant lustre, a very light grey or greenish white color. They resemble volcanic peridot more than any species of the augitic or feldspar family. Nickelic iron, of a bright white color, in delicate filaments and semi-crystalline grains, is thickly diffused through the mass; and these grains, as well as those of the peridotic mineral, are flecked with brilliant points of pyrrhotine (FeS). The specific gravity is 3·56. In general color and effect to the eye, it approaches nearest to the Klein-Wenden stone (Sept. 16, 1843); but it differs from this in being larger grained, and looser in its texture.

4. *Remarks upon the Ohio stones of May* 1, 1860.—Through the much valued assistance of Prof. J. L. Smith, the large 53-pound stone that fell near the house of Mr. Wm. Law of New Concord, forms part of my meteoric cabinet. Without attempting at present a complete description of its form and character,

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I will only offer a few remarks upon the relationship of the Ohio meteorites to those of other falls. In its internal aspect it approaches the stone of Jekaterinoslaw, Russia (1825), though it is somewhat firmer and more compact. In crust, the two are identical. It is also similar to the stone of Slobodka, Russia (Aug. 10, 1808); and compares closely with those of Politz (Oct. 13, 1819), of Nanjemoy, Maryland (Feb. 10, 1828), and of Kuleschowka, Russia (March 12, 1811); but the crust is less smooth on the Ohio stone than in that of the latter.

A pearl grey peridot forms the chief constituent (above two-thirds) of the stone. This mineral is often rolled up into obscurely formed globules, which are so firmly imbedded in the more massive portions of the same mineral, as to be broken across on the fracture of the stone, which thereby presents a sub-pisiform appearance. Snow white particles of Chladnite are thickly scattered in mere specks through the mass, and closely incorporated with the peridot. The nickelic iron, of a bright white color, is also everywhere thickly interspersed in little points. Pyrrhotine is less conspicuous, though often visible in rather broad patches; while black grains of chromite are easily distinguishable by the aid of a glass, and sometimes with the naked eye.

The crust is of medium thickness, and the usual wavy and pitted impressions are also strictly characteristic of these stones. Their origin in meteorites generally, is perhaps still obscure, but may be conceived to originate in the flaking off of fragments in consequence of the sudden transition from cold to hot, which must happen to bodies coming instantaneously from a temperature far below zero into a state of vivid incandescence, at least upon their immediate surface. We see a somewhat analogous flaking up from heated surfaces of granite blocks during a conflagration, when wetted by cold water; though in the latter case, as might be expected, convexities take the place of concavities.

5. *Supposed Fall of a Meteoric Stone in Independence County, Iowa, during the summer of* 1857.—I casually learned while recently in Missouri, that a stone fell at a place called Pilot Grove, near the stage road, in or near the month of August, 1857. The stone was preserved; and I am not without hopes of obtaining a portion of it, having heard of its exhibition during the last year before the Academy of Sciences at Chicago.*

* *Detection of Phosphorus in the native steel of Montgomery (Vermont), and in the Waterloo (New York) Meteoric stone:*

I have examined the first named substance chiefly with a view to determine its relationship to the Rutherfordton (N. C.) Ferrosilicine, (see the September number of this Journal for 1859), and find that while it is free from silicon, it nevertheless abounds in phosphorus. The Waterloo stone, whose resemblance is so great to a well burnt Bristol brick, gives a very decided test for phosphoric acid. The problematical steel from Bedford County (Pennsylvania) is free both from silicon and phosphorus.

New Haven, July 1, 1860.

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ART. XX.—*Influence of Arsenious Acid upon the Waste of the Animal Tissue.*

ACCORDING to experiments made by Prof. Schmidt and Dr. Stuerzwage of Dorpat,* arsenious acid when introduced into the circulation, occasions a considerable diminution of the ordinary waste of the tissues.

This decrease, which amounts to from twenty to forty per cent, occurs even after the administration of very small doses; more rapidly if the acid is injected directly into the veins; more slowly, yet with equal intensity, if absorbed from the intestines. The action is most striking in the case of fowls which neither vomit after injection of the arsenic nor reject their accustomed food; but even in cats which are subject to vomiting after the injection and must therefore be regarded as in a starving condition, the waste of the organism was diminished about twenty per cent after subtracting the decrease occasioned by the mere want of food.

This fact satisfactorily explains the fattening of horses after small doses of arsenious acid, a phenomenon well known to horse dealers.

An amount of fat and albuminous substances equivalent to the repressed carbonic acid and urea remains in the body and increases its weight, if the animal receives at the same time a sufficient amount of food.

When larger doses of arsenious acid are given nervous symptoms appear, which may be classified in two groups: spinal irritation and paralysis. To the first may be referred the vomiting, the accelerated respiration, the feeble pulse; to the last, the inclination to sleep, the weakness, and the retarded and labored breathing. Both may be explained by the very considerable congestion of the central organs which was constantly observed in post mortem examinations.

These experiments are of particular interest since they go far to prove the complete reliability of the published accounts of the custom of "arsenic eating," which is said to prevail among the peasantry of several Austrian provinces. These accounts have been time and again held up to ridicule by toxicologists,† and as a rule have been received with suspicion by all scientific men. They have nevertheless been widely published and are consequently well known to the public.

During the last eight or ten years the origin of these accounts

* *Journal für praktische Chemie*, 1859, lxxviii, p. 373.

† See for example, *Christison. Edinburgh Medical Journal*, Feb. 1856, i, 709. *A. Chevallier, Journal de Chimie Médicale, etc.*, 1854, [3.] x, 439. Or *Taylor*, in his work *On Poisons*. London, Churchill, 1859, p. 91.

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has been generally attributed to Dr. v. Tschudi, who published a communication upon the subject in 1851;* an abstract of which may be found in Chambers' Edinburgh Journal, December 20, 1851, [N. S., No. 416,] p. 389.†

Two years later, v. Tschudi made another communication,‡ in support of his previous assertions:—this, in connection with his first letter which had previously attracted comparatively little attention, was very extensively copied.§

Similar stories had been circulated, however, long before the letters of v. Tschudi were made public. For example, our own attention was first directed to the subject by the statement published in the Penny Cyclopædia of the Society for the Diffusion of Useful Knowledge, London, 1832, ii, 403. Art. "*Arsenic, Medical Uses of.*" * * *

"That its [white oxyd of arsenic] employment in such doses [$\frac{1}{30}$ or $\frac{1}{15}$ of a grain] as we have stated is not only safe but beneficial, may be satisfactorily proved. Not only are old worn out horses endowed with new vigor, improved appetite, &c., by its use, but pigeons to which this article is given, show greater appetite and liveliness than others without it; and in Upper Styria the peasantry use it as a seasoning with many articles of food, such as cheese." * * *

In the Boston Medical and Surgical Journal, 1835, xii, 211, is the following:

"Dr. Strohmayer in his Medicinische Praktische,‖ relates in exemplification of the extent to which the system may become accustomed to the operation of arsenic, that a peasant who resided near a convent in the Tyrol, for a long time, took ten grains of arsenic daily with his food." * *

In noticing the article in Chambers' Journal, for 1851, a correspondent of the London and Edinburgh Monthly Journal of Medical Science, February, 1852, xiv, 190, cites the following extracts:

* *Wiener medizinische Wochenschrift*, October 11th, 1851, vol. i, No. 28.

† From which it was copied into Wells's Annual of Scientific Discovery, 1852, p. 362.—Hays's American Journal of the Medical Sciences for July, 1852, vol. xxiv, p. 270, also contains extracts of v. Tschudi's letter, taken from the French *Gazette des Tribunaux* through the *Journal des connaissances Med. Chirurg.*, December 16th, 1851.

‡ *Wiener medizinische Wochenschrift*, 1853, No. 1.

§ *In extenso* in *Journal de Chimie Médicale, etc.*, 1854, [3.] x, 439; from *La Presse Médicale Belge;* from *Journal de la Société des Sciences médicales et naturelles de Bruxelles;* abstract in Chambers's *Edinburgh Journal*, June 11th, 1853, [N. S.,] vol. xix, No. 493, p. 382.

An abstract of the first (1851) letter, in the *Gazette de Hôpitaux*, of Paris, May 16, 1854, p. 229; from *Journal de Médicine de Bruxelles*, (see also *London Medical Times and Gazette*, July 1854, xxx, 66,) is perhaps the best known of any of the numerous extracts from v. Tschudi's statements, unless it be that given by J. F. W. Johnston in his Chemistry of Common Life, New York, Appleton, 1855; i, 166; also in Blackwood's Magazine, Dec. 1853, lxxiv, 687.

[‖ Quere? Strohmayr, Frz. *Medicinische praktische Darstellung gesammelter Krankheitsfälle, u. des Heilverfahrens aus dem Tagebuch meiner Erfahrung Wien, Gerold*, 1831.]

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"From Vogt's,* Arzneimittellehre, B. 1, S. 507." "It is well-known that old worn out horses gain an appetite, strength and spirit by the use of arsenic; and a pigeon which often got arsenic was observed to have its appetite increased and its movements more lively."

"From *Med. Jahb. des Oester. Staates*, 1822, S. 99." "There is scarce a district of Upper Styria in which in at least one house, arsenic may not be found under the name of Hydrach, Orpiment, &c. It is used for diseases of the domestic animals, against vermin, and also as a stomachic to increase the appetite. A peasant in my presence showed, with the point of a knife, how much arsenic he took daily and without which he said he could not live. I estimated the quantity at about two grains. It is also said to be used as a seasoning for cheese, and indeed several cases of poisoning by Styrian cheese have occurred, and one but lately."

Similar statements made by Wibmer, (probably in his book, entitled, "*Die Wirkung der Arzneimittel, u. Gifte im gesunden thierischen Körper*," 4 vols., Munich, 1831–39,) are referred to in German works upon the materia medica; while travellers who have spent much time in these provinces, all concur in their statements regarding the common custom of mixing arsenic with the food of horses.†

Evidence of this sort could without doubt be multiplied to almost any extent by any one familiar with the literature of the provinces in question, or with the habits of their people. A quantity of such material,‡ has indeed been recently collected by Heisch, and published in the London Chemical News, May 19, 1860, i, 280; from which we quote it, as being recent, (for the most part,) precise and tolerably direct; although it does not differ in its general import from the testimony which had already been offered.

On the Arsenic Eaters of Styria, by Charles Heisch, Esq., F.C.S., Lecturer on Chemistry at the Middlesex Hospital Medical College.§—At the last meeting of the Manchester Philosophical Society I observe that Dr. Roscoe called attention to the arsenic eaters of Styria. Having for the last two years been in communication with the medical men and other residents in the districts where this practice prevails, I shall feel obliged if you will allow me through your journal to make known the facts I have at present collected. The information is derived mainly from Dr. Lorenz, Imperial Professor of Natural History, formerly of Salzburg, from Dr. Carl Arbele, Professor of Anatomy in Salzburg, and Dr. Kottowitz, of Neuhaus, besides several non-medical friends. If human testimony be worth anything, the fact of the existence of arsenic eaters is placed beyond a doubt. Dr. Lorenz, to whom questions were first addressed, at once stated that he was aware of the practice, but added, that it is generally difficult to get hold of individual cases, as the obtaining of arsenic

* [Qu.? Voigtel, Fr. G. *System der Arzneimittellehre*, Leipzig, 1816.]

† A custom which seems also to prevail to a certain extent in England. Compare Kesteven, cited by Taylor, (*op. cit.*, p. 89.) from the Association Medical Journal, Sept. 6, and 20, 1856. We have to regret our inability to refer to K.'s original paper, the tenor of which is not readily to be inferred from Dr. Taylor's extracts.

‡ Compare Boner of Ratisbon in Chambers's Journal of Pop. Lit., &c., Feb. 9th, 1856, vol. v, No. 110, p. 90; see also *ibid*, July 19th, 1856, vol. vi, No. 133, p. 46.

§ From the Chemical News, May 19th, 1860.

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without a doctor's certificate is contrary to law, and those who do so are very anxious to conceal the fact, particularly from medical men and priests. Dr. Lorenz was, however, well acquainted with one gentleman, an arsenic eater, with whom he kindly put me in communication, and to whom I shall refer again more particularly. He also says that he knows arsenic is commonly taken by the peasants in Styria, the Tyrol, and the Salzkammergut, principally by huntsmen and woodcutters, to improve their wind and prevent fatigue. He gives the following particulars:—

"The arsenic is taken pure when fasting, in some warm liquid, as coffee, beginning with a bit the size of a pin's head, and increasing to that of a pea. The complexion and general appearance are much improved, and the parties using it seldom look so old as they really are, but he has never heard of any case in which it was used to improve personal beauty, though he cannot say that it never is so used. The first dose is always followed by slight symptoms of poisoning, such as burning pain in the stomach and sickness, but not very severe.

"Once begun, it can only be left off by very gradually diminishing the daily dose, as a sudden cessation causes sickness, burning pains in the stomach, and other symptoms of poisoning, very speedily followed by death.

"As a rule, arsenic eaters are very long lived, and are peculiarly exempt from infectious diseases, fevers, &c., but unless they gradually give up the practice invariably die suddenly at last.

"In some arsenic works near Salzburg with which he is acquainted, he says the only men who can stand the work for any time are those who swallow daily doses of arsenic, the fumes, &c., soon killing the others. The director of these works, the gentleman before alluded to, sent me the following particulars of his own case. (This gentleman's name I suppress, as he writes that he does not wish the only thing known about him in England to be the fact that he is an arsenic eater; but if any judicial inquiry should arise which might render positive evidence of arsenic eating necessary, his name and testimony will be forthcoming):—

"'At seventeen years of age, while studying assaying, I had much to do with arsenic, and was advised by my teacher, M. Bönsch, Professor of Chemistry and Mineralogy at Eisleben, to begin the habit of arsenic eating. I quote the precise words he addressed to me:—'If you wish to continue the study of assaying, and become hereafter superintendent of a factory, more especially of an arsenic factory, in which position there are so few, and which is abandoned by so many, and to preserve yourself from the fumes, which injure the lungs of most, if not of all, and to continue to enjoy your customary health and spirits, and to attain a tolerably advanced age, I advise you, nay, it is absolutely necessary, that besides strictly abstaining from spiritous liquors, you should learn to take arsenic; but do not forget, when you have attained the age of fifty years, gradually to decrease your dose, till from the dose to which you have become accustomed you return to that with which you began, or even less.' I have made trial of my preceptor's prescriptions till now, the forty-fifth year of my age. The dose with which I began, and that which I take at present, I enclose; they are taken once a day, early, in any warm liquid, such as coffee, but not in any spirituous liquors.' The doses sent were No. 1, original dose, three grains: No. 2, present dose, twenty-three grains of pure white arsenic in coarse powder. Dr. Arbele says this gentleman's daily dose has been weighed there also, and found as above. Mr. —— continues:—'About an hour after taking my first dose (I took the same quantity daily for three months,) there followed slight perspiration with griping pains in the bowels, and after three or four hours a loose evacuation; this was followed by a keen appetite and a feeling of excitement. With the exception of the pain, the same symptoms follow every increase of the dose. I subjoin, as a caution, that it is not advisable to begin arsenic eating before the age of twelve or after thirty years.' In reply to my question, if any harm results from either interrupting, or altogether discontinuing the practice, he replies, 'Evil consequences only ensue from a long continued interruption. From circumstances I am often obliged to leave it off for two or three days, and I feel only slight languor and loss of appetite, and I resume taking the arsenic in somewhat smaller doses. On two occasions, at the earnest solicitations of my friends, I attempted entirely to leave off the arsenic. The second time was in January 1855. I was induced to try it a second time from a belief that my first illness might have arisen from some other cause. On the third day of the second week after leaving off the dose I was attacked with faintness, depression of spir-

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its, mental weakness, and a total loss of the little appetite I still had; sleep also entirely deserted me. On the fourth day I had violent palpitation of the heart, accompanied by profuse perspiration. Inflammation of the lungs followed, and I was laid up for nine weeks, the same as on the first occasion of leaving off the arsenic. Had I not been bled, I should most likely have died of apoplexy. As a restorative, I resumed the arsenic eating in smaller doses, and with a firm determination never again to be seduced into leaving it off, except as originally directed by my preceptor. The results on both occasions were precisely the same, and death would certainly have ensued had I not resumed arsenic eating.' One of the most remarkable points in this narrative is that this gentleman *began* with a dose which we should consider poisonous. This is the only case of which I have been able to obtain such full particulars, but several others have been mentioned to me by those who knew the parties, and can vouch for their truth, which I will briefly relate.

"One gentleman, besides stating that he is well aware of the existence of the practice, says he is well acquainted with a brewer in Klagenfürth who has taken daily doses of arsenic for many years; he is now past middle life, but astonishes every one by his fresh juvenile appearance; he is always exhorting other people to follow his example, and says, 'See how strong and fresh I am, and what an advantage I have over you all! In times of epidemic fever or cholera, what a fright you are in, while I feel sure of never taking infection.'

"Dr. Arbele writes, 'Mr. Curator Kürsinger (I presume curator of some museum at Salzburg,) notwithstanding his long professional work in Lungau and Binzgau, knew only two arsenic eaters, one the gentleman whose case has just been related, the other the ranger of the hunting district in Grossarl, named Trauner. This man was at the advanced age of 81 still a keen chamois hunter and an active climber of mountains; he met his death by a fall from a mountain height while engaged in his occupation. Mr. Kürsinger says he always seemed very healthy, and every evening regularly, after remaining a little too long over his glass, he took a dose of arsenic, which enabled him to get up the next morning perfectly sober and quite bright. Professor Fenzl of Vienna was acquainted with this man, and made a statement before some learned society concerning him, a notice of which Mr. Kürsinger saw in the *Wiener Zeitung*, but I have not been able to find the statement itself. Mr. Krum, the pharmaceutist here, tells me that there is in Stürzburg a well known arsenic eater, Mr. Schmid, who now takes daily twelve; and sometimes fifteen, grains of arsenic. He began taking arsenic from curiosity, and appears very healthy, but always becomes sickly and falls away if he attempts to leave it off. The director of the arsenic factory before alluded to is also said to be very healthy, and not to look so old as forty-five, which he really is.'*

"As a proof how much secrecy is observed by those who practise arsenic eating, I may mention that Dr. Arbele says he inquired of four medical men, well acquainted with the people of the districts in question, both in the towns and country, and they could not tell him of any individual case, but knew of the custom only by report.

"Two criminal cases have been mentioned to me, in which the known habit of arsenic eating was successfully pleaded in favor of the accused. The first, by Dr. Kottowitz of Neuhaus, was that of a girl taken up in that neighborhood on strong suspicion of having poisoned one or more people with arsenic, and though circumstances were strongly against her, yet the systematic arsenic eating in the district was pleaded so successfully in her favor, that she was acquitted, and still lives near Neuhaus, but is believed by every one to be guilty. The other case was mentioned by Dr. Lorenz. A woman was accused of poisoning her husband, but brought such clear proof that he was an arsenic eater, as fully to account for arsenic being found in the body. She was, of course, acquitted.

"One fact mentioned to me by some friends is well worthy of note. They say: 'In this part of the world, when a graveyard is full, it is shut up for about twelve years, when all the graves which are not private property by purchase are dug up, the bones collected in the charnel-house, the ground ploughed over, and burying begins again. On these occasions the bodies of arsenic eaters are found almost un-

* The man above mentioned seems quite to differ with Mr. —— on the impropriety of taking arsenic with spiritous liquors, and actually employs it as a means of correcting their effects. All others that I have heard of concur in saying that it should be taken fasting.

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changed, and recognisable by their friends. Many people suppose that the finding of their bodies is the origin of the story of the vampire.' In the *Medicinischer Jahrbuch des Oster. Kaiserstaates*, 1822, *neuest Folge*, there is a report by Professor Schallgruber, of the Imperial Lyceum at Grätz, of an investigation undertaken by order of government into various cases of poisoning by arsenic. After giving details of six post-mortem examinations, he says:—'The reason of the frequency of these sad cases appears to me to be the familiarity with arsenic which exists in our country, particularly the higher parts. There is hardly a district in Upper Styria where you will not find arsenic in at least one house under the name of hydrach. They use it for the complaints of domestic animals, to kill vermin, and as a stomachic to excite an appetite. I saw one peasant show another, on the point of a knife, how much arsenic he took daily, without which, he said, he could not live; the quantity I should estimate at two grains. It is said, but this I will not answer for, that in that part of the country this poison is used in making cheese; and, in fact, several cases of poisoning by cheese have occurred in Upper Styria, one not long since. The above-mentioned peasant states, I believe truly, that they buy the arsenic from the Tyrolese, who bring into the country, spirits and other medicines, and so are the cause of much mischief.' This report is, I believe, mentioned in Orfila's *Toxicology*, and one or two other works, but I have not seen it quoted myself; it is interesting, as being early and official evidence of arsenic eating. Since I received the above information, a gentleman who was studying at this hospital, told me that, when an assistant in Lincolnshire, he knew a man who began taking arsenic for some skin disease, and gradually increased the dose to five grains daily. He said he himself supplied him with this dose daily for a long time. He wrote to the medical man with whom he was assistant, and I have been for a long time promised full particulars of the case, but beyond the fact that he took five grains of arsenic, in the form of Fowler's solution, daily, for about six years, and could never leave it off without inconvenience and a return of his old complaint, I have as yet not received them. I have delayed publishing these facts for some time, hoping to get information on some other points, for which I have written to my friends abroad; but as considerable delay takes place in all communications with them, I have thought it better to publish at once the information I have already received. All the parties spoken of are people on whom the fullest reliance can be placed, and who have taken much pains to ascertain the foregoing particulars. The questions which still remain unanswered are these:—

"1st. Can any official report be obtained of the trials of the two people mentioned by Drs. Kottowitz and Lorenz?

"2nd. Do medical men in these districts, when using arsenic medicinally, find the same cumulative effects as we experience here? Or is there anything in the air or mode of living which prevents it?

"3rd. Can any evidence be obtained as to how much of the arsenic taken is excreted? to show whether the body gradually becomes capable of enduring its presence, or whether it acquires the power of throwing it off?*

"I have proposed to the gentleman who furnished me with the particulars of his own case either to make an estimate of the arsenic contained in his own urine and fæces during twenty-four hours, or to collect the same and forward them to me that I may do so, but as yet have received no answer."—*Pharmaceutical Journal.*

The only wonder is that direct experiments have not been made long ago upon the excretions of suspected arsenic eaters, or upon their bodies after death. In calling attention to the subject some months since, (*Répertoire de Chimie Appliquée*, February, 1860, ii, 44,) we took occasion to urge this point and to bring forward the observation of Prof. E. Kopp†, who found in the course of his experiments upon arsenic acid—which was manufactured upon the great scale and largely employed in calico-print-

* The fact of the preservation of the bodies shows that some considerable quantity must be retained [or rather offers an example of the well known fact that corpses loaded with fat decompose but slowly.—F. H. S.]

† Comptes Rendus, 1856, xlii, 1063; *J. pr. Ch.* lxix, 273.

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ing by him—that the weight of his body rapidly increased, some 20 lbs. having been gained in the course of the two months, during which he was subject to absorb the acid, his hands having been frequently in contact with the arsenical solution: arsenic being detected the while in his solid and liquid excrements. As soon, however, as the exposure to the arsenic ceased, his weight began to decrease, and in the course of 9 or 10 weeks, fell back again to its normal—150 lbs. Believing that direct, positive evidence like this—though the instance be solitary—where the subject of the experiment was a healthy, vigorous man, and a trained observer, ought to outweigh almost any amount of negative testimony, such as has been brought forward by physicians who have not witnessed similar effects upon their *diseased* patients when the latter were treated with arsenical preparations. It should be observed that one of the strongest arguments brought up by toxicologists against the truth of the accounts of the arsenic eaters has been drawn from the result of medical practice. Without attempting to discuss the matter at length, it may, nevertheless be permitted to the non-professional reader of the medical literature bearing upon this subject to remark that the evidence there accumulated in spite of its apparent obscurity and of the contradictions with which it is involved seems to point clearly to the fact that in minute doses arsenious acid acts beneficially upon the general health of many patients. Thus, according to Dr. Henry Hunt:*

"Arsenic operates most favorably on persons who are of lax fibre, accompanied by a languid state of the circulation and whose secretions are rather profuse than otherwise; the urine pale and plentiful, and more especially on those whose skin is cold and moist. In persons of this description, whilst arsenic to an extent far beyond other medicines, relieves the neuralgic pain, it improves the general health, and gives firmness and vigor to the constitution." * * * * While upon certain peculiar temperaments it is hurtful; thus Dr. Hunt continues, "when neuralgia is associated with the same morbid action in the spine, with anæmia; or arises from injuries of nerves; or local irritation of nerves by diseases, or unnatural growth of bone; or if it be complicated with engorgement of the liver and other viscera; arsenic is usually injurious, and I believe seldom useful."

Again, not to multiply instances further, Erichsen,† in treating of the use of arsenic in diseases of the skin, "lays great stress upon the necessity of attending particularly to the constitution, and temperament of the patient before commencing the use of the medicine. It will be badly borne by individuals of a plethoric habit of body or of a highly sanguine or sanguineo-nervous temperament,—this arises from the stimulating properties of the metal. In such cases, the digestive organs become so ir-

* In his work upon Neuralgic Disorders, cited in Braithwaite's Retrospect of Pract. Med., 1844, No. ix, p. 34; also, *ibid*, No. x, p. 23.

† Braithwaite's Retrospect, 1843, No. viii, p. 14.

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ritated and the nervous system so excited under the use of the arsenic that it is impossible to employ it in any such dose as can be expected to produce a beneficial effect upon the cutaneous affection. There are other circumstances which contra-indicate the use of this remedy, namely, 'the complication of the cutaneous affection with other diseases,' and especially with irritative or inflammatory gastric dyspepsia, accompanied with a sensation of heat and oppression at the epigastrium, increased by food so well described by Dr. Todd. When this form of dyspepsia is present the smallest doses of arsenic will do harm, as the usual effect of the remedy when continued too long is to produce these very symptoms. Besides this form of indigestion any other local inflammatory condition of the system or the superinvention of phthisis, will contra-indicate the use of so powerfully stimulating a tonic as arsenic." Further on he remarks: "It has already been shown that the use of the preparations of this metal is exceedingly hazardous in individuals of a sanguine or sanguineo-nervous temperament and excitable habit of body, or in those who suffer from or are peculiarly disposed to irritative gastric dyspepsia, or any inflammatory disorder. On the other hand, they are in most cases borne well by individuals of a somewhat phlegmatic, debilitated, or lax habit of body, more particularly, if they are past the middle age, with a pale cachectic complexion, languid weak circulation, and a general want of tone about the system, acting upon such patients as powerful and useful tonics. In persons of this habit of body the diseases of the skin appear rather to be dependent upon a degree of debility or want of power in the cutaneous capillaries; and it is in these patients that the preparations of arsenic are of great service in exciting in a peculiar manner a more healthy action in this class of vessels, thereby modifying or removing those morbid changes that are the results of an abnormal condition in their secernent and nutrient functions."

In this connection it must not be forgotten that in the opinion of many scientific men, the healing action of various mineral waters may depend, in part, at least, upon the arsenic which these springs are known to contain.* A doctrine which is publicly taught by several of the chemical Professors at Paris.

Taken as a whole the medical evidence which has fallen under our notice, is adverse to the possibility of "arsenic eating," only in so far as relates to the large quantities of the poison which, as is affirmed, the human body can accustom itself by long continued habit, to support with impunity. This last enquiry, however interesting in itself, is one on which very little is known with certainty as yet, and is plainly of quite secondary importance in a scientific point of view to that of the beneficial action of moderate doses of arsenious acid, which would now appear to be proved. From the very general interest which attaches to the subject it is greatly to be hoped that further researches may soon decide the amount of this tolerance. F. H. S.

* Compare *Walchner, Ann. Ch. u. Pharm.*, lxi, 206; or *Comptes Rendus*, xxiii, 614. *Figuier, Comptes Rendus*, xxiii, 820. *Chevallier* and *Schanefele, ibid.* xxv, 750. *Thenard, ibid.* xxxix, 769.

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ART. XXI.—*Geographical Notices.* No. XIII.

JOURNAL OF THE AMERICAN GEOGRAPHICAL SOCIETY.—The first number of the second volume of the American Geographical Society's Journal has appeared in a new and enlarged form, an octavo volume of 148 pages. The articles (eight in number) are of a more extended and scientific character than usual, and the whole appearance of the Journal is such as will reflect great credit on the society and enlarge its sphere of usefulness.

Article first, which is compiled from data furnished by the Hydrographical Office, Washington, gives an account of the progress of Marine Geography, within the past few years; which is followed by a synopsis of the operations of the Coast Survey during the year 1859, by Prof. Bache. An account of the lake Yojoa in Honduras, contributed by Mr. E. G. Squier, is the next article. The address commemorative of Karl Ritter, delivered before the Society by Prof. Guyot, comes next in order, forming the most complete and eloquent tribute to the great geographer which has yet appeared. Its review of the characteristics and contents of Ritter's Erdkunde is especially valuable. The sixth article is a translation by Mr. E. R. Straznicky from the Journal of the Geographical Society of France, of an essay on the Geographical distribution of Animals, by Mons. A. Maury. Mr. J. G. C. Kennedy, the superintendent of the U. S. Census, then reviews the origin and progress of Statistics, and Dr. Wynne illustrates the working of benevolent societies, such as the Odd Fellows, among the laboring classes. The number is concluded by an excellent and full survey of recent geographical and statistical literature, prepared by the General Secretary of the Society, Mr. D. W. Fiske, to whom, with the coöperation of the Committee of Publication, the editing of the Journal was entrusted. The Society now has a small but well selected library, with rooms in a central part of New York City. Its list of active members enrolls about five hundred names, and its usefulness and importance have never been greater than at present.

SCHLAGINTWEIT'S MISSION TO CENTRAL AND HIGH ASIA.—We have received through Mr. S. H. Grant of New York, the prospectus of Mr. Brockhaus of Leipsic, announcing the contents and character of the Report which is soon to be published by the brothers Schlagintweit on their journey to the Himalayas, from 1854 to 1858. It contains some information to which we have not had access in our previous notices of their expedition. Since the return of the authors from India in June 1857, they have been engaged in preparing for publication the results

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of their observations, and are now able to promise a work in nine volumes, quarto, with an atlas in three folio volumes. The importance of this work will justify us in explaining its character at some length.

The first volume will be devoted to Astronomy and Magnetism. The observations extend from Ceylon to Turkistan and from Assam to Kabul. Their importance may be the better appreciated if it is borne in mind that "with the exception of the well known observations at the government observatories of Bombay and Madras, some very valuable ones by Taylor and Caldecott in Southern India, and others recently made by Brown at Travankor, scarcely any observations have been taken in the interior of India, so that the modifications of the magnetic lines over this large area form a new object of scientific discussion."

In India proper the astronomical labors of the Schlagintweits relate chiefly to the determination of the true and magnetic meridian and to observations for finding time. But in the Himalaya, their operations included also determinations of latitude and longitude. The whole of Western Thibet has been found to be farther west than has hitherto been supposed, and for Kuenluen and Turkistan the latitudes have also been largely corrected.

The second volume will contain the hypsometrical and trigonometrical observations, including the determination of about 2000 points in the various countries explored. The third volume, on Topical Geography, has for its object a practical aim, reviewing chiefly the commercial and military routes in High Asia, with reference to their commercial and military importance. Part of this volume is devoted to linguistic researches and vocabularies. The fourth and fifth volumes include all that the explorers have collected on Meteorology, and the sixth is devoted to Geology. Volume seventh relates to Botany and Zoology. Volume eighth is given to Ethnography, including an examination and comparison of the facial casts to which we have previously referred in this Journal. The ninth and last volume presents in a popular form comparative descriptions of the various regions of India and High Asia.

The Atlas will contain: 1. Maps, geographical, physical, and geological; 2. Profiles, meteorological, hydrographical and geological; and 3. Views and general panoramas. The general size of the plates is three feet by two. The whole cost of the work will be £86, and its completion is promised in about three years.

Canadian Expedition to the Red River under Gladman, Dawson, Hind, and Napier, 1857–1858.—The interest which has been manifested in the report of the Palisser expedi-

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tion contained in a recent number of this Journal,* leads us to condense and translate from Dr. Petermann's excellent *Mittheilungen* (January, 1860) an account of the explorations of the Red River which were made in 1857 and 1858 by Gladman, Dawson, Hind, and Napier. We regret that we cannot reproduce the admirable maps which accompany the article. The writer in Petermann's Journal remarks substantially as follows:

Although the Canadians had long endeavored to direct the attention of the British government to that vast portion of British North America, which stood until very recently under the immediate supervision of the Hudson's Bay Company, and had tried to induce them to effect a revision of the claims of that mercantile body, it was nevertheless, not until 1856 when gold was discovered in Fraser's and Thompson's rivers, that the British government took the matter into serious consideration, and in 1857 sent out an expedition (Pallisser's expedition) and declared in 1858 New Caledonia, as it was called under the above mentioned company, an independent colony, to be known in future by the name of British Columbia. At the same time it was urged, that the government of Canada might be empowered to incorporate adjacent portions of land, particularly the so-called Saskatchewan district, east of the Rocky Mountains. This expedition accomplished its chief object, to find a passage across the Rocky Mountains, and also reported favorably in regard to future settlements in the Saskatchewan district, which may be called the intermediate district between the settled portion of British North America and the new gold region in British Columbia. At the same time with Palliser's expedition another expedition was started directly by the Canadian government, and it is our object in the present paper, after having presented a few general remarks on the country, to give a brief synopsis of the course of this latter expedition.

The Saskatchewan district between the Red River and the Rocky Mountains has already, since the beginning of the present century, been the object of many explorations, the most prominent of which are those of Astronomer Thompson†, Lefroy, Richardson, Lord Selkirk, Blodget, and others. They all agree that the Saskatchewan district is well adapted for cultivation. It comprises an immense area, and as early as 1805, Lord Selkirk

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* Vol. xxviii, p. 320.

† Thompson was from 1790 over 30 years in the employ of the Hudson's Bay Company, and the reports of his explorations (37 vols.) are deposited in the Archives of this Company. From fragments of them it appears that Thompson possessed a great knowledge of the country, but it is doubtful whether these reports will ever be accessible to such as are not connected with the Company. Until now the Company has kept them back. [Compare this Journal, xxviii, 344, note.]

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said that it could give bread to at least 30 millions of people. In regard to the climate, says Blodget, who is most thoroughly acquainted with the subject, that the average temperature in winter is not below that of St. Petersburg and Moscow; in summer it equals that of northern Italy and New York. The temperature increases, just as in Europe, as you go from east to west. Spring commences at all points almost at the same time. There is no want of rain; grass, forests and buffaloes abound. Useful timber is abundant; coal is found in many places, but particularly rich deposits exist at the foot of the Rocky Mountains, and near the Little Sauris River. The country is level and appears so even, that Blakistone remarked that for the construction of a railroad nothing was required but to put down the rails. Its numerous lakes and rivers can easily be connected for internal communication, and afford even now the only means of transport between the different stations of the Hudson's Bay Company. The Saskatchewan district can also easily be connected with the new gold region by means of commodious roads through Palliser's passage across the Rocky Mountains. This new colony will, by reason of its very favorable situation, its beautiful harbors, but particularly by reason of its wealth in gold, surely rise as speedily as Southern California; and, as it is less capable of agriculture, would naturally become the great market for the products of its eastern neighbors, in the Saskatchewan district.

We may therefore well be justified in prognosticating for this district a prosperous future in regard to agriculture, but we cannot agree with such opinions expressed some time ago in the Montreal Pilot, that by a regularly established road from Lake Superior to Lake of the Woods, Red River, Lake Winnipeg, Saskatchewan river, across the Rocky Mountains to the rivers of British Columbia, thence to the Pacific, all commercial intercourse between Europe and China, Japan and India would take this route. A road which changes so often between land and water can never become a general commercial road for such a distance, not to mention the almost insurmountable difficulties for vessels of a larger draught, such as sudden bends, rapids, falls, shallow waters, etc., and the entirely uncultivated state of the country.

After these few remarks we return to our subject proper. We can give but a brief synopsis, and refer those who desire a detailed account of the Canadian expedition, to the "Reports on the exploration of the country between Lake Superior and the Red River Settlement." A still more minute account is given in the "Papers relative to the Explorations of the Country between Lake Superior and the Red River settlement, presented to both Houses of Parliament, London, 1859." Three charts by

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Hind (one a reprint of Thompson's), and a sketch of the regions, which Dawson traveled through, by himself, appeared at the same time.

The members of the Canadian expedition landed July 31, 1857, at Fort William, and started in boats along the usual route of the Hudson's Bay Company for Lake Winnipeg, in order to ascertain the practicability of this route. To this end surveys of rivers were made and a very minute determination of levels. Napier estimates the whole length of the route to be 747 miles, viz: from Lake Superior to Rainy Lake 335 miles; thence to Rat Portage at the northern extremity of Lake of the Woods 176 miles; from this point to Fort Garry on the Red River 236 miles. Of these three portions only the middle one, upon Rainy Lake, which is at an average 460 feet wide and 6 feet deep, forms a continuous water road. Its falls (Chaudiere falls near Fort Francis, 22 feet,) may, according to Dawson, easily be made harmless by two water gates. The two remaining portions of the route can only be travelled by land, unless one prefers the tedious transport from one little river to another. The Kaministiquoria on the first portion of the route cannot be navigated, as its rapids, shallow water places, and falls (Kakabeke falls, 119 feet,) are too numerous. From Little to Great Dog Lake, a distance not over a mile, this river falls 348 feet, and yet the portage in this place has still an elevation of 142 feet over Great Dog Lake. This is the steepest descent on the whole route. The passage upon Dog River is partially obstructed by rocks and sandbanks, and on Prairie Portage, between Lake Superior and Lake Winnipeg, it leads mostly through swamps. The difference of elevation between Lake Superior and Prairie Portage, 54 miles distant from one another, is, after Dawson, 879 feet, according to Napier 887 feet; that between Prairie Portage and Lake Winnipeg (325 miles) is calculated by Dawson 892, by Napier 870 feet. Thus the descent toward the east is much more rapid than toward the west. The canoe route from Savannah River to Rainy Lake has too many portages and the Rivière la Seine is, by reason of the numerous difficulties in its course, entirely objectionable. But the Winnipeg River, from Lake of the Woods to Lake Winnipeg, was by all declared to be the most difficult and impracticable on the whole route. The canoe route on the Pigeon River, from Lake Superior to Rainy Lake along the boundary, is the shortest, but it has 29 portages, of which many lead through United States territory. Another route to the Red River, which is still used by the Hudson Bay Company, commences from Fort York, near Hudson's Bay, and goes up Hays River, through Knee and Holy Lakes, Wepinapanis River, White Water Lake and Sea River, down to Lake Winnipeg; but it requires three weeks of hard work to travel it, besides the ac-

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cess to Fort York, through Hudson's Bay is only about two months open during the year. But the most commodious and most frequented road to the Red River over St. Paul and Crow Wing leads entirely through United States territory. In the English possessions the best connection between Lake Superior and the Red River would be established by country roads, the one from Lake Superior to Rainy Lake, the other from Lake of the Woods to the Red River. In regard to the first, however, nothing has as yet been done, and only in the latter district explorations have been made with this view. When Gladman had arrived at Fort Garry (September, 1857,) he sent out engineers Napier and Dawson to reconnoitre this hitherto entirely unknown district, which explorations were continued by Gaudet and Wells during the winter 1857–1858.

The whole country between the Red River and Lake of the Woods appeared perfectly level, although it actually descends toward the east nearly 400 feet. Dry prairies change alternately with wooded districts and extensive swamps, the latter being particularly frequent toward north. The establishment of a road through this district seemed to them an utter impossibility.

Hind went up the Assiniboine River, explored the Great and Little Rat river, examined the valley of the Red river up to Pembina, and followed the Reed Grass or Roseau river up to a great swamp, which separated this stream from a lake of the same name. Unfortunately Hind could not survey this river up to its sources, but all the Indians who lived there agreed that a swamp of 9 miles in extent existed between Roseau lake and Lake of the Woods. This swamp sends the Reed river, 30 miles long, to the latter lake, and another little rapid river, about 40 or 50 miles long, to lake Roseau. From the Great Muskeg morass goes a little river westward into an extensive swamp, from which the Rat river issues.

Gladman was relieved from his post as chief in April, 1858, and Napier also recalled about this time. But Hind went the same spring again with Dickinson, Fleming and Hine on another expedition known as the "Assiniboine and Saskatchewan expedition." Their object was to explore the regions west of the Red River and Lake Winnipeg up to the Saskatchewan river. Before they arrived at Fort Garry, Dawson, Wells and Gaudet had already made some new surveys, around the Red river, Lake Winnipeg, and the lower Assiniboine, and had just left for the lake district. This latter party went by way of Lake Manitobah and Lake Winnipego-sis, over Mossy Portage toward Cedar or Bourbon lake to the grand rapids of the Saskatchewan river. At Mossy Portage they separated; Wells went over Lake Winnipego-sis, Lake Dauphin, Lake Manitobah, the Little Saskatchewan river, which he found to be 8 to 12 feet deep, 250 yards wide,

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free from rapids and throughout adapted for steam navigation, thence over Lake Winnipeg to the Red river. The rest of the party followed Swan river to Fort Pelly, and thence went down the Assiniboine river.

Dawson considers the whole alluvial plain east of the Pasquia and Porcupine hills and Dauphin mountains, where the large lakes are situated, well adapted for settlements. It is partly prairie land, for the most part, however, thickly wooded. North of Lake Dauphin wood predominates; south of it the country becomes more open, and toward the Assiniboine an apparently endless prairie commences. Wheat gives abundant harvests near Lake Manitobah and the Little Saskatchewan river, and near the latter even Indian corn may be cultivated. The valley of the Swan river is particularly fertile and its climate equals that of the Red river district. The Red Deer river district has also a good soil and fine climate, as its maple tree forests plainly show. Coal is said to be found in the Porcupine hills and the Duck mountain; Dawson himself found samples of lignite near Snow river. The great alluvial valley of the Assiniboine and its branches will, in his opinion, hereafter become one of the finest wheat growing districts upon earth. Near Moss or Dauphin river, a fine navigable stream, the Indians grow maize, melons and potatoes. Vines, hops, and vetches grow naturally in abundance.

Hind and his companions went (June 14, 1858,) from Fort Garry in a westerly direction over Fort Ellis toward the missionary station near Qu' Appelle lake (July 18), where he divided his corps into three parties: Dickinson traveled on the Qu' Appelle river up to its mouth, thence on horseback to Fort Pelly; Hine surveyed Long lake northwest of the Qu' Appelle mission, then went over land to Fort Pelly to meet Dickinson, and to explore with him the Dauphin mountains; Hind and Fleming followed the Qu' Appelle river up to its source, went over to the elbow of the southern arm of the Saskatchewan or Bow river, on which they travelled down until they reached Fort à la Corne (Aug. 9). The Qu' Appelle and Bow rivers have no connection as Dr. Hector believes. The latter (southern arm of the Saskatchewan) has down from its elbow for a distance of about 100 miles, a width of 300 yards to half a mile, then it becomes narrower and straighter in its course, its sand and mud banks disappear and finally it hurries through a narrow and deep valley, with a strong current toward the northern arm of the Saskatchewan, with which it unites forming one river (Saskatchewan), which now goes toward Fort à la Corne through Pine and Cedar lakes into Lake Winnipeg. Fleming followed this course from Fort à la Corne into Lake Winnipeg, along its western coast, until he reached the Red River. Hind made a

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land voyage along Long creek, then turning southeast went over Touch Wood hills to Fort Ellis, where he met Dickinson with whom he returned over White Mud river to Fort Garry (September 4).

But Hind and Fleming soon started on another excursion (September 18). They went in boats along the western shores of Lake Winnipeg, up to the mouth of the Little Saskatchewan, hence (September 29) into Lake Manitobah, and by means of Water Hen river and a lake of the same name reached Lake Winnipego-sis, where they examined the salt springs, which had been imprudently exhausted by the Indians. From here they started for Lake Dauphin, ascended the Dauphin mountains (1700 feet high), and navigated Lake Manitobah in different directions. Hind stayed four days on a little island there, which was much revered by the Indians as the seat of the "Manitou," or fairies. On its northern side were limestone cliffs about fifteen feet high, which by the beating of the waves emitted sounds very similar to chimes from a number of church bells, ringing at a distance. From Oak Point, at the southern extremity of the lake, the party went over land toward Fort Garry, where they arrived the 31st of October, 1858.

Hine, while sojourning on the Red river during the fall months, took photographic views of landscapes, churches, Indians, etc. Dickinson made excursions in the district east of the lower Red River, and in the regions between the Assiniboine and the U. S. boundary, but particularly along Rivière Sal through the Pembina mountains and Blue hills.

Some Canadian journals have blamed this Expedition for not having made any determination of points and for giving generally but little positive information, although $50,000 to $60,000 had been expended for the purpose. They said that the whole country had been much better explored by the late astronomer Thompson. This, however, is an unjust imputation. Astronomical observations of points, although very valuable, cannot be the main object of explorers, who have to run through a great number of districts in a comparatively very short time, and who must give us the general features of the country; moreover, as here a great number of such fixed points already exist, a careful survey of routes by dead reckoning is perfectly sufficient. The reproach that the country had been much better explored by Thompson is most unjust. Thompson's reports were undoubtedly as little accessible to the members of the Canadian expedition as they were to the rest of the world; besides, if we compare Thompson's chart with that of the expedition of 1858, we perceive that our knowledge of the country between Lake Winnipeg and Bow river is more accurate and more complete than Thompson's.

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The expedition has achieved much. They made very comprehensive levellings, effected numerous measurements of width, depth and rapidity of rivers and lakes, made geological observations, inquired into the climate, forests, quality of soil, etc., made surveys and discoveries between Lake of the Woods and the Red river, between the Assiniboine river and the U. S. boundary, along the upper Assiniboine and Qu' Appelle rivers, in the district of the great lakes, etc. A comparison of their charts with the older ones of these districts will at once show that the money was not thrown away.

This expedition has moreover excited the curiosity of the people more than that under Capt. Palliser. Thus a society was formed at St. Paul in Minnesota, who, under the direction of Col. Nobles, left this city in June, 1859, with the object to explore the valleys and sources of the Saskatchewan and Columbia rivers. Their plan was, to start from the elbow of Bow river toward the Rocky Mountains, to explore carefully the region of their eastern foot up to Edmonton House, thence to go over Athalaska Portage between Mount Hooker and Mount Brown toward the sources of Thompson's river and here to disperse in different directions. Col. Nobles intended to start for the sources of Columbia river, and to return over Lewis and Clarke's Passage, the Missouri Falls, the valley of the Milk river, Fort Mandan, Big Stone Lake, and Fort Ridgley to St. Paul. Dr. Goodrich accompanies them as physician, and the Smithsonian Institution sent Dr. C. L. Anderson, of Minneapolis, to make scientific observations and collections.

The "Board of Trade" in St. Paul offered a reward of $1000 for the first steamer that should ply on or before the first of June on the Red river, and the "Anson Northup" really commenced her voyages in June. She carries, besides passengers, 100 to 150 tons of cargo, and is intended to do the post service between the mouth of the Shagerme river and Fort Garry, and thus to connect St. Paul, (which sustains a post wagon up to the Shagerme river,) directly with the Red river.

Another company in Canada intend to put four steamers on Rainy lake, Red river and Lake Winnipeg. Even the settlers on the Red river themselves show an active spirit of progress.

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ART. XXII.—*Discussion between two Readers of Darwin's Treatise on the Origin of Species, upon its Natural Theology.*

FIRST READER.—Is Darwin's theory atheistic or pantheistic? or, does it tend to atheism or pantheism? Before attempting any solution of this question, permit me to say a few words tending to obtain a definite conception of *necessity*, and *design*, as the sources from which events may originate, each independent of the other; and we shall, perhaps, best attain a clear understanding of each, by the illustration of an example in which simple human designers act upon the physical powers of common matter.

Suppose, then, a square billiard table to be placed with its corners directed to the four cardinal points. Suppose a player standing at the north corner, to strike a red ball directly to the south; his design being to lodge the ball in the south pocket; which design, if not interfered with, must, of course be accomplished. Then suppose another player, standing at the east corner, to direct a white ball to the west corner. This design also, if not interfered with, must be accomplished. Next suppose both players to strike their balls at the same instant, with like forces, in the directions before given. In this case the balls would not pass as before, namely, the red ball to the south, and the white ball to the west, but they must both meet and strike each other in the centre of the table, and, being perfectly elastic, the red ball must pass to the west pocket, and the white ball to the south pocket. We may suppose that the players acted wholly without concert with each other, indeed they may be ignorant of each other's design, or even of each other's existence; still we know that the events must happen as herein described. Now the first half of the course of these two balls is from an impulse, or proceeds from a power, acting from design. Each player has the design of driving his ball across the table in a diagonal line to accomplish its lodgment at the opposite corner of the table. Neither designed that his ball should be deflected from that course and pass to another corner of the table. The *direction* of this second part of the motion, must be referred entirely to *necessity*, which directly interferes with the purpose of him who designed the rectilinear direction. We are not in this case, to go back to find design in the creation of the powers or laws of inertia, and elasticity, after the order of which the deflection, at the instant of collision, necessarily takes place. We know that these powers were inherent in the balls, and were not created to answer this special deflexion. We are required, by the hypothesis, to confine attention in point of time, from the instant preceding the impact of the balls, to the time of their

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arrival at the opposite corners of the table. The cues are moved by design. The impacts are acts from design. The first half of the motion of each ball is under the direction of design. We mean by this the particular design of each player. But at the instant of the collision of the balls upon each other, direction from design ceases, and the balls no longer obey the particular designs of the players, the ends or purposes intended by them are not accomplished, but frustrated, by *necessity*, or by the necessary action of the powers of inertia and elasticity, which are inherent in matter, and are not made by any design of a Creator for this special action, or to serve this special purpose, but would have existed in the materials of which the balls were made, although the players had never been born.

I have thus stated, by a simple example in physical action, what is meant by design and what by necessity; and that the latter may exist without any dependence upon the former. If I have given the statement with what may be thought, by some, unnecessary prolixity, I have only to say that I have found many minds to have a great difficulty in conceiving of necessity as acting altogether independent of design.

Let me now trace these principles as sources of action in Darwin's work or theory. Let us see how much there is of design acting to produce a foreseen end, and thus proving a reasoning and self-conscious Creator; and how much of mere blind power acting without rational design, or without a specific purpose or conscious foresight. Mr. Darwin has specified in a most clear and unmistakeable manner the operation of his three great powers, or rather, the three great laws by which the organic power of life, acts in the formation of an eye. (See p. 169). Following the method he has pointed out, we will take a number of animals of the same species, in which the eye is not developed. They may have all the other senses, with the organs of nutrition, circulation, respiration and locomotion. They all have a brain and nerves, and some of these nerves may be sensitive to light; but have no combination of retina, membranes, humors, &c., by which the distinct image of an object may be formed and conveyed by the optic nerve to the cognizance of the internal perception, or the mind. The animal in this case would be merely sensible of the difference between light and darkness. He would have no power of discriminating form, size, shape, or color, the difference of objects, and to gain from these a knowledge of their being useful or hurtful, friends or enemies. Up to this point there is no appearance of *necessity* upon the scene. The billiard balls have not yet struck together, and we will suppose that none of the arguments that may be used to prove, from this organism, thus existing, that it could not have come into form and being without a creator acting to this end with intelligence

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and design, are opposed by anything that can be found in Darwin's theory; for so far, Darwin's laws are supposed not to have not come into operation. Give the animals thus organized, food and room, and they may go on, from generation to generation, upon the same organic level. Those individuals that, from natural variation, are born with *light-nerves* a little more sensitive to light than their parents, will cross or interbreed with those who have the same organs a little less sensitive, and thus the mean standard will be kept up without any advancement. If our billiard table were sufficiently extensive, *i. e.*, infinite, the balls rolled from the corners would never meet and the *necessity* which we have supposed to deflect them would never act.

The moment, however, that the want of space or food commences *natural selection* begins. Here the balls meet, and all future action is governed by *necessity*. The best forms, or those nerves most sensitive to light, connected with incipient membranes and humors, for corneas and lenses, are picked out and preserved by natural selection, of necessity. All cannot live and propagate, and it is a necessity, obvious to all, that the weaker must perish, if the theory be true. Working on, in this way, through countless generations, the eye is at last formed in all its beauty and excellence. It must, (always assuming that this theory is true,) result from this combined action of natural variation, the struggle for life, and natural selection, with as much certainty as the balls, after collision, must pass to corners of the table different from those to which they were directed, and so far forth, as the eye is formed by these laws, acting upwards from the nerve merely sensitive to light, we can no more infer design, and from design, a designer, than we can infer design in the direction of the billiard balls after collision. Both are sufficiently accounted for by blind powers acting under a blind necessity. Take away the struggle for life from the one, and the collision of the balls from the other,—and neither of these were designed,—and the animal would have gone on without eyes. The balls would have found the corners of the table to which they were first directed.

While, therefore, it seems to me clear that one who can find no proof of the existence of an intelligent creator, except through the evidence of design in the organic world, can find no evidence of such design in the construction of the eye, if it were constructed under the operation of Darwin's laws; I shall not for one moment contend that these laws are *incompatible* with design and a self-conscious, intelligent creator. Such design, might indeed, have coexisted with the necessity or natural selection; and so the billiard players might have designed the collision of their balls; but neither the formation of the eye, nor the path of the balls after collision, furnishes any sufficient proof of such design in either case.

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One, indeed, who believes from revelation or any other cause, in the existence of such a Creator, the fountain and source of all things in heaven above and in the earth beneath, will see in natural variation, the struggle for life and natural selection, only the order or mode, in which this Creator, in his own perfect wisdom, sees fit to act. Happy is he who can thus see and adore. But how many are there who have no such belief from intuition, or faith in revelation; but who have by careful and elaborate search in the physical, and more especially in the organic world, inferred, by induction, the existence of God from what has seemed to them the wonderful adaptation of the different organs and parts of the animal body to its, apparently, designed ends! Imagine a mind of this skeptical character, in all honesty and under its best reason, after finding itself obliged to reject the evidence of revelation, to commence a search after the Creator, in the light of natural theology. He goes through the proof for final cause and design, as given in a summary though clear, plain, and convincing form in the pages of Paley, and the Bridgewater treatises. The eye and the hand, those perfect instruments of optical and mechanical contrivance and adaptation, without the least waste or surplusage;—these, say Paley and Bell, certainly prove a designing maker as much as the palace or the watch prove an architect or a watchmaker. Let this mind, in this state, cross Darwin's work, and find that after a sensitive nerve, or a rudimentary hoof or claw, no design is to be found. From this point upwards the development is the mere necessary result of natural selection; and let him receive this law of natural selection as true, and where does he find himself? Before, he could refer the existence of the eye, for example, only to design, or chance. There was no other alternative. He rejected chance, as impossible. It must then be design. But Darwin brings up another power, namely, natural selection, in place of this impossible chance. This not only may, but, according to Darwin, must of necessity produce an eye. It may indeed coexist with design, but it must exist and act and produce its results, even without design. Will such a mind, under such circumstances, infer the existence of the designer—God—when he can, at the same time, satisfactorily account for the thing produced, by the operation of this natural selection? It seems to me, therefore, perfectly evident that the substitution of natural selection, by necessity, for design in the formation of the organic world, is a step decidedly atheistical. It is in vain to say that Darwin takes the creation of organic life, in its simplest forms, to have been the work of the Deity. In giving up design in these highest and most complex forms of organization, which have always been relied upon as the crowning proof of the existence of an intelligent Creator, without whose intellectual power

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they could not have been brought into being; he takes a most decided step to banish a belief in the intelligent action of God from the organic world. The lower organisms will go next.

The atheist will say, wait a little. Some future Darwin will show how the simple forms came *necessarily* from inorganic matter. This is but another step by which, according to La Place, 'the discoveries of science throw final causes further back.'

SECOND READER.—It is conceded that if the two players in the supposed case were ignorant of each other's presence the designs of both were frustrated, and from necessity. Thus far it is not needful to inquire whether this necessary consequence is an unconditional or a conditioned necessity, nor to require a more definite statement of the meaning attached to the word *necessity* as a supposed third alternative.

But if the players knew of each other's presence, we could not infer from the result that the design of both or of either was frustrated. One of them may have intended to frustrate the other's design, and to effect his own. Or both may have been equally conversant with the properties of the matter and the relation of the forces concerned, (whatever the cause, origin, or nature of these forces and properties), and the result may have been according to the designs of both.

As you admit that they might or might not have designed the collision of their balls and its consequences, the question arises whether there is any way of ascertaining which of the two conceptions we may form about it, is the true one. Now, let it be remarked that *design* can never be *demonstrated*. Witnessing the act does not make known the *design*, as we have seen in the case assumed for the basis of the argument. The word of the actor is not proof; and that source of evidence is excluded from the cases in question. The only way left, and the only possible way in cases where testimony is out of the question, is to infer the design from the result, or from arrangements which strike us as *adapted* or *intended* to produce a certain result, which affords a presumption of design. The strength of this presumption may be zero, or an even chance, as perhaps it is in the assumed case; but the probability of design will increase with the particularity of the act, the speciality of the arrangement or machinery, and with the number of identical or yet more of similar and analogous instances, until it rises to a moral certainty,—*i. e.*, to a conviction which practically we are as unable to resist as we are to deny the cogency of a mathematical demonstration. A single instance, or set of instances, of a comparatively simple arrangement might suffice. For instance, we should not doubt that a pump was designed to raise water by the moving of the handle. Of course the conviction is the stronger, or at least the sooner

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arrived at, where we can imitate the arrangement, and ourselves produce the result at will, as we could with the pump, and also with the billiard-balls.

And here I would suggest that your billiard-table with the case of collision, answers well to a machine. In both, a result is produced by indirection,—by applying a force out of line of the ultimate direction. And, as I should feel as confident that a man intended to raise water who was working a pump-handle, as if he was bringing it up in pails-full from below by means of a ladder, so, after due examination of the billiard-table and its appurtenances, I should probably think it likely that the effect of the rebound was expected and intended no less than that of the immediate impulse. And a similar inspection of arrangements and results in nature would raise at least an equal presumption of design.

You allow that the rebound might have been intended, but you require proof that it was. We agree that a single such instance affords no evidence either way. But how would it be if you saw the men doing the same thing over and over? and if they varied it by other arrangements of the balls or of the blow, and these were followed by analogous results? How if you at length discovered a profitable end of the operation, say the winning of a wager? So in the counterpart case of natural selection; must we not infer intention from the arrangements and the results? But I will take another case of the very same sort, though simpler, and better adapted to illustrate natural selection; because the change of direction,—your necessity—acts gradually or successively, instead of abruptly.

Suppose I hit a man standing obliquely in my rear, by throwing forward a crooked stick, called a boomerang. How could he know whether the blow was intentional or not? But suppose I had been known to throw boomerangs before; suppose that, on different occasions, I had before wounded persons by the same, or other indirect and apparently aimless actions; and suppose that an object appeared to be gained in the result, *i. e.*, that definite ends were attained—would it not at length be inferred that my assault, though indirect, or apparently indirect, was designed?

To make the case more nearly parallel with those it is brought to illustrate, you have only to suppose that, although the boomerang thrown by me went forward to a definite place, and at least appeared to subserve a purpose, and the bystanders, after a while, could get traces of the mode or the empirical law of its flight, yet they could not themselves do anything with it. It was quite beyond their power to use it. Would they doubt, or deny *my* intention, on that account? No: they would insist that design on my part must be presumed from the nature of the results;—that, though design *may* have been wanting in any one

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case, yet the repetition of the result, and from different positions and under varied circumstances, showed that there *must* have been design.

Moreover, in the way your case is stated, it seems to concede the most important half of the question, and so affords a presumption for the rest, on the side of design. For you seem to assume an actor, a designer, accomplishing his design in the first instance. You—a bystander—infer that the player effected his design in sending the first ball to the pocket before him. You infer this from observation alone. Must you not from a continuance of the same observation equally infer a common design of the two players in the complex result, or a design of one of them to frustrate the design of the other? If you grant a designing actor, the presumption of design is as strong, or upon continued observation of instances soon becomes as strong, in regard to the deflection of the balls, or variation of the species, as it was for the result of the first impulse or for the production of the original animal, &c.

But in the case to be illustrated, we do not see the player. We see only the movement of the balls. Now, if the contrivances and adaptations referred to (p. 229,) really do "prove a designer as much as the palace or the watch prove an architect or a watchmaker,"—as Paley and Bell argue, and as your skeptic admits, while the alternative is between design and chance,—then they prove it with all the proof the case is susceptible of, and with complete conviction. For we cannot doubt that the watch had a watchmaker. And if they prove it on the supposition that the unseen operator acted *immediately*,—*i. e.*, that the player directly impelled the balls in the directions we see them moving, I insist that this proof is not impaired by our ascertaining that he acted *mediately*, *i. e.*, that the present state or form of the plants or animals, like the present position of the billiard-balls, resulted from the collision of the individuals with one another, or with the surroundings. The original impulse, which we supposed was in the line of the observed movement, only proves to have been in a different direction; but the series of movements took place with a series of results, each and all of them none the less determined, none the less designed.

Wherefore, when, at the close, you quote Laplace, that "the discoveries of science throw final causes farther back," the most you can mean is, that they constrain us to look farther back for the impulse. They do not at all throw *the argument for design* farther back, in the sense of furnishing evidence or presumption that only the primary impulse was designed, and that all the rest followed from chance or necessity.

Evidence of design, I think you will allow, every where is drawn from the observation of adaptations and of results, and

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has really nothing to do with any thing else, except where you can take the *word* for the *will*. And in that case you have not *argument for design*, but *testimony*. In nature we have no testimony; but the argument is overwhelming.

Now, note that the argument of the olden time,—that of Paley, &c., which your skeptic found so convincing,—was always the argument for design in the movement of the balls *after deflection*. For it was drawn from animals produced by generation, not by creation, and through a long succession of generations or deflections. Wherefore, if the argument for design is perfect in the case of an animal derived from a long succession of individuals as nearly alike as offspring is generally like parents and grand-parents, and if this argument is not weakened when a variation, or series of variations, has occurred in the course, as great as any variations we know of among domestic cattle, how then is it weakened by the supposition, or by the likelihood, that the variations have been twice or thrice as great as we formerly supposed, or because the variations have been 'picked out,' and a few of them preserved as breeders of still other variations, by natural selection?

Finally let it be noted that your element of *necessity*, has to do, so far as we know, only with the picking out and preserving of certain changing forms, *i. e.*, with the natural selection. This selection, you may say, must happen under the circumstances. This is a necessary result of the collision of the balls; and these results can be predicted. If the balls strike so and so, they will be deflected so and so. But the *variation* itself is of the nature of an origination. It answers well to the original impulse of the balls, or to a series of such impulses. We cannot predict what particular new variation will occur from any observation of the past. Just as the first impulse was given to the balls at a point out of sight, so the impulse which resulted in the variety or new form was given at a point beyond observation, and is equally mysterious or unaccountable, except on the supposition of an ordaining will. The parent had not the peculiarity of the variety, the progeny has. Between the two is the dim or obscure region of the formation of a new individual, in some unknown part of which, and in some wholly unknown way, the difference is intercalated. To introduce necessity here is gratuitous and unscientific; but here you must have it to make your argument valid.

I agree that judging from the past—it is not improbable that variation itself may be hereafter shown to result from physical causes. When it is so shown you may extend your necessity into this region, but not till then. But the whole course of scientific discovery goes to assure us that the discovery of the cause of variation will be only a resolution of variation into two

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factors,—one, the immediate secondary cause of the changes, which so far explains them; the other an unresolved or unexplained phenomenon, which will then stand just where the product, variation, stands now, only that it will be one step nearer to the efficient cause.

This line of argument appears to me so convincing, that I am bound to suppose that it does not meet your case. Although you introduced players to illustrate what design is, it is probable that you did not intend, and would not accept, the parallel which your supposed case suggested. When you say that the proof of design in the eye and the hand, as given by Paley and Bell, was convincing, you mean, of course, that it was convincing, so long as the question was between *design* and *chance*, but that now another alternative is offered, one which obviates the force of those arguments, and may account for the actual results without design. I do not clearly apprehend this third alternative.

Will you be so good, then, as to state the grounds upon which you conclude that the supposed proof of design from the eye, or the hand, as it stood before Darwin's theory was promulgated, would be invalidated by the admission of this new theory.

First Reader.—As I have ever found you, in controversy, meeting the array of your opponent, fairly and directly, without any attempt to strike the body of his argument through an unguarded joint in the phraseology, I was somewhat surprised at the course taken in your answer to my statement on Darwin's theory. You there seem to suppose that I instanced the action of the billiard balls and players as a parallel, throughout, to the formation of the organic world. Had it occurred to me that such an application might be supposed to follow, legitimately, from my introduction of this action, I should certainly have stated that I did not intend, and should by no means accede to, that construction. My purpose in bringing the billiard table upon the scene was to illustrate, by example, *design* and *necessity*, as different and independent sources from which results, it might indeed be identical results, may be derived. All the conclusions therefore that you have arrived at through this misconception or misapplication of my illustration, I cannot take as an answer to the matter stated or intended to be stated by me. Again, following this misconception, you suppose the skeptic (instanced by me as revealing through the evidence of design, exhibited in the structure of the eye, for its designer, God,) as bringing to the examination a belief in the existence of design in the construction of the animals as they existed up to the moment when the eye was, according to my supposition, added to the heart, stomach, brain, &c. By skeptic I, of course, intended one who doubted the existence of design in every organic struc-

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ture, or at least required proof of such design. Now as the watch may be instanced as a more complete exhibition of design than a flint knife or an hour-glass; I selected, after the example of Paley, the eye, as exhibiting by its complex but harmonious arrangements a higher evidence of design and the designer, than is to be found in a nerve sensitive to light, or any mere rudimentary part or organ. I could not mean by skeptic one who believed in design so far as a claw, or a nerve sensitive to light was concerned, but doubted all above. For one who believes in design at all will not fail to recognize it in a hand or an eye. But I need not extend these remarks, as you acknowledge in the sequel to your argument that you may not have have suited it to the case as I had stated it.

You now request me to "state the grounds upon which I conclude that the supposed proof of design from the eye and the hand, as it stood before Darwin's theory was promulgated, is invalidated by the admission of that theory." It seems to me that a sufficient answer to this question has already been made in the last part of my former paper; but as you request it I will go over the leading points as there given with more minuteness of detail.

Let us then suppose a skeptic, one who is yet considering and doubting of the existence of God, having already concluded that the testimony from any and all revelation is insufficient, and having rejected what is called the *a priori* arguments brought forward in natural theology, and pertinaciously insisted upon by Dr. Clark and others, turning as a last resource to the argument from design in the organic world. Voltaire tells him that a palace could not exist without an architect to design it. Dr. Paley tells him that a watch proves the design of a watchmaker. He thinks this very reasonable, and although he sees a difference between the works of nature and those of mere human art, yet if he can find in any organic body, or part of a body, the same adaptation to its use that he finds in a watch, this truth will go very far towards proving, if it is not entirely conclusive, that in making it, the powers of life by which it grew were directed by an intelligent, reasoning master. Under the guidance of Paley he takes an eye, which, although an optical, and not a mechanical, instrument like the watch, is as well adapted to testify to design. He sees, first that the eye is transparent, when every other part of the body is opaque. Was this the result of a mere Epicurean or Lucretian "fortuitous concourse" of living, "atoms?" He is not yet certain it might not be so. Next he sees that it is spherical and that this convex form alone is capable of changing the direction of the light which proceeds from a distant body, and of collecting it so as to form a distinct image within its globe. Next he sees at the exact place where this image must

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be formed a curtain of nerve work, ready to receive and convey it, or excite from it, in its own mysterious way, an idea of it in the mind. Last of all, he comes to the crystalline lens. Now he has before learned that without this lens an eye would by the aqueous and vitreous humors alone form an image upon the retina, but this image would be indistinct from the light not being sufficiently refracted, and likewise from having a colored fringe round its edges. This last effect is attributable to the refrangibility of light, that is, to some of the colors being more refracted than others. He likewise knows that more than a hundred years ago Mr. Dollond having found out, after many experiments, that some kinds of glass have the power of dispersing light, for each degree of its refraction, much more than other kinds, and that on the discovery of this fact, he contrived to make telescopes in which he passed the light through two object-glasses successively, one of which he made of crown and one of flint glass, so ground and adapted to each other that the greater dispersion produced by the substance of one should be corrected by the smaller dispersion of the other. This contrivance corrected entirely the colored images which had rendered all previous telescopes very imperfect. He finds in this invention all the elements of design, as it appeared in the thought and action of a human designer. First, conjecture of certain laws or facts in optics. Then, experiment proving these laws or facts. Then, the contrivance and formation of an instrument by which those laws or facts must produce a certain, sought, result.

Thus enlightened, our skeptic turns to his crystalline lens to see if he can discover the work of a Dollond in this. Here he finds that an eye, having a crystalline lens placed between the humors, not only refracts the light more than it would be refracted by the humors alone, but that in this combination of humors and lens, the colors are as completly corrected as in the combination of Dollond's telescope. Can it be that there was no design, no designer, directing the powers of life in the formation of this wonderful organ? Our skeptic is aware that in the arts of man, great aid has been, sometimes, given by chance, that is, by the artist or workman observing some fortuitous combination, form, or action around him. He has heard it said that the chance arrangement of two pairs of spectacles, in the shop of a Dutch optician, gave the direction for constructing the first telescope. Possibly, in time, say a few geological ages, it might in some optician's shop, have brought about a combination of flint and crown glass which, together, should have been achromatic. But the space between the humors of the eye is not an optician's shop where object-glasses of all kinds, shapes, and sizes are placed by chance, in all manner of relations and positions. On the hypothesis under which our skeptic is making his examina-

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tion,—the eye having been completed in all but the formation of the lens,—the place which the lens occupies when completed, was filled with parts of the humors and plane membrane, homogeneous in texture and surface, presenting, therefore, neither the variety of the materials, nor forms which are contained in the optician's shop for chance to make its combinations with. How then could it be cast of a combination not before used, and fashioned to a shape different from that before known, and placed in exact combination with all the parts before enumerated, with many others not even mentioned? He sees no parallelism of condition then, by which chance could act in forming a crystalline lens, which answers to the condition of an optician's shop, where it might be possible in many ages for chance to combine existing forms into an achromatic object-glass.

Considering, therefore, the eye thus completed and placed in in its bony case and provided with its muscles, its lids, its tear-ducts, and all its other elaborate and curious appendages, and, a thousand times more wonderful still, without being encumbered with a single superfluous or useless part, can he say that this could be the work of chance? The improbability of this is so great, and consequently the evidence of design is so strong, that he is about to seal his verdict in favor of design when he opens Mr. Darwin's book.

There he finds that an eye is no more than a vital aggregation or growth, directed, not by design nor chance, but moulded by natural variation and natural selection, through which it must, necessarily, have been developed and formed. Particles or atoms being aggregated by the blind powers of life, must become under the given conditions, by natural variation and natural selection, eyes, without design, as certainly as the red billiard ball went to the west pocket, by the powers of inertia and elasticity, without the design of the hand that put in motion. (See Darwin, p. 169.)

Let us lay before our skeptic the way in which we may suppose that Darwin would trace the operation of life, or the vital force conforming to these laws. In doing this we need not go through with the formation of the several membranes, humors, &c., but take the crystalline lens as the most curious and nicely arranged and adapted of all the parts, and as giving moreover a close parallel, in the end produced, to that produced by design, by a human designer, Dollond, in forming his achromatic object-glass. If it can be shown that natural variation and natural selection were capable of forming the crystalline lens, it will not be denied that they were capable of forming the iris, the sclerotica, the aqueous humors, or any and all the other parts. Suppose, then, that we have a number of animals, with eyes yet wanting the crystalline. In this state the animals can see, but

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dimly and imperfectly, as a man sees after having been couched. Some of the offspring of these animals have, by *natural variation*, merely, a portion of the membrane which separates the aqueous from the vitreous humor, a little thickened in its middle part, a little swelled out. This refracts the light a little more than it would be refracted by a membrane in which no such swelling existed, and not only so, but in combination with the humors, it corrects the errors of dispersion and makes the image somewhat more colorless. All the young animals that have this swelled membrane see more distinctly than their parents or brethren. They, therefore, have an advantage over them in the *struggle for life*. They can obtain food more easily; can find their prey, and escape from their enemies with greater facility than their kindred. This thickening and rounding of the membrane goes on from generation to generation by natural variation; natural selection all the while "picking out with unerring skill all the improvements, through countless generations," until at length it is found that the membrane has become a perfect crystalline lens. Now where is the design in all this? The membrane was not thickened and rounded to the end that the image should be more distinct and colorless; but, being thickened and rounded by the operation of natural variation, *inherent* in generation, natural selection *of necessity* produced the result that we have seen. The same result was thus produced *of necessity*, in the eye, that Dollond came at, in the telescope, with design, through painful guessing, reasoning, experimenting, and forming.

Suppose our skeptic to believe in all this power of natural selection; will he now seal up his verdict for design, with the same confidence that he would before he heard of Darwin? If not, then "the supposed proof from design is invalidated by Darwin's theory."

SECOND READER.—Waiving incidental points and looking only to the gist of the question, I remark that, the argument for design as against chance in the formation of the eye, is most convincingly stated by you on p. 235-237. Upon this and numerous similar arguments the whole question we are arguing turns. So, if the skeptic was about to seal his verdict in favor of design, and a designer, when Darwin's book appeared, why should his verdict now be changed or withheld? All the facts about the eye, which convinced him that the organ was designed, remain just as they were. His conviction was not produced through testimony or eye-witness, but design was irresistibly inferred from the evidence of contrivance in the eye itself.

Now, if the eye as it is, or has become, so convincingly argued design, why not each particular step or part of this result?

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If the production of a perfect crystalline lens in the eye—you know not how,—as much indicated design, as did the production of a Dollond achromatic lens,—you understand how—then why does not "the swelling out" of a particular portion of the membrane behind the iris—caused you know not how—which, by "correcting the errors of dispersion and making the image somewhat more colorless," enabled the "young animals to see more distinctly than their parents or brethren," equally indicate design —if not as much as a perfect crystalline, or a Dollond compound lens, yet as much as a common spectacle glass?

Darwin only assures you that what you may have thought was done directly and at once, was done indirectly and successively. But you freely admit that indirection and succession do not invalidate design, and also that Paley and all the natural theologians drew the arguments which convinced your skeptic wholly from eyes indirectly or naturally produced.

Recall a woman of a past generation and show her a web of cloth; ask her how it was made, and she will say that the wool or cotton was carded, spun, and woven by hand. When you tell her it was not made by manual labor, that probably no hand has touched the materials throughout the process, it is possible that she might at first regard your statement as tantamount to the assertion that the cloth was made without design. If she did, she would not credit your statement. If you patiently explained to her the theory of carding machines, spinning jennys, and power-looms, would her reception of your explanation weaken her conviction that the cloth was the result of design? It is certain that she would believe in design as firmly as before, and that this belief would be attended by a higher conception and reverent admiration of a wisdom, skill, and power so greatly beyond any thing she had previously conceived possible.

Wherefore, we may insist that, for all that yet appears, the argument for design, as presented by the natural theologians, is just as good now, if we accept Darwin's theory, as it was before that theory was promulgated; and that the skeptical Juryman, who was about to join the other eleven in an unanimous verdict in favor of design, finds no good excuse for keeping the Court longer waiting.

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ART. XXIII.—*Description of three New Meteoric Irons, from Nelson County, Ky., Marshall County, Ky., and Madison County, North Carolina;* by J. LAWRENCE SMITH, M.D., Prof. of Chemistry, University of Louisville, Ky.

Nelson County, (Ky.) Meteorite.—This came into my possession about two months ago, being obtained from a ploughed field where it may have laid for a considerable length of time, attention was drawn to it by a plough striking it; its metallic character leading the neighboring farmer to believe it to be silver.

It is a flattened mass of tough metal, a little scaly at one corner, being 17 inches long, 15 inches broad, and 7 inches in the thickest part, shelving off like the back of a turtle, and weighs 161 lbs.

It is free from any large proportion of thick rust, consequently showing no indications of chlorine. On analysis, the following constituents were found in 100 parts, No. 1 in the table below:

	(1.)	(2.)	(3.)
Iron, - - - -	93·10	90·12	91·12
Nickel, - - - -	6·11	8·72	7·82
Cobalt, - - - -	·41	·32	·43
Phosphorus, - · -	·05	·10	·08
Copper, - - - -	*trace*	*trace*	*trace*
	99·67	99·26	99·45

Marshall County, (Ky.) Meteorite.—A piece of this Meteorite was sent to me from Marshall County, in this State. I have not yet seen the entire mass, which is said to weigh 15 lbs., and to be scaly in structure. It has the usual characteristics of meteoric iron, as seen from the analysis, No. 2.

Madison County, (N. C.) Meteorite.—This meteorite was presented to me some time ago by the Hon. T. L. Clingman, of North Carolina. It came from Jewel Hill, Madison County, of that State. There is a great deal of thick rust on the surface, with constant deliquescence from chlorid of iron. Its form and surface indicates that it is entire, its dimensions are $7 \times 6 \times 3$ inches, with a number of indentations; its weight is 8 lb. 13 oz. Its composition is given in the analysis, No. 3.

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ART. XXIV.—*Description of a new Trilobite from the Potsdam Sandstone;* by FRANK H. BRADLEY, with a note by E. BILLINGS.

[Read before the Am. Assoc. for the Advancement of Science, at Newport.]

CONOCEPHALITES MINUTUS, (n. sp.)

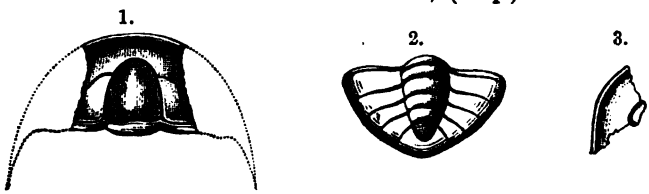


Fig. 1. The head magnified. The dotted lines represent the supposed outlines of the parts not preserved.
Fig. 2. The pygidium magnified.
Fig. 3. A detached cheek, magnified.

Cephalic shield apparently semi-circular, or nearly so; anterior margin as far as preserved with a narrow slightly elevated rim, just within which there is a rather strong groove. Glabella conical, slightly narrowed at the neck segment, three-fourths the whole length of the head, very convex and obtusely carinated along the median line. Neck segment rounded and prominent; neck furrow narrow, but well defined. There are two pairs of deep glabellar furrows which are inclined inwards and backwards at an angle of about 45°; their inner extremities distant from each other rather more than one-third the width of the glabella. The anterior lobe is a little less than one-half the whole length of the glabella, excluding the neck segment; the two posterior pairs are nearly equal to each other. The glabella is distinctly separated from the cheeks by a narrow, deep groove, which extends all round. From the anterior lobe on each side a narrow filiform ridge curves outwards and backwards on the fixed cheek to the edge of the portion preserved. The eyes appear to be situated just where these ridges terminate as represented in figure 1. Judging from the portion of the eye preserved in a detached cheek-plate, its form is semi-annular, and its length at least one-fourth that of the glabella. Its distance must be at least one-half the width of the glabella. Caudal shield nearly as large as the head, its width scarcely equal to half its length; the lobes nearly equal; the middle lobe very convex with five sharp transverse grooves; the side lobes somewhat flat, and each with five grooves.

The largest head discovered is exactly two lines in length.

The course of the facial suture has not been ascertained. The surface of the glabella in one of the specimens appears to be smooth, but in none of the others can it be distinguished.

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The specimens are mostly in a clayey layer, which is full of fragments of all degrees of perfection; in one specimen I count ten heads and three tails, all in a fair state of preservation. In two instances, I have found the casts of maxillary plates, showing distinctly the elevated margin, of one of which I give a figure.

The original specimens were collected, (at High Bridge, near Keeseville, N. Y.,) in August, 1856, while on a geological excursion with Col. Jewett of Albany, but were not recognized until July, 1857, when a second visit to the locality secured a few *casts* in the solid sandstone, none of the clay layer being obtained. By the kindness of Prof. Dana, the specimens were presented at the Am. Assoc. for Adv. of Sci. at Montreal, but were not recognized as belonging to any known species. Since that time, I have looked for descriptions, but cannot find any to correspond.

At the same locality, I also procured the cast, a Pleurotomaria, and one of what seems to have been a plate from the stem of a crinoid.

New Haven, June 15th, 1860.

Note by E. Billings.—Mr. Bradley having favored me with a view of his very interesting specimens, I think there can be no doubt but that they belong to the genus *Conocephalites*. If this reference be correct, then we have at least three, if not four species in North America.

1. *C. antiquatus* (Salter,) described from "a cast in a brown sandstone, said to be a bouldered fragment from Georgia." (See Quart. Jour. Geol. Soc., vol. xv, p. 554.)

2. *C. minutus* (Bradley.) In this species, the form of the glabella and its proportions in relation to the length of the head are almost precisely the same as in *C. antiquatus*, and yet I think the two are not identical, for the following reasons: In the first place, all the specimens of *C. minutus* are of a nearly uniform size, the length of the head being about two lines, and, therefore, it seems probable that they are the remains of adult individuals. The total length would thus be about half an inch, while Mr. Salter's species is full one inch and three-fourths. In the second place, the distance of the eye from the glabella, in *C. antiquatus* is only one-third the width of the glabella, but in *C. minutus* it must be at least one-half the width. These are the only differences that can be well made out, from the imperfect specimens, but they seem to me sufficient to indicate two species. Mr. Salter says further, that the lobes of the glabella in *C. antiquatus* are very obscure, and that the ocular ridge, if any existed, must have been very slight. His specimen was somewhat abraded. In *C. minutus* the ocular ridge is, for so small a species, very strongly defined, and the glabellar furrows are so deep

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that it would require a very considerable amount of abrasion to obliterate them.

3. *C. Zenkeri*, (n. sp.) This is a new species recently discovered in the magnesian limestone near Quebec. It will probably be described in the next No. of the Canadian Naturalist and Geologist.

4. There is in the collection of the geological survey of Canada, a plaster cast of the surface of a fragment of rock which holds four specimens of a trilobite, each about the size of *C. antiquatus*. They appear to me to belong to the genus *Conocephalites*. The original specimen was collected in Newfoundland, in the same slate that holds *Paradoxides Bennettii* (Salter,) and I am informed that it is in the possession of a gentleman who lives somewhere in the United States, but whose name or place of residence, I have not been able to ascertain.

Of the above four species, Mr. Bradley's is at present the most important as it fixes indisputably, at least one point in the geological range of the genus on this side of the Atlantic. In Europe, *Conocephalites* has not been found out of the *primordial zone* of Barrande, but the Quebec and Keeseville specimens show that here it reaches the Lower Silurian.

Montreal, July 25th, 1860.

ART. XXV.—*On the Combustion of Wet Fuel, in the Furnace of Moses Thompson;* by B. SILLIMAN, Jr., Prof. Gen. and App. Chem. in Yale College.

[Read before the Am. Assoc. for the Adv. of Sci., at Newport, August, 1860.]

IN all ordinary modes of combustion, it is well known that the use of wet fuel is attended with a very great loss of heat, rendered latent in the conversion of water into steam. As the most perfectly air dried wood still contains about 25 per centum of water, according to the experiments of Rumford, the term *wet fuel* might seem appropriate to all fuels, but mineral coal and charcoal. But technically, this term is restricted to substances like peat and those residual products of the arts which, like spent tan, begasse and dye stuffs contain at least one half and often more than half of their weight of water. Until a recent period the attempt to consume these products as sources of heat has been attended with uneconomical results, or total failure. It is the object of this paper to describe a mode of combustion in which by a modification in the form of the furnace the combustion of wet fuel is not only rendered consistent with the best economical results; but which as it involves chemical reactions never before, it is believed, successfully applied for such

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purposes, is deserving of particular notice from a scientific as well as from a practical point of view.

It is a well established fact in chemistry, that the affinity of carbon for oxygen, at high temperatures is so strong, that if oxygen is not present in a free state, any compound containing oxygen, which happens to be present is decomposed, in order to satisfy this affinity. This fact is well illustrated in the familiar case of the Blast Furnace where this affinity is employed to deprive the ores of iron of their oxygen in the process of reduction to metallic iron.

In the first stages of combustion, in wet fuels, the chief products given off are steam from the drying of the wet mass, smoke or volatilized carbon and oxyd of carbon, with, of course, a variable proportion of carbonic acid and carburetted hydrogen. These products in all ordinary furnaces, pass on together into the stack, carrying with them the heat which they have absorbed and rendered latent. The problem presented is then to recover the heat thus locked up and lost, and by the furnace now under consideration *this is accomplished by shutting off almost entirely the access of the outer air and causing the wet fuel to supply its own supporter of combustion drawn from the decomposition of the vapor of water at a high temperature by its reaction with free carbon and the oxyd of carbon.*

The practical solution of this problem was first successfully accomplished, as appears from a decision of Patent Commissioner Holt, by the late Moses Thompson, in 1854. The controversial questions growing out of this invention, are entirely foreign to our present purpose and in no way affect its practical or scientific value. Suffice it to say, in passing, that we find in this invention another instance of a truth already so often signalized in the history of inventions, that important results are often obtained, of the highest value in promoting material prosperity and the welfare of society, by those who are guided in their search only by the result in view, and not by any exact knowledge of the scientific principles involved.

Mr. Thompson seems to have been inspired with the conviction that if he could bring the products from the combustion of wet fuel together in a place, hot enough for the purpose, and from which the atmospheric air was excluded, they would, as he expresses it in his patent, mutually "*consume each other.*" This notion was realized, and the reaction secured between the elements of water and the carbon of smoke, or the oxyd of carbon in a part of the furnace called by the inventor, the *mixing chamber.*

Wherever that place may be situated, or however constructed, the one essential thing about it, is, that it should be a very hot place, and one to which the atmospheric air can have no direct access, until it has passed by, and through the burning fuel. It

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is in fact a retort or place for combination and reaction, and may be a distinct chamber or flue, or only a recess or enlargement greater or less of the main furnace. Wherever it may be placed, or however built, it must meet the essential conditions of a high temperature, and of atmospheric isolation. In this mixing chamber, then, the important chemical reaction before insisted on, must be set up. The vapor of water is decomposed, furnishing its oxygen to the highly heated carbon to form carbonic acid, while the oxyd of carbon is in like manner exalted to the same condition, and any excess of carbon forms with free hydrogen, marsh gas or light carburetted hydrogen. The vapor of water is thus made to give up not only its constituent elements to form new compounds with oxygen, producing in the change great heat, but a great part of the heat absorbed by the water in becoming steam is also liberated in this change of its physical and chemical condition. Moreover as all these products of combustion and of chemical reaction pass together over the bridge-wall of the furnace into a space from which atmospheric air is not excluded, it then and there happens that any free hydrogen, light carburetted hydrogen or oxyd of carbon which have previously escaped combustion, take fire and burn, yielding up their quota of heat to the general aggregate.

Such is the intensity of heat in that portion of the furnace where these reactions take place that only the most solid structures of refractory fire bricks will endure it, and the color seen throughout that portion of the furnace is of the purest white.

In view of the facts already stated it is easy to understand why it is that when the reactions described are once set up, the admission of a free current of atmospheric air should immediately check the energy of the combustion and soon result in total suspension of the peculiar energy of this furnace. The air containing only one-fifth part of its bulk of oxygen gas, the active agent in combustion, the access of so large a proportion of cold air—four-fifths of which are not only indifferent but positively prejudicial from the quantity of heat it absorbs,—it happens that the temperature of the mixing chamber is rapidly reduced below the point at which carbon can decompose vapor of water and the instant that point is reached the arrival of fresh supplies of steam completes the decline of energy and the furnace commences forthwith to belch forth from its stack dense volumes of smoke and watery vapor. When in proper action not a particle of smoke is visible from the stack of a furnace in which wet fuel is burning, and what is more remarkable the reactions are so evenly balanced that no wreaths of watery vapor are observed, while in the earlier stages of combustion before the proper temperature in the mixing chamber is reached, both these products are seen in great abundance.

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DESCRIPTION OF THE FURNACES.

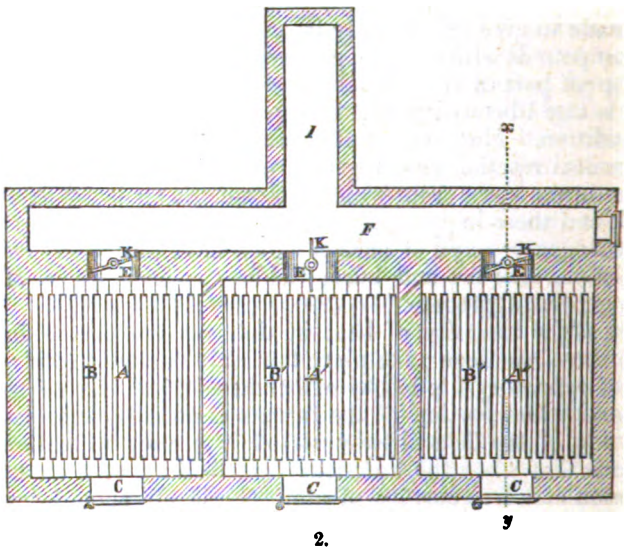
1st. Furnace for combustion of wet tan, sawdust, &c.

Fig. 1, is a horizontal section of a furnace constructed according to the specifications of Thompson's first patent, (issued April 10th, 1855).

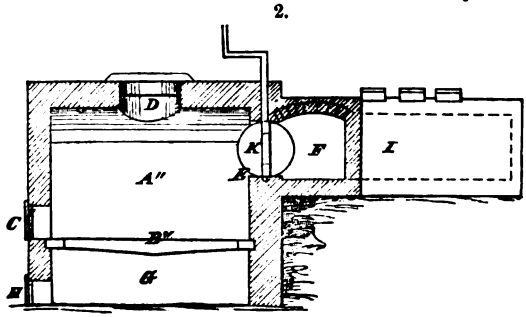
Fig. 2, is a vertical section of the same in the line *x y*, of figure 1.

Similar letters indicate corresponding parts in both figures.

1.



2.



The furnace shown in these figures has three square or oblong fire chambers, *A*, *A'*, *A''*, side by side, experience having shown that not less than three compartments are required to secure the best results in the practical working of the furnace, although in some cases two may suffice, but frequently more than three are

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desirable. The fire chambers are furnished with grate bottoms B, B', B'', of fire bricks, and are arched at top. Each chamber has a door C, in front for lighting and tending the fire. This opening is seldom used after the furnace is once set in action. The wet fuel is supplied through the opening D at top. E is an opening at the back of each chamber leading to the flue F, or the mixing chamber. This opening may be provided with a damper K (which must be of fire clay, if of iron the intense heat soon destroys it). Each chamber has a separate ash pit G with its opening H. This although called an ash pit serves a most important purpose in the economy of the furnace as a receptacle for the burning coals which constantly fall into it from the lower part of the wet mass above, as will be more particularly explained beyond.

If the furnace is used for generating steam the best place for the boiler is over the flue I. The inventor remarks in his first patent that the current from the mixing chamber in passing to the place of use, in the case of burning wet tan or other very wet fuel, should descend or pass under a bridge to the place of use equal to about one half of the depth of the burning chamber between the grate and the crown, then rise to the place of use. In case of dry or nearly dry fuel, such as green wood and saw dust, the current should rise immediately after leaving the burning chamber to the place of use.

The mode of conducting the operation of the furnace is as follows: fires being lighted in all the fire chambers with dry fuel and the masonry heated to a high degree, two of the three chambers A A' are fed with wet fuel and have their ash pits closed. The other fire chamber is kept in action by dry fuel (its ash pit door being proportionally open) until the process of combustion sets in over the surface of the pile of wet fuel resting on the grates of the other furnaces. As soon as this is the case, wet fuel is added by degrees to the third fire chamber, the ash pit door being at the same time closed. If things have been properly managed so far, the process will now continue by the addition of new portions of wet fuel to each furnace in succession or alternately. The temperature of the mixing chamber F is now seen to be of the most perfect whiteness and not a visible particle of smoke issues from the stack.

Before discussing this process more in detail, let us first consider the Inventor's description of his furnace as designed more particularly for the consumption of *begasse* or crushed cane stalks.

2. *Furnace for Combustion of Wet Cane Begasse.*

Fig. 3 is a sectional side view, the interior and exterior form of the furnace, and its several parts according to the specifications of Thompson's patent of Dec. 15, 1857.

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3.

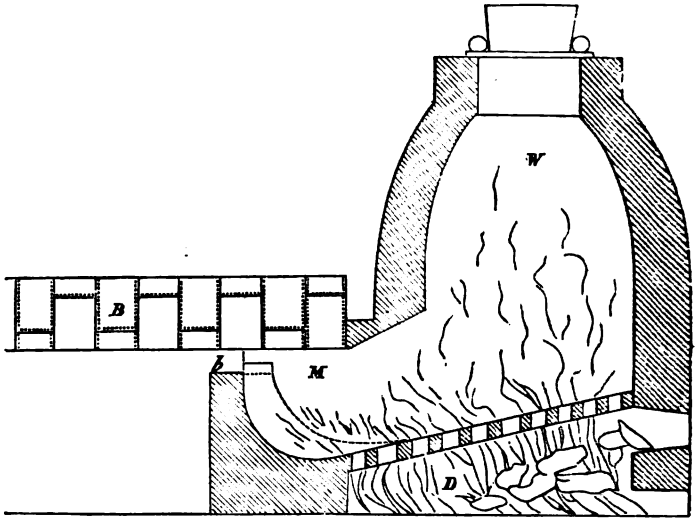


Fig. 4 is a front sectional view of the same, showing the combination of two double furnaces.

4.

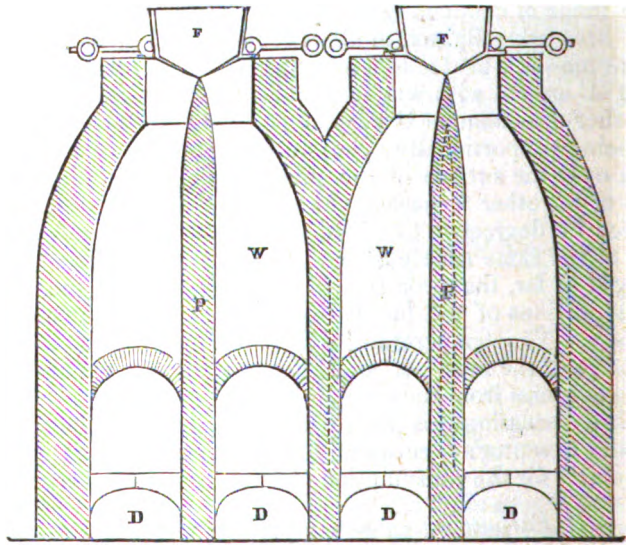
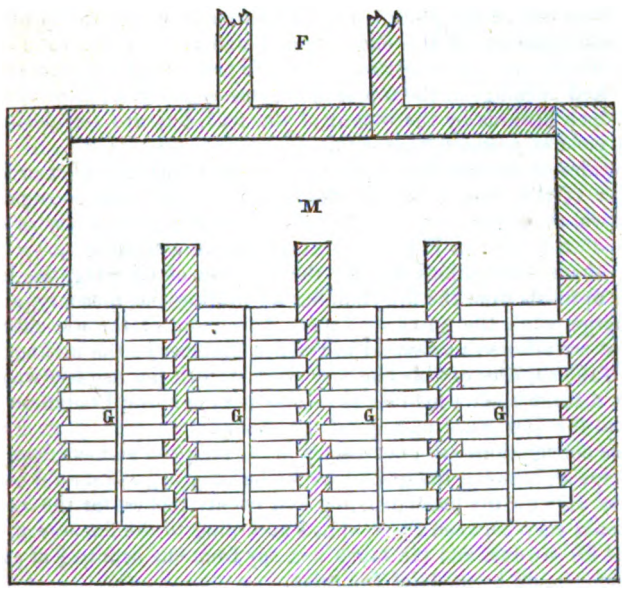


Fig. 5 is a horizontal view of the grate and its relation to the mixing chamber *M* and flue *F*.

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5.



Here let the Inventor speak for himself in the language of the patent last named.

"I build two furnaces side by side, each nearly square in its horizontal section. Towards the top I draw in the wall in such manner as to form a kind of dome with a sufficient opening at top to feed the bagasse. The outer walls of these furnaces should be from 24 to 30 inches thick and built with a special view to rendering them non-conducting, the wall near the top, and the partition between the two furnaces may be thinner. In each furnace chamber there should be a partition of fire brick extending across it from front to back and rising nearly to the top, dividing it into two nearly equal parts. The whole interior of the furnace should be of fire brick. The main chamber of each furnace should be divided into two parts—upper and lower—by a fire brick grate about one-fifth the height of the furnace above the hearth, the back end of the grate being a little lower than the front. The bottom of the lower chamber may be a grate with an ash pit, but a hearth is much better.

In each furnace at the front, on each side of the central partition and immediately under the front end of the grate should be doors for feeding wood or other dry fuel, and directly under these doors at the hearth of the lower chamber should be draught openings capable of adjustment to support combustion in the lower chamber.

Extending across the back of both furnaces, and opening into both by flues is a mixing chamber into which all the gases from both furnaces enter in a highly heated state and mix and consume each other on their way to the boiler and stack. This chamber should be about one-half the

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capacity of all the fire chambers and it should extend down about as low as the back end of the grate. The flue through which the products of combustion pass out of this chamber and under the boiler should be in section about one square foot to forty cubic feet of mixing chamber.

The feed openings at the top of the furnaces should be closed by doors which open inwards by the weight of the feed, but are self-closing, and do not yield to pressure from within.

The sides of the interior of the upper or wet fuel chamber or drying chamber of the furnace, except the front and back, are corrugated up and down, as also the sides of the central walls or partitions as shown by the dotted lines in Fig. 4, the corrugations extending down to the grate; these corrugations are for the purpose of allowing the heat to radiate upwards from the fire chamber for heating the masonry, and the wet charge, while the gases or vapors driven out of the wet charge by the heat are allowed to descend to the fire chamber or the mixing chamber. If the surfaces of this masonry were smooth the bagasse would lie against them in such a manner as to obstruct the upward radiation of the heat and the downward passage of the vapors.

These corrugations are unnecessary in burning tan and sawdust.

The spaces between the grate bars for burning bagasse should be about 6 inches wide for the finest grinding and twenty inches for the coarsest, and should vary between these widths according to the fineness of grinding, but for sawdust and tan much less, say from one inch to $\frac{3}{4}$ of an inch. The grate should be made of fire brick.

The operation of my furnace is as follows: A hot fire of dry fuel is kindled in the lower or fire chambers of the furnaces and after it has been continued till the masonry is well heated, the chamber above the grate is fed with the begasse or other wet fuel. This hot fire in the fire chamber, especially towards the front of it under the principal mass of the wet fuel, must be preserved throughout the operation. The heat from the masonry and the fire chamber will be communicated to the wet fuel which will cause steam and other gases to issue from it and mix with the intensely hot gases of combustion from the fire chamber, and in a short time the mixing chamber will present intense combustion and heat, the dampers of the fire chambers being partially closed. The lower part of the wet charge will by degrees become dry and charred and will fall through the grate prepared as above unto the fire chamber and supply or nearly supply the place of other dry fuel in preserving the fire in this chamber and the wet fuel being from time to time supplied will furnish in a highly heated state aqueous vapors which descending through the corrugations and otherwise into the fire chamber and mixing chamber, will be decomposed, furnishing much oxygen to the fire, and supply the oxygen necessary to combustion of all the combustible gases issuing from the fire chamber. If by accident the fire in the lower part of the furnace should predominate, the draught should be diminished and more wet fuel added, and, if by accident, the fire in the fire chamber should become too much cooled down the draught should be let on, and any deficiency of dry fuel should be supplied to the fire chamber. Under proper management little or no dry fuel need be fed to the fire chamber after the operation is fairly commenced, the charred matter falling through the open grate will supply its place; and the caloric thus produced by the combustion of wet

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fuel, will be vastly greater than from the same quantity by measure of the same fuel when dry. In the fire chamber and in the mixing chamber under intense heat the carbonaceous gases will decompose the steam from the wet fuel and effect complete combustion.

When the operation is fairly commenced if the water in the wet charge amounts to say fifty per cent by weight of the fuel, the dampers of the fire chamber should be nearly or quite closed to exclude the air; vapor from the wet charge will then descend through the corrugations and otherwise into the fire chambers and support the combustion therein, while other portions of the vapor will enter the mixing chamber and complete the combustion there. If the fuel, however, contains much smaller quantities of water, more air in proportion should be admitted at the damper, the object being to admit no more air than will supply the deficiency of the vapor.

In the drawings, *D* represents the chambers for the dry fuel, *W* those for the wet, *M* the mixing chamber, the dotted line *m* in Fig. 3 limits it for the wettest bagasse, P the partition, F the feed openings for the wet fuel with their doors, B the boiler, *b* the bridge. Little if any of the boiler should extend over the mixing chamber. If any considerable portion of the mixing chamber is covered by the boiler its cooling influence will prevent the decomposition of the the vapor and defeat the object of my invention. Great care should be observed in giving proper dimensions to the mixing chamber, for the perfection of the combustion and the efficiency of the furnace depend greatly upon it. The principal object of this chamber is to give the combustible carbonaceous gases from the fire, and the aqueous gases from the mass of wet fuel an opportunity of mingling together in such a manner and under such circumstances that the aqueous vapor will be decomposed by the carbonaceous gases, and its oxygen given out to complete the combustion of the carbon, without the introduction of air into the mixing chamber, thus saving the caloric previously communicated to the wet charge, while drying it and charring its lower portions, and avoiding the cooling influences of cold air. This can take place effectually only in the presence of a high degree of heat and in the absence of a supply of free oxygen. If this chamber be too small to receive these gases as fast as the furnace is able to produce them the operation will of course be choked and impeded. If the chamber is larger than can be kept densely filled with these gases, of course atmospheric air will be found there at the commencement, and will continue to find its way into the chamber, and while atmospheric air is present, the carbonaceous gases will take their oxygen from that source principally instead of decomposing the steam, and the heat in the chamber will be much diminished and the large quantity of nitrogen $\frac{4}{5}$ contained in the air, which is neither a combustible nor a supporter of combustion, will at once greatly increase the volume of gases to be sent forward to the stack and proportionably decrease its temperature; and when the chamber becomes very large the cooling influences become so great that combustion will immediately cease, and smoke mingled with steam oxygen and nitrogen, will go forward, thus wasting the fuel and imparting only a faint degree of heat to the boiler.

I have therefore fixed the size of the mixing chamber by many careful experiments—and that given above will produce the desired effect with

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wet bagasse. For dryer fuels furnishing less vapor, the mixing chamber should be proportionably increased in size to supply the defficiency with air and to effect complete combustion. Rules more precise would be inconsistent with the nature of the subject.

A large and hot fire should always be preserved in the fire chamber below the grate, and directly under the charge of wet fuel, for the purpose of driving the vapor out of it and charring its lower portion—and the grate is left much more open than in furnaces for burning dry fuel of the same size, for the purpose of allowing the charred portions of the wet charge to fall through to supply fuel for this fire as fast as it becomes fit for that purpose, thus consuming the mass with little or no expenditure of other fuel.

What I claim as my improvement in furnaces for burning bagasse and and other fuels too wet to be conveniently burned in the usual way and well known ways is:

First, the combination of two chambers, the one above the other, and separated by a grate, the lower one for the combustion of any known dry carbonaceous fuel, and the upper one in immediate proximity therewith to receive heat therefrom for heating and drying the charge of wet fuel, with a mixing chamber, into which both continuously and simultaneously discharge their gases before reaching the thing to be heated, for mingling and mutual combustion.

I also claim in combination with said fire chamber and wet fuel chamber or drying chamber making the grate upon which the wet charge rests sufficiently open to allow the lower portion of the wet charge as it becomes dried and charred to fall through into the fire chamber and keep a hot fire therein, supplying the place of other dry fuel, while the uncharred portions of the wet fuel is properly supported by the grate till dried as described.

I also claim placing the mixing chamber of combustion in substantially the same position described relatively to the fire, and the wet charge, so that the products of combustion from the dry fuel may pass along the lower part of the wet charge, drying and charring it on their way to the mixing chamber, and reach it without being in any considerable degree obstructed or cooled by the wet charge substantially as shown.

I wish it distinctly understood that I make no claim to any of the parts or combination above specified except in their application to the preparation and combustion of wet fuels."

It will be observed that in this mode of combustion the wet fuel is subject to a constant process of distillation by the fire in the ash pit. The products of this distillation react on each other in the mixing chamber in the manner already described, while at the same time a portion of watery vapor is decomposed in the ash pit.

Theoretically no more heat can be generated in this mode of combustion than is consumed in the transformation of water into steam and the conversion of fixed into volatile products. But it is by no means a matter of indifference whether the oxygen requisite for complete combustion is drawn from the atmosphere or is derived from the decomposition of water by carbon

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and its oxyd. In the former case, not only is there a great loss of heat carried away by the inefficient nitrogen of the air, but the diluted oxygen can never produce so intense a heat with the carbon as is the result of the reaction of the nascent oxygen with that element. Although Mr. Thompson was no chemist, he did not fail with his natural acumen to perceive this advantage and in his earliest patent he remarks: "After ample experiments I have discovered that any results that can be produced, by the use of dry fuel are inferior (to those obtained from my process) in proportion to the quantity used, and that results like mine can only be obtained by the use of wet fuel * * * fed into an intensely heated chamber: under such circumstances the water in the fuel in presence of the carbonaceous substances in the furnace will be decomposed, giving its oxygen to the carbonaceous matter, dispensing with a draft and its cooling and wastful influence and rendering the combustion so perfect that no smoke is visible."

Although this mode of combustion of wet fuel is now in use on many sugar plantations in Louisiana, and in some Tanneries of Pennsylvania and New York, no notice of it has so far as I am aware appeared in the scientific Journals. I am not without personal experience of its operation on a large scale, having in 1857 enjoyed the opportunity of studying carefully the management of one of Thompson's furnaces in three compartments (similar to Figs. 1 and 2) built for the combustion of wet peat. That fuel contained over seventy-five per cent of its whole weight of water and was too wet for the best results. But with the use of one-fourth part of dry wood, even this extremely wet and otherwise valueless fuel was rendered efficient, three cords (of 128 cubic feet) of wet peat and one cord of dry wood doing the work of four cords of dry wood in driving a steam boiler.

ART. XXVI.—*Note on a case of Artificial Crystallization of Metallic Copper and Dinoxyd of Copper;* by J. W. MALLET.

A FLASK, in which nitric oxyd had been prepared from nitric acid and scraps of copper, was allowed to remain over night by the pneumatic trough—the end of the gas-delivering tube dipping under the surface of the water. On the next day several scraps of copper were observed dotted over with very minute but brilliant crystals of metallic copper, which under the microscope proved to be octahedrons and combinations of the octahedron with the cube and dodecahedron, of various sizes—the largest measuring about twelve-hundredths of a millimeter along an octahedral edge. Along with these crystals of copper there were little cubes of the dinoxyd of copper in great abundance—

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the latter not more than two or three hundredths of a millimeter on the edge, translucent, and of a splendid garnet red color.

On examination it became evident that these two bodies had been deposited in the crystalline state in consequence of the formation of the so-called "Bucholzian circuit"—one solid and two liquids so arranged as to produce electric action. As the flask cooled down the water from the peneumatic trough gradually rose in the tube, and in time ran down into the flask. The latter being in an inclined position the water flowed gently down the neck and formed a distinct stratum above the strong solution of nitrate of copper. Some of the scraps of metal which had projected above the surface of the solution, were now immersed, partly in the latter and partly in water, and it was upon the lower portion of each of these scraps—the part immersed in solution of nitrate of copper—that crystallization had taken place.

The deposition of metallic copper under similar conditions was observed by Bucholz, and other metals may, as is well known, be thus crystallized from solutions of their salts. The crystallization of the dinoxyd of copper was effected by Becquerel, but the arrangement of substances in his experiment was different from that now noticed—he placed a strip of copper in a saturated solution of the nitrate, the lower end of the strip touching some protoxyd of copper at the bottom of the solution. The crystals which he obtained were octahedral, not cubic.

Art. XXVII.—*Review of Dr. Antisell's Work on Photogenic Oils, &c.*

[Concluded from page 121.]

In describing the methods of purification proposed by Selligue, we shall make no attempt to follow their various details, our limited space compelling us to content ourselves with only the broadest generalities. Selligue sets forth at length two methods: 1st. A cold treatment which consists in agitating the oil with sulphuric, muriatic, or nitric acid. This agitation should be thorough, he says, and should be continued for a longer or shorter time according to the nature and quantity of the matter treated. Here follows a description of his agitators. After several hours' repose, the oil may be decanted, except from muriatic acid in which case more time and a larger amount of acid is required. After the oil has been thus separated from the deposit of tar, the acid remaining in it must be neutralized by means of an alkali. I prefer, says Selligue, to employ the lye of soap-boilers marking 36° to 38° [B. ?], since it is easy of application, and produces a sure effect; I thus precipitate together the coloring matter and tar which would otherwise have remained in the oil. The oil is then decanted: if it is the first distillation of the crude oil I do not allow the mixture to subside entirely, preferring to leave a portion of the alkali mixed with the oil, and to distil off only $\frac{4}{5}$ths of the latter. * * * When the soda lye—in quantity slightly greater than is necessary to neutralize the acid—is added, the liquid must be agitated violently in order that each particle of the oil may be brought in contact with the alkali; this agitation must be continued until the color of the oil undergoes change.

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The oil becomes less odorous and less highly colored after each such "cold treatment."

After having been allowed to separate from the lye, the oil is decanted off; if it has not lost much of its color the process has been badly conducted. It should be stated that the oil must not be agitated several times with the alkali, for, by so doing, the dark color of the oil would be restored. * * * As for the residues of the soda treatment, continues Selligue, they should be allowed to stand at rest during some days beneath a portion of oil, which will protect them from contact with the air; the clear lye at the bottom being then drawn off may be used for other operations, while the remainder is a soap, containing excess of alkali. By adding to it a little grease a soap can be made, or by adding water, grease may be separated. This grease is similar to that used for wagons.

2d. A warm treatment which follows the cold, and consists of a series of fractional distillations,—special operations for the purification of the "light-stuffs," being resorted to. For the details of these we must refer to the original specification of Selligue—a truly classical document which should be read by every one interested in the manufacture of coal-oils.* Nor will our limited space permit us to cite the detailed "example" of his treatment which Selligue has described. We trust that we have already written enough to enable the reader to judge whether or no Selligue understood his business.

As for paraffine, Selligue obtained it by subjecting the oil to a low temperature in order that this substance might crystallize. The mixed oil and paraffine was then thrown upon fine metallic filters through which the oil flowed while the paraffine was separated. Or one may separate, he says, the oil by imbibition, but this occasions a great loss of oil and also requires more labor. * * With this specification the scientific discussion of the subject by Selligue appears to have ceased, yet in the same year he replies† to a note published by Chenot‡ who asserted that the oil of shale often contains arsenic, denying that arsenic can be found in the products from his own establishments. He again describes the locality and geological position of his shale, the method of distillation employed,—how the temperature is gradually elevated, &c.

This is of interest as showing that the manufacture of coal-oil in France was no ephemeral fancy, but for many years was a well established branch of industry. In this connection the scientific research, upon the commercial products of the distillation of bituminous shale, of Saint-Evre§ should also be mentioned. Contemporaneous with Selligue we find other inventors occupied with the same subject. Thus Holthorp,‖ in 1841, claims that he has first discovered a means of purifying the fluid substance, which he calls "schiste," resulting from the distillation of coal or of bitumen. His attention was evidently chiefly devoted to the volatile naphthas, but he also obtained paraffine.

Guillard Meynier,¶ in 1842, speaks of the fixed oil from shale, telling us that it may be used for lighting or lubricating and that paraffine may be separated when the oil is cooled or treated with alcohol.

In the same year Bonnet** in treating of liquids suitable for lighting incidentally mentions eupion and paraffine.

Nor should we omit to mention the very interesting article upon *Hydrocarbures Liquides*, by A. Mallet (in Laboulaye's Dictionnaire des Arts et Manufactures, 2d Ed., Paris, 1854††), in which Selligue's processes are incidentally

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* A tolerably accurate English translation of this important patent may be found in the specification of M. A. B. B. Du Buisson, 1845; specification No. 10,726 of the English Patent Office.

† *Comptes Rendus*, 1845, xx, 573.

‡ *Ibid*, xx, 806.

§ *Comptes Rendus*, 1849, xxix, 339.

‖ *Brevets d'Invention*, liii, 263.

¶ *Brevets d'Invention*, lxxviii, 91.

** *Ibid*, lxxix, 63.

†† A portion of this article, which directly refers for the most part only to the volatile products suitable for "burning fluids," which may be obtained in any way from coal, is also contained in Dingler's *Polytechnisches Journal*, 1847, cvi, 128.

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described. After discussing in detail the light volatile products obtained by distilling coal-tar, he says, we have still to speak of the carbo-hydrogens from shales; a branch of industry which we owe entirely to Selligue—cut off, alas! prematurely, in the midst of his career so full of discoveries and of useful works. As is well known, he obtained by distilling shales from the environs of Autun: I, volatile ethereal oils, II, fixed oils, III, oils combined with paraffine from which he prepared grease for carriages, IV, paraffine suitable for making candles, &c. Among all these bodies, Mallet continues, we have only to occupy ourselves with the volatile oils. Further on M. remarks that the acid and alkaline treatment used by Selligue is similar to that proposed by Barral for products from coal-tar. Thus far, says Mallet, these hydro-carbons have found no application,* partly on account of their insupportable odor when not purified and partly on account of their high price—about $10·00 the hundred lbs.—when purified.

We have been at no pains to ascertain whether the industrial distillation of shales, so well grounded by Selligue, has been continued in France without interruption up to the present time, for we know of no reason to doubt the fact. Certain it is that coal-oils produced by French manufactories were exhibited, at the Exposition Universelle at Paris in 1855, and likewise in 1851 at London.†

To any one familiar with the extreme slowness with which the practical applications of chemistry are even now imparted to, and recorded by, scientific writers, it would have been no matter of surprise if the results obtained by Selligue had remained uncopied upon the records of the French patent office. Such however was not the case. From the preceding citations it will be seen that his results were published in various well known journals and were widely diffused. Dumas, in his *Traité de Chimie Appliquée aux Arts*,‡ expressly calls attention to them. They are also noticed in the *Handwörterbuch der reinen und angewandten Chemie, von* Liebig, Poggendorff u. Wœhler, 1844, iii. 364. What we cannot explain is the apparent ignorance of these facts which was exhibited by several of the leading chemists of Great Britain on the occasion, of a trial,§ Young, *v.* White and others held in June, 1854, in the Court of Queen's Bench before Lord Chief Justice Campbell.

Several patents for the production of oils [coal-oils] from bituminous substances were meanwhile obtained in England. Butler,‖ for example, in describing his "improvements in the manufacture of oil and gas" proposes to distil bituminous shales, &c. for the purpose of obtaining oil and gas free from naphthaline. The shale, best after wetting it with water if the principal object is to obtain oil, is distilled in common gas retorts under which a gentle fire is lighted. As soon as oil begins to flow over freely the fire is to be increased and the retorts brought to a red heat; a large quantity of gas is thus obtained which is collected in a gas holder. The rough oils, as Butler informs us, may be purified by washing with sulphuric acid, filtration, &c., or they may be used in the rough state for making oil-gas. The oils in their rough state are often found entirely free from oxygen, and if obtained by the process described never contain so much as is contained in the coal-tar obtained in the coal-gas works where the coal is thrown into retorts already brought to a red heat. These oils in their rough state are further distinguished from coal-tar by their containing no naphthaline. Moreover the less volatile part of the

* It will be observed by the reader of Mallet's treatise that he is interested only in a single branch of the subject, viz., the volatile naphthas—"light stuffs," just as we are here giving prominence to another portion of it, viz., the fixed, or paraffine-oil; and that he holds the naphthas from shale in small repute, since in his opinion they can never compete in the matter of cost with those from coal-tar.

† A. U. Morean (No. 1361, Cat. 9), *Bas-Rhin.*

‡ Paris, 1844, t. vii, p. 390; also t. iii, p. 315 of the Liege edition; and B. vii, S. 510 of the German translation.

§ Reported in Barlow's London Journ. of Gas Lighting, Aug. 10, 1854, vol. iii, p. 508.

‖ Patent granted Jan. 29, 1853. Specification No. 6375 of the English Patent Office.

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oil [No. 2] offers another characteristic feature; if after being drawn off and distilled, and if in this latter process the more volatile or first proceeds, say one half of the quantity acted upon, be set apart and the remaining half exposed to a low temperature, there will soon appear in this part of the distilled oil small flakes of a white, odorless, and light substance which is a compound of carbon and hydrogen [paraffine]. The familiarity with the subject, somewhat remarkable in view of the early date of his patent, which Butler exhibits cannot fail to strike the reader. This inventor was however unfortunate in the idea of trying to make at the same time oil and gas—in endeavoring to reconcile two antagonistic processes.

In 1841, Sept. 4, Count de Hompesch of Prussia* specified certain "improvements in obtaining oils and other products from bituminous matters."† It is well known, he says, that oils may be obtained from these substances but from the imperfection of the processes now used the quantity obtained is small, the quality inferior, and the smell noxious. My invention consists in an improved process, whereby I increase the quantity, improve the quality, and remove or greatly modify the smell. I have found by experiment, he continues, that the oil from shale, &c., possesses three different characters which may be called essential oil, intermediary fat oil, and thick oil, and these oils I separate by means of peculiar apparatus—which he describes in detail. In distilling shales heat is applied until the temperature reaches 100° R. = 257° F., at which temperature essential oil will pass over. The charge, after having been subjected to this temperature for half an hour, is pushed forward in the retort which is now subjected to a heat of 200° R. = 482° F., by which increased heat the intermediary or fat oil is obtained. After having subjected the charge to this increased temperature for half an hour the workmen again pushes the charge further on in the retort where it becomes of a red heat; the vapor now given off yields the thick oil. The carbonization is now complete; and I obtain these three separate oils by the gradual increase of the heat; and I effect this distillation without decomposition of the substance, the vapors escaping from the retort as fast as they are formed.

The essential oil is separated from the fat oil by exposing the mixture to a current of steam by which the more volatile oil is carried off. The oil [fixed] thus prepared must be filtered and is then ready for application to all kinds of machinery, being very fat, works without friction and leaves no sediment. The essential oil is collected and subjected to further treatment.

The specification of Du Buisson‡ for improvement in the distillation of bituminous substances, is an almost literal translation of Selligue's last patent. Indeed, Du Buisson tells us that the extensive works at Autun, Department of the Saone and Loire, France, are partly his property and that he has the management of them as chemist. He affirms moreover that the most important results have there been attained — results which place the distillation and treatment of schistus among the most useful and productive of chemical manufactures.

Since we have already extracted largely from Selligue's specification it is unnecessary to cite more of it here. It is a little curious that this most important patent is not mentioned in Dr. Antisell's "list of English Patents" (p. 141).

The well known attempts to prepare paraffine and oils from peat§ need not be discussed here. Another patent, not mentioned in Dr. A.'s list, is that of

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* Specification No. 9060 of the English Patent Office.

† In a "memorandum of alteration," dated July 5, 1842, de Hompesch claims the right of distilling "bituminous schists, shales, or slates, or other rocks or minerals containing bitumen or bituminous substances."

‡ Dated June 23, 1845. Specification No. 10,726 of the English Patent Office.

§ Antisell, p. 85; compare Rees Reece's patent dated Jan. 23, 1849. Specification 12,436 of the English Patent Office.

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George Michiels.* It is peculiarly interesting since a portion of it relates to the preparation of oils from caking coals. Michiels proposes in fact to prepare coke from bituminous coals, and from mixtures of such coals with anthracite, by moistening the powdered coal with water and introducing it—in charges of six tons—into brick retorts furnished with ordinary condensing apparatus and other appliances. The retorts are then heated as if it were intended to produce gas, with this difference, that the temperature for the first fifty hours should not exceed nascent red heat, or 964° F.; after that time it should be increased progressively until it attains a clear red heat, which would be about the ninety-sixth hour, I should remark, continues Michiels, that about the sixtieth hour I shut off the communication between the retort and the condensor by closing the hydraulic valves, and at the same time open the valve on top of the retort, &c., so as to allow the air to enter, which burns the hydrocarburets [now being evolved] and the products of that combustion heat the retort, &c. in passing through the flues which surround the retort. * * * I thus obtain coke, ammoniacal liquors and liquid hydro-carburets. These "hydro-carburets" were repeatedly distilled by M. in order to obtain as much light volatile oil as possible, A heavy yellow oil of density 0·911, or lower, was also obtained which according to M. will be found applicable to many useful purposes, and is suitable for his principle object of turning into gas.

Further on (p. 15 of his specification) Michiels explains that this oil is well adapted for manufacturing gas upon a small scale, since the gas prepared from it requires no purification, and since it can be used in any of the ordinary apparatus for making gas from camphene, oil, or resin. In a word, he proposed using it just as rosin oil is now so largely employed by private gas-works in this country, or as Boscary and Butler had used the same coal-oil before him.

We pass† to a consideration of the well known labors of Mr. James Young of Glasgow.‡ From evidence brought forward in the trial already cited it appears that Mr. Young's attention was called in 1847 to a mineral oil [petroleum] found exuding from a coal pit at Riddings in Derbyshire. From it he obtained a good lubricating oil which he continued to prepare as long as his supply of petroleum lasted. Occupied as he was with the subject it can surprise no one that he should soon have turned his attention to the distillation of the highly bituminous mineral of Torbane-hill, now known as Boghead coal in England and in this country, which was introduced to public notice in 1850.§ From this substance Young was enabled to prepare a much larger amount of oil per ton of mineral than had been obtained by any of his predecessors. To the discovery of the vast source of an admirable raw material which the Boghead mine furnished is evidently due the immense increase in the production, and of course consumption, of coal-oil which immediately ensued. To this we say, more than to anything else is to be attributed the rise and progress, during the past few years, of the almost innumerable manufactories of coal-oil on the continent of Europe and in our own country. From the impetus thus given, a branch of industry which had long been, comparatively speaking, of only local importance soon attained an enormous development.‖

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* Granted April 30, 1850. Specification No. 13,066 of the English Patent Office.

† Making no pretence, be it understood, that we have been able to collect all that has been published upon the subject before 1850.

‡ Patent dated Oct. 7, 1850.

§ According to Mr. T. G. Barlow, *London Journal of Gas Lighting*, iii, 519.

‖ We cannot, in this connection, forbear quoting the following pertinent remarks from Lord Campbell's charge to the jury in the case—Young *v.* White and others (see *London Journal of Gas Lighting*, iii, 521)—already cited.

"And this brings me to an observation," says his Lordship, "which I meant to make, and which I should have been sorry if I had forgotton, which is this—that it was the discovery of this Boghead coal that seems to have given the great value

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Let it be distinctly understood that we would in no wise detract from the real merit of Mr. Young. Uniting, as he does, no small share of chemical knowledge with the cautious, untiring energy of his countrymen, few men could have been found better qualified to grasp the golden opportunity of which he so fortunately availed himself. His name must ever remain associated with those of the distinguished observers from whose labors this most important branch of industry has resulted. It is Dr. Antisell only whom we blame for his incorrect and partial "history." When, for example (on p. 14), Dr. A. tells us that: "only since the year 1850 has the manufacture of paraffine from pit-coal, turf and bituminous shales succeeded as an art. The first manufacture was that of James Young in Manchester, by whose process, from 100 parts of Cannel-coal 40 per cent of oil and 10 per cent of paraffine could be obtained." He makes a statement which is grossly exaggerated—if not *entirely* at variance with fact—as our readers must already have perceived.

We willingly quote what follows: "In thus showing [*i. e.*, dogmatically *asserting*] that the practical manufacture of oils from coal is due to James Young, it may not be amiss to call attention to what it was which he produced from coals by distillation. He claimed the production of paraffine oils—not the production of naphtha or benzule [benzol?], nor naphthalin, but paraffine and its congeners: this involves the slower distillation of coals at a lower temperature than had been hitherto effected, and this novelty in practice was followed by the novel result of a copious production of isomeric liquid hydrocarbons; so that really two great results were first demonstrated, practically by the operation of Young's process, namely—1st, That coal was a material from which liquids could be manufactured economically, as tar, bitumens, and schists had been hitherto employed; and 2nd, That the liquids so formed were paraffine-containing compounds." Having merely to suggest that the sentence might have been more tersely put. For in truth it means only—if it means anything—that in the opinion of Dr. A., Mr. Young was the first person who distilled coal [on a manufacturing scale?] at comparatively low temperatures. What Dr. Antisell's private views regarding "low temperature" or "practical" may be, we are ignorant. But we do know that when, 30 years ago, Reichenbach distilled quantities of coal of 75 lbs. weight each, and exercised the greatest care in maintaining the temperature of his retort at as low a degree as was admissible, as he has most minutely described in the memoirs which we have already cited;—when he obtained paraffine and eupion as results of his operation; he most certainly demonstrated the practicability and the manner of preparing both paraffine and "paraffine-oil."

All this however does not appear to satisfy Dr. A. in the least degree, who repeatedly assures his readers that the manufacture of oil from *coal* dates from the patent of Mr. Young. Since our author has seen fit to dwell at length upon this point and to devote so much space to its discussion we may be pardoned for referring to it here.

As is well known the term "coal" is applied in common language to a great variety of mineral combustibles no two kinds of which are precisely alike while some sorts are exceedingly unlike others. The term is at best merely conventional; used, in lack of any better one, to designate substances with the real nature of which we are still almost entirely ignorant.

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to paraffine, because until then I do not find it was obtained in such quantities as really were of any considerable value; but the Boghead coal now being discovered, and this schist or coal being discovered, which is of very rich quality, and having a great deal of what is the essential part of the paraffine; from that time it has become much more important; and that may explain why, although the mode of obtaining paraffine was before well known, it should not have been put in practice because it would not appear that it could be put into practice with much profit or benefit, unless you had such a substance as Boghead coal on which you could operate."

In confirmation of this view compare also: Payen, *Précis de Chimie Industrielle*, 4e Ed.. Paris, Hâtchette. 1859. t. ii, p. 685.

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With the flint-like anthracite of Wales, the beautiful Albert coal*—but a step removed from asphaltum—of Hillsboro, New Brunswick; our common lignites, frequently shading into peat, and the bituminous shales as frequently passing by insensible gradations into common slate, as points upon its extreme limit, we have within the circumference of the circle an infinite number of substances, shading into each other by scarcely perceptible degrees,—all of which are, in technological language at least, varieties of *coal*. The "amplitude of variation" which this species, or rather this term, enjoys is indeed so great that it would be a matter of no small difficulty to choose any single member of the medley as a central point, or even to conceive of an ideal coal to which all other varieties should be referred. In attempting any such selection a native of one of our sea-board states would assuredly lean towards anthracite; the South-German towards his excellent lignites; the Scotchman towards his cannel; while Newcastle would claim a proverbial right of precedent. We would, for our own part, vote for the last named, or some other good caking coal, capable of furnishing both gas and serviceable coke, and of being used for an infinite variety of purposes. Starting from this then as a type, observe, that as we pass towards the cannels, the different varieties of coal become better and better suited for the manufacture of gas or oil, *i. e.*, they contain more and more hydrogenous compounds. The appearance of the mineral meanwhile approaching more and more closely to that of slate, while at the same time the value of the fixed carbonaceous residue becomes less and less, soon ceasing to be "coke" at all, but rather a more or less carbonaceous slate. At length a maximum of hydrogenous matter is reached as in the case of Boghead coal, a slaty substance, the fixed residue from the distillation of which is a slightly carbonaceous stone, valueless as fuel and useful only, as a substitute for bone-black, for purposes of disinfection or decolorizing liquids, uses to which the residues of the French shales have long been applied. Beyond this maximum, as the amount of gas and oil-producing substances diminishes, and the amount of earthy matter increases,—taking the place of the fixed carbon in our typical caking coal, we pass into "bituminous shales," and these become less and less bituminous until at length we reach common clay slate containing no organic matter whatsoever. We have here traced no fancy sketch. That the "cannel coals" thus gradually pass into "bituminous shales" is now well enough known, at least to gas engineers and other practical observers. It would not for that matter be exceedingly difficult to obtain a continuous series of specimens exhibiting this almost insensible gradation. Now did Mr. Young devote his attention to the distillation of caking coals similar to our typical Newcastle? By no means! On the contrary we find him occupied with a mineral which was called indifferently "shale" or "coal," until it was in 1853 decided in a Scottish court that it should henceforth be legally known as coal.†

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* On page 18 of Dr. A.'s work the following remark occurs. "In one respect they [bitumens] differ from coal. In no case can an organic tissue or structure be demonstrated when they are examined under the microscope. Viewed in this light the mineral found at the Albert mine, New Brunswick, should be classed as a bitumen since Dr. J. Leidy was unable to detect any trace of structure in its mass." We cannot understand the motive of our author in thus again dragging to light this negative result, for it is perfectly well known to the scientific men of America that Prof. J. Bacon detected the existence of vegetable structure in the interior of masses of the Hillsboro coal. See *Reports on the Geological Relations, chemical analyses, and microscopic examination of the coal of the Albert Coal Mining Co., situated in Hillsboro, Albert Co., N. B.*, by Chas. T. Jackson. M.D. New York: printed by Nesbitt, 1851, p. 30; compare this Journal, [2], xiii, 276.

† We had supposed, when the above was written, that the decision of this court had been sustained. That in this we were mistaken appears from the following, which we extract from a statement in a late number of the *London Journal of Gas*

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We would cast no reflection upon the judgment rendered in this famous suit. Looking at it as a mere matter of equity, depending upon the business relations of the parties at issue, this verdict was in our own opinion, just. But the fact of this mineral being, or not being, called a *coal*, does not in the least degree prevent it from being *also a shale*; and that it is more nearly related to the shales than to the coals is believed by a large proportion of those who are intimately acquainted with it, be they scientific or practical men. We have no space to discuss at greater length this quibble of Dr. Antisell's—which, however excusable it might have been in a retained attorney or solicitor of patents, is anything but becoming to the chemical professor or the historian—being content to refer the reader to the published reports of the trial just mentioned. We will here cite only a few lines* descriptive of Selligue's mineral: "The quantity of oily matter in these shales is very variable and often very

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Lighting (Jan. 17th, 1860. vol. ix, p. 41), received as this article is going through the press.

"SETTLEMENT OF THE GREAT TORBANE-HILL CASE.

We have been favored with the following particulars connected with the well known case, the 'Bathgate or Boghead Gas Coal *alias* the Torbane-hill Mineral,' which has lasted upwards of seven years, having passed through several phases in the Supreme Law Courts of Scotland and England. A compromise was finally come to on Wednesday last, the eleventh current. It is embodied in a minute of agreement between Mr. and Mrs. Gillepsie of Torbane-hill, of the first part, and Messrs. James Russel and Son, and James Russel, Esq., of Blackbraes, of the second part.

The preamble of the minute of agreement, which itself consists of twelve articles, is as follows:

'The said parties, considering that disputes and differences have arisen between them and a lengthened litigation has taken place, with respect to the missives of agreement for a lease of certain minerals in the lands of Torbane-hill, entered into betwixt the said first party hereto on the one part, and the said company of James Russel and Son, and individual partners thereof, on the other part, and dated the 30th of March and 1st of April, 1850; and both parties being now desirous that the said litigation should be brought to an end, and all disputes and differences between them amicably adjusted and settled, they have agreed, and hereby mutually agree and bind themselves as follows:'—

The first two articles provide that the actions at present depending shall be abandoned, as a consequence of the execution of the minute.

The third article provides that each party pay their own expenses.

The fourth article, which has for title 'Name of Mineral,' is both an important and curious one—important in a scientific point of view, and curious as illustrative of the pertinacity with which either party have clung to their own views. The article is as follows:—'Whereas the second party have been, and are, working in the said lands of Torbane-hill, and disposing of, under the name of Bathgate or Boghead gas, parrot, or cannel coal, a mineral which they, the second party, deemed and deem to be a parrot or cannel coal, and which the first party deemed and deem to be a new mineral substance, having an argillaceous base, and to be of so peculiar a nature as to constitute it in truth a new and very peculiar variety of bituminous schist, slate, or clay, and have been for some time in use to call 'the Torbane-hill Mineral'; it is hereby agreed that, throughout the remainder of these presents, where the mineral in question is named, it shall be called for the sake of brevity the disputed mineral.'

Article fifth relates to the subject of a portion of Torbane-hill which was reserved from the operation of the mineral lease, and by this article 'the disputed mineral,' as we now call it, contained in the reserved portion of the estate, may be worked or let by the proprietors, without the danger of any obstruction being offered by the second party in the agreement." * * *

* From Dufrénoy et Elie de Beaumont's Explication de la Carte Géologique de la France. Paris, Imp. Royale, 1841. t. i, p. 673.

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considerable. According to M. Xardel some rare samples exist which afford even 45 @ 50 per cent; * * * other specimens afford 20 @ 25 per cent. The beds which are worked, or are capable of being worked, yield from 5 to 9 per cent." [p. 675.]—Again [p. 676], "The impressions of fossils, so common in the shales of d'Igornay, occur in the poorer shales. The rich shales, on the contrary, often contain vegetable remains analagous to those commonly found in the coal measures. Perhaps the beds of rich shale are in a manner the representatives of coal-beds; it is to be remarked that in the shale which yields 9 per cent of oil its sheets are covered with a multitude of shining (*miroirtantes*), lenticular veins, having a waxy fracture, which by their aspect and manner of burning recall the variety of coal which is called *cannel coal*." —Leaving it for our readers to answer the question; how far removed in anything but productiveness is the "coal" (Boghead) upon which Mr. Young has operated from the "*shale*" distilled by M. Selligue?

It may not be amiss to mention the fact that upon the continent of Europe the Boghead mineral is almost universally called, not coal, but shale. A fact with which the reader can readily enough familiarize himself by consulting the German chemical journals of the last eight or ten years. In proof of it we cite only the following:* "The recent verdict in the celebrated Torbane-hill-mineral case appears to be contrary to the scientific opinions held in Germany, as proof of which we have a case in point, and which, although not at the time known in this country, was officially decided upon in Berlin previous to the trial coming on in Scotland, which terminated on the 4th of August last. It appears that in Frankfort-on-the-Main there has, for some time past, been in existence a company for lighting the streets and houses by gas from oil, resin, &c. A rival English company contracted to light with coal-gas; and to give both fair play, it was decided that the latter company should be confined to the use of coal alone. Mr. Engelhard, the manager of the Oil (Resin) Gas Company, having heard of the Boghead and Torbane mineral, obtained specimens, and having found they produced excellent gas, gave an order for a large consignment which reached Frankfort *vid* Rotterdam, through a Dutch agent. This was entered at the Custom-house as cannel coal, much to the annoyance of Mr. Engelhard, who was no more at liberty to make gas from coal than his rivals were to make it of anything else but coal. He was, however, prevented from the necessity of a trial at law, for the officials did not feel themselves justified in charging the duty as coal, although, as other mineral, it would pass free, and applied to higher authorities for instruction. These parties were as much at a loss as their inferiors, and the case was eventually transferred to the Central Board of Customs at Berlin, the last court of appeal of the Prussian *Zollverein*, where all disputed questions in the German States are settled. Scientific men, connected with the board, examined the Boghead and Torbane mineral and decided that it was *not* coal, but *bituminous shale*, which is said to be the general opinion among German chemists. It has been admitted into Germany, duty free, and Frankfort is now partially lighted with gas from this mineral, charged for as resin or oil gas. It is described as a clay containing bitumen, and producing oil when heated. At all events, we may take this German decision as impartial as, had it been admitted as cannel coal it would have been subject to a duty varying from 1*s.* to 1*s.* 6*d.* per ton."

In connection with the question of the products of the distillation of *coal* which Dr. A. would have us believe so entirely new to the world and to this country in particular, we cite the following from The *Encyclopædia of Chemistry*, by James C. Booth and Campbell Morfit. 8vo, Philadelphia, Baird, 1850, p. 461. Article, Coal: paragraph, "products of dry distillation."

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* See *London Journal of Gas Lighting*, Oct., 1853, iii, 256; from *London Mining Journal*.

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"These products [of the dry distillation of coal] are somewhat analogous to those derived from wood, and some are identical with them. * * * The liquid products consist of various bodies closely allied to petroleum, and the solids *Naphthaline* and *Paraffine*. The relative proportions of these products vary with the temperature. The lower the heat employed, the less gas, and the more solids and liquids are produced; the higher the temperature the greater is the quantity of carburetted hydrogen." Before closing this sketch we must refer to and correct a palpable error of Reichenbach's* which has been cited by Dr. Antisell (p. 14), from whom we quote it: "So remained paraffine until this hour [date of Y.'s patent], a beautiful item in the collection of chemical preparations; but it has never escaped from the rooms of the scientific man."

Upon the reader who has followed us thus far we need not urge that the above statement is incorrect. As an offset to it we cite the following:†

"In the Parisian Industrial Exhibition for 1839 Selligue exhibited: 1st—Bituminous shale, then fluid bitumen [crude oil], mineral grease, crude and refined (the former at 50 fr. the 100 kilog. [= $5.00 per 100 lbs.]), also *mineral wax* [paraffine] crude and refined (the former at 125 fr., the latter at 180 fr. [= respectively 12½ and 18 dollars per 100 lbs]). The purified mineral wax was beautifully white but the candles made of it had a soiled appearance." V. Hermann (now, according to Wagner, councillor of State in Munich goes) on to assert that "if these fatty products can be prepared economically they belong to the most important objects of the Exhibition."

It would be foreign to our purpose were we to attempt to trace the recent history of the art of manufacturing coal-oil, even if our space allowed of it. During the past few years a large number of papers‡ on the subject have been published in the Scientific Journals of Germany; while several recent works upon the materials used for producing light have each devoted a separate chapter to its description. A few special treatises have also been published of which the following is a, doubtless very incomplete, list.—§

UHLENHUTH, Ed. *Handbuch der Photogen- und Paraffin-Fabrikation aus Torf, Braunkohle und bituminösem Schiefer nach den neuesten Versuchen und Erfahrungen. Quedlinburg, Basse*, 1858.

MUELLER, Carl, Georg, *Die trockene Destillation und die hauptsächlichsten auf ihr beruhenden Industriezweige.* Leipzig, Barth. 1858.

DANCKWORT, MEITZENDORFF *und* WERNECKE. [Committee of the Magdeburg *Gewerbeverein.*] *Ueber das Photogen oder Mineralœl, so wie die ihm*

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* Erdmann's Journal für praktische Chemie, lxiii. 63. Did our space allow, we would gladly transcribe the whole of this article—an English translation of which may be found in the London, Edinburgh and Dublin Philosophical Magazine, [4.] viii, 463—in proof of our assertion that the present widely-spread manufacture of coal-oil and paraffine is mainly due to the comparatively recent discovery of rich stores of highly bituminous substances.

† From v. Hermann's *Die Industrie Ausstellung zu Paris im Jahre*, 1839, *Nürnberg*, 1840. p. 147;—in Wagner's *Jahresbericht ueber die Fortschritte der chemischen Technologie*, 1855, i. 416.

‡ Very complete synopses of these may be found in Wagner's *Jahresbericht*, four volumes of which have thus far been published. For references to the recent admirable *scientific* researches of GREVILLE WILLIAMS, DE LA RUE, and others, which have been chiefly confined however to the more volatile portions of the oil and to the basic compounds which occur in it, see Liebig and Kopp's *Jahresbericht der Chemie, u. s. w.*

§ Small as this list is, it will nevertheless recall to the mind of the reader the modest lines with which Dr. Antisell's preface commences, namely these: "the present little treatise is the first published monograph on the art of distilling oils from minerals containing Bitumen."

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ähnlichen Leuchtstoffe, in Bezug auf ihre Feuergefährlichkeit und ihre Anwendung. Magdeburg, 1856.

Also the insignificant brochure of SCHRADER, F. W. *Ueber die industrielle und national-ökonomische Bedeutsamkeit der Gewinnung von Chemikalien insbesondere des Paraffin's und Photogen's aus dem Kohlentheer, u. s. w.* Aschersleben, Beyer, 1856.

This article must here close. Leaving unnoticed several inaccuracies which we had intended to discuss we will dismiss the subject with two brief quotations. The first from Dr. Antisell's book, p. 15. "An impression has taken hold of the American manufacturing public that the patent of James Young has no force, as it was not a new invention at the date of the patent; and from the unfavorable effect of that patent upon the actual manufacture of coal-oils in this country, an ill-feeling has been produced against it. That the owners of this patent have not acted wisely by witholding sales and licenses under it until very lately, is to be regretted; but that it was a *bona fide* improvement in an art at the time when it was patented, and that therefore the patent was rightly issued in this country, there can be no shadow of a doubt in the mind of any one who carefully traces the steps of the discovery of the production of photogenic oils from different materials."

The second from Lord Chief Justice Campbell's charge* to the jury in the case already alluded to. "Now gentlemen I direct you, in point of law, that if there were books then [at date of Young's patent, 1850] in circulation in England disclosing this mode of obtaining paraffine and paraffine oil which were known, were accessible, that the patent would be invalid, although Mr. Young never read those books, and although that mode had not been actually put in practice. If there were books in England in circulation, accessible to all who were interested in the subject, which disclosed this, and would instruct them and enable them to obtain the paraffine and the paraffine oil from the distillation of bituminous substances, then Mr. Young's patent would be invalid."

FRANK H. STORER.

SCIENTIFIC INTELLIGENCE.

I. CHEMISTRY AND PHYSICS.

1. *On a probable means of rendering visible the Circulation in the Eye;* by OGDEN N. ROOD, Prof. of Chemistry in Troy University.—Some time ago while looking at a bright sky through three plates of cobalt-glass, I saw with astonishment that the field of view was filled with, and traversed in all directions by small bodies resembling animalcules.

They were seen on the blue field as yellowish spots, and always appeared elongated in the direction of their motion, which was as a general thing tolerably uniform. The same result was obtained by experimenting upon the eyes of a number of persons.

Convex lenses of various foci, (from 3 in. to ½ in.), were now held before the eyes, so as to give the blue light various degrees of convergence and divergence, without in the least altering the appearance of the moving bodies; this seemed to indicate that their locality was in the retina or in its immediate neighborhood.

A position near the axis of vision was selected, and observed, when it was found that these bodies in traversing this spot always pursued the same direction and path, disappearing at the same point; other positions near the axis gave like results.

* *Loc. cit.*, p. 520.

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This would seem to preclude the possibility of the moving bodies being animalcules swimming in the humor of the eye; the most probable remaining supposition is, that they are blood-corpuscles circulating in the retina or in its immediate neighborhood. The apparent diameter of these bodies when seen projected on a window six feet distant may be about $\frac{1}{20}$ of an inch, which corresponds to about $\frac{1}{1600}$ of an inch on the retina. The average diameter of the blood globules is $\frac{1}{3200}$ of an inch, but taking into account the fact that the shadows of the moving bodies are not well defined the correspondence may be considered pretty satisfactory.

The question now arises as to the manner in which the blue glass renders the circulation visible, for these moving shadows cannot be seen with distinctness through red, orange, yellow, green, nor even purple media; they are on the other hand well shown by a certain thickness of a solution of the cupro-sulphate of ammonia.* Yellow solutions when combined with the blue glass or blue solutions render the circulation invisible, and it does not reappear till the yellow solution has been made so dilute as barely to preserve a yellow tint, and to transmit the spectrum almost unaltered. This shows that the indigo and violet rays are principally concerned in the production of this appearance, but that it cannot be attributed to fluorescent properties in the blood discs is indicated by the fact that the circulation can be seen through a considerable thickness of crown glass, through an infusion of red sanders wood mixed with ammonia, as well as through a solution of the disulphate of quinine.

The only explanation that has occurred to me as being probable is the following: the blood discs are yellow and consequently opaque to a great extent to the indigo and violet rays; they would therefore in passing before the retina cast shadows on it; now the retina being already strongly impressed with blue light, that portion of it which was momentarily protected from the action of this light, would experience the complimentary sensation—or would see instead of a moving shadow a yellowish moving streak. This explains also why the appearance is not seen with any distinctness in red, orange, yellow, or green light, for yellow media are to a great extent transparent to all their rays and therefore fail to cast shadows. These observations if new may be of some interest to those engaged in the study of the physiology of the eye.

Troy, May 14th, 1860.

TECHNICAL CHEMISTRY.

1. *Care of Platinum Crucibles.*—In connection with some sensible remarks upon the use of sand in cleaning platinum crucibles,—a practice which, with Berzelius, (Lehrbuch der Chemie, 1841, 4th Aufl. p. 516,) he heartily commends—urging that it should be employed every time that a crucible is used, ERDMANN explains the cause of the gray coating which forms upon platinum crucibles whenever they are ignited in the flame of Bunsen's gas-burner.

This coating has given rise to much annoyance and solicitude among chemists. Indeed it has often been asserted, that the use of Bunsen's

* Mr. Wm. B. Taylor of Washington to whom some of these facts were communicated by Prof. J. Henry was able to trace this appearance, though with *diminishing distinctness*, through plates of bluish-green and yellow glass.

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burner is unadvisable in quantitative analysis, since by means of it the weight of platinum crucibles is altered and the crucibles themselves injured. The coating is produced most rapidly when the crucible is placed in the inner cone of the flame, and the more readily in proportion as the pressure under which the gas is burned, is higher. Having found it advantageous to maintain, by means of a special small gas holder, a pressure of four or five inches upon the gas used in his own laboratory, Erdmann has observed that the strong gas flame thus afforded, immediately occasions the formation of a dull ring upon the polished metal placed in the inner flame, this ring being especially conspicuous when the crucible becomes red hot; it increases continually so that after long continued ignition the whole of the bottom of the crucible will be found to be gray, and with its lustre dimmed.

This ring is caused neither by sulphur, as some have believed, nor by a coating of inorganic matter, but is simply a superficial loosening of the texture of the platinum in consequence of the strong heat; whence it first of all appears in the hottest part of the flame.

In conjunction with Pettenkofer, Erdmann instituted several experiments which have left but little doubt that the phenomenon depends upon a molecular alteration of the surface of the metal. If a weighed polished crucible be ignited for a long time over Bunsen's lamp, the position of the crucible being changed from time to time in order that the greatest possible portion of its surface shall be covered with the gray coating, and its weight be then determined anew, it will be found that this has not increased. The coating cannot be removed either by melting with bisulphate of potash or with carbonate of soda. It disappears, however, when the metal is polished with sand; the loss of weight which the crucible undergoes, being exceedingly insignificant, a crucible weighing 25 grams, having lost hardly half a milligram. When the gray coating of the crucible is examined under the microscope it may be clearly seen that the metal has acquired a rough, almost warty, surface, which disappears, when it is polished with sand. Platinum wires which are frequently ignited in the gas flame, for example the triangles which are used to support crucibles, become as is known, gray and brittle. Under the microscope they exhibit a multitude of fine longitudinal cracks which as the original superficial alteration penetrates deeper become more open, or as it were spongy—until finally the wire breaks.

If such wire is strongly and perseveringly rubbed with sand, the cracks disappear, and the wire becomes smooth and polished, for the grains of sand acting like burnishers restore the original tenacity of the metal; very little of its substance being rubbed off meanwhile. The loosening effect of a strong heat upon metals is beautifully exhibited when silver is ignited in the gas flame; a thick polished sheet of silver immediately becoming dull white when thus heated. Under the microscope, the metal appears swollen and warty. Where it has been exposed to the action of the inner flame along its circumference, this warty condition is visible to the naked eye. A stroke with the burnishing stone however, presses down the loosened particles, and reproduces the original polish. This peculiar condition which the surface of silver assumes when it is ignited, is well-known to silversmiths, it cannot be replaced by any etching with

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acids; and it must be remembered that what is dull white in silver, appears gray in platinum.

If each commencement of this loosening is again destroyed, the crucibles will be preserved unaltered, otherwise they must gradually become brittle. Crucibles of the alloy of platinum and iridium are altered like those of platinum when they are ignited; it is, however, somewhat more difficult to reproduce the original polish of the metal, by means of sand, as might be expected from the greater hardness of the alloy.

The sand used should be well worn; when examined under the microscope, no grain of it should exhibit sharp edges or corners, all the angles should be obtuse.—*Journ. für praktische Chemie*, March, 1860, lxxix, 117.

2. *Safe and easy method of rectifying Sulphuric Acid;* by N. NEESE of Kiev.—"The rectification of sulphuric acid offers an example of how readily even the most practical matters may fall into oblivion in spite of the copiousness and the vigilance of our literature. Fifteen years ago, Prof. Siller, at that time filling the chair of Pharmacy at Dorpat, called my attention to an exceedingly ingenious device for rectifying sulphuric acid, which he had read in some journal. The retort in which the rectification of the acid is to be conducted, should be placed upon a bed of ashes about an inch in thickness, and sand then heaped up all around it; the distillation may then be proceeded with, without any further precautions. By this method I have twice rectified sulphuric acid, operating with a very bad retort, upon portions of 15 lbs weight; and have been astonished at the facility and tranquillity with which the distillation proceeded. The ashes evidently act as a non-conductor, so that the acid can boil only from the sides of the retort. Since then, manifold expedients have been proposed, all depending upon some peculiar apparatus, constructed for this particular purpose, but I have not yet met with any reference to the simple contrivance which has here been mentioned."*—From *Archiv der Pharmacie*, cxlv, 267; in *polytechnisches Notizblatt*, 1860, xv, 43.

[FREDERKING of Riga, (*Archiv d. Pharm.*, Aug., 1859; in *American Journal of Pharmacy*, viii, 88,) corroborating the statement of Neese, states that he can recommend the process from his own experience,

* The author is evidently not aware that several devices almost as simple as the one which he describes have previously been proposed. Thus Otto, (in his *Lehrbuch der Inorganischen Chemie*, Braunschweig, 1855; i, 275,) directs that the retort be caused to rest upon an inverted crucible cover, which is placed upon a grate in an extemporaneous fireplace constructed of loose bricks, (figured in Otto,) so that the bottom of the retort may be less strongly heated than the upper portions by the charcoal fire which is built around it. Otto, however, considers it necessary to cover the retort with a thin coating of clay which has been mixed with a dilute solution of carbonate of soda or of borax, instead of water, so that it may adhere firmly to the retort when heated; urging that the danger of fracturing the latter is not only lessened thereby, but that a more rapid distillation is insured, since little or no condensation can occur within a retort thus protected. In Mohr, Redwood and Procter's *Practical Pharmacy*, Phila., 1849, p. 337, a somewhat similar proposal is made. A Hessian crucible being employed, however, instead of the cover used by Otto:—moreover, the retort is not coated.

Possibly a more convenient method than either of the above would be to fill a sufficiently capacious crucible with ashes, or some other non-conducting substance—calcined gypsum, for instance,—as a bed upon which to rest the bottom of the retort. A charcoal fire similar to Otto's being then built around it. F. H. S.

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inasmuch as he has during the last twelve years, repeatedly rectified sulphuric acid in the manner indicated.—F. H. S.]

3. *Vulcanization of Caoutchouc, by means of mixed Sulphur and Hypochlorite of Lime.*—Gaultier de Claubry, having detected the presence of chlorid of calcium in many samples of vulcanized rubber, and suspecting that this salt might have been derived from hypochlorite of lime employed in some modification of Parkes' process of cold sulphuring, [English Patent of March 6, 1846. Repertory of Patent Inventions, (E. S.,) ix, 46,] to produce chlorid of sulphur in the rubber paste, was led to perform the following experiments:

When flowers of sulphur and dry hypochlorite of lime, (bleaching powder,) are shaken together, a very strong odor of chlorid of sulphur is immediately developed. If the mixture be somewhat forcibly rubbed in a mortar, elevation of temperature ensues, the sulphur softens and the mixture becomes solid while abundant vapors are evolved. When a much larger amount of sulphur than of the hypochlorite is used and friction is avoided when the two are blended, a mixture is obtained which being added to the caoutchouc paste—either with or without the addition of inert matters, such as chalk, oxyd of zinc, etc., serving to give "body" to the product—effects the vulcanization of the latter either at the ordinary temperature, or when gently heated. By this means, objects of any thickness can be uniformly vulcanized.

If instead of employing an excess of sulphur, an excess of the hypochlorite be introduced into the mixture, and this be agitated, so much heat will be developed that the vessel containing the mixture can no longer be held in the hands; if the flask be closed, the action becomes so violent that the cork will be blown out, or the flask broken by a violent explosion.—*Comptes Rendus*, May, 1860, l, 876.

4. *Preparation of Cyanid of Barium, and of Ammonia with the Nitrogen of the Air;* by Margueritte and De Sourdeval.—In a brief preliminary note, the authors claim:—

That baryta when calcined in the presence of charcoal and of atmospheric air combines very readily with carbon and nitrogen, cyanid of barium being formed.

That the cyanid of barium heated in a current of aqueous vapor is decomposed at a temperature of about 300° (C.) [572° F.] and disengages in the form of ammonia, all the nitrogen which it contains.

Trials which they have made upon a tolerably large scale, have been eminently successful, leading them to hope that not only the various cyanids employed in the arts, but also ammonia and nitric acid may thus be economically produced.*—*Comptes Rendus*, June, 1860, l, 1100.

5. *Gun-Cotton Filters;* by Prof. Boettger.—Since gun-cotton—itself a product of the action of strong acids—when properly prepared is scarcely at all acted upon at the ordinary temperature, by chemicals, being capa-

* To prepare baryta from its carbonate, M. and De S. ignite a mixture of the latter with the pitch ("asphaltum") of coal tar. Each molecule of the carbonate being thus brought in contact with the reducing agent, carbon, excellent results are obtained, the decomposition of the carbonate being easy and the product of baryta abundant. From observing the odor of ammonia which was at times developed during their experiments upon this method of preparing baryta the authors were led to the important discoveries which are noticed in the text.—*Répertoire de Chimie appliq.* June, 1860, ii, pp. 169, 170.

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ble of withstanding the most corrosive agents; it affords a material for filtering strong acids, and the like, as well as liquids which would be decomposed by contact with the organic matter of ordinary filters, the excellence of which cannot be too highly extolled.

Besides employing it for removing from strong nitric acid the chlorid of silver which is precipitated in the common method of purifying this acid by means of nitrate of silver, (as has recently been advised in the Berlin *polytechnisches Intelligenz-Blatt*, No. 4, p. 30); Boettger affirms that, he has for several years past, found it of special use in filtering off the slimy precipitate, containing selenium, which is gradually deposited, when fuming sulphuric acid is mixed with a little water; in separating crystallized chromic acid from the sulphuric acid of the mother liquor; and in filtering concentrated solutions of permanganate of potash, in order to separate suspended peroxyd of manganese. He has even found the gun-cotton well suited for filtering concentrated alkaline lyes, aqua-regia, and concentrated solutions of chlorid of zinc, not to allude to many other instances. In using the cotton a small bit of it is pushed loosely, like a stopper, into the throat of a funnel.

The materials which have heretofore been used for similar purposes, viz.: garnets, asbestos, powdered glass, &c., are very much inferior, as filters, to the loose, fibrous gun-cotton.—From *polytechnisches Notizblatt*, 1860, No. 7; in Dingler's *polyt. Journal*, clv, 463.

6. *Preservation of Flesh;* by VERDEIL.—Having been separated from the bones, and, as far as possible, from fat, the flesh is cut into slices from one to five centimetres, (one centimetre =0·3937 inch,) in thickness; the slices being cut as nearly as possible across the grain of the flesh. These are now laid upon hurdles of basket-work, which are subsequently placed in a chamber. As soon as a sufficient number of the trays have been introduced into the chamber, it is closed, and steam under a pressure of three or four atmospheres, consequently of 135° to 145° C. [=275° to 293° F.] is admitted through several openings.

The chamber, which may be of lead or iron, must not be absolutely tight, a small outlet for the steam being necessary, in order that the pressure may not become too great.

After from six to ten or fifteen minutes, according to the kind of flesh and the thickness of the slices, the steam is shut off, this part of the process being finished.

The flesh is now very nearly in the condition of boiled meat, but has retained all of its ingredients, the albumen having been coagulated: its taste recalling that of roasted meat. It presents a wrinkled appearance, is of a gray color, and may be readily divided.

Being removed from the steam chamber the flesh is now placed upon trays, or hung upon hooks, in another chamber which is warmed, but in which the temperature is never allowed to exceed 40° or 50° C. [=104° to 122 F.]. The drying process is completed in the course of eight or twelve hours.

Packed in tight casks or in tin boxes, so that it may be protected from the action of moisture, and from insects, the flesh thus prepared may be preserved for any length of time which may be desirable. It is nevertheless well to place a layer of salt in the casks, in order that it shall absorb any moisture which the flesh may have retained. Before using

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this meat it must be soaked for an hour or two in warm water in which it softens and regains its original condition. When boiled with water it affords an excellent soup, and passes into a condition, in which it cannot be distinguished from fresh meat.—From *Le Génie Industriel;* in BOETTGER'S *polytechnisches Notizblatt*, 1860, xv, 71.

7. *Magnesium as a Source of Light.*—Prof. A. SCHMITT calls attention to the practicability of employing metallic magnesium for purposes of illumination, as had already been suggested by Bunsen. From the researches of the last named chemist, it is known that when magnesium is ignited it readily takes fire and burns with an exceedingly brilliant flame. The intensity of the light thus produced, as determined by Bunsen and Roscoe in one of their photo-chemical researches (*Pogg. Annalen*, cviii, 261, *et seq.*) is only some 525 times less than that of the sun. Compared with an ordinary candle, it appeared that a wire of magnesium 0·297 millimetre [1 mm. =0·0394 inch] in diameter, produced as much light in burning as 74 stearine candles, five to the pound. In order to support this light during one minute, a piece of wire 0·987 metres long, weighing 0·1204 gram [1 grm. =15·4325 grains], was required.

Only 72·2 grams of magnesium, therefore, would be needed, in order to maintain during ten hours an amount of light equal to that of 74 stearine candles, consuming about 10·000 grams of stearine.

According to Bunsen, magnesium wire is readily obtained, by forcibly pressing the metal through a hot steel die by means of a steel piston. Bunsen's arrangement for burning the wire was made, by connecting spools of it with rollers moved by clock-work so that the wire should be unrolled like the ribbon of paper in Morse's telegraph, the end of the wire thus gradually pushed forward, passed into the flame of an ordinary alcohol lamp, where it took fire.

It is evident that a magnesium lamp of this sort must be much simpler and more compendious than any of the existing arrangements of the electrical, or of Drummond's light; for light-houses, &c.: where an intensely brilliant illumination is required it can hardly fail to rival either of these. Where an extraordinary amount of light is needed, it could readily be produced by burning large wires, or several thin ones at the same time. Another important consideration is the fact that the spools of wire, as well as the clock-work and spirit lamp, are easily transportable.

It is not, however, to the intensity alone of the magnesium flame that these lamps owe their utility, for the photochemical, (*i. e.*, photographical) effect of the light is also very great. According to Bunsen, the photochemical power of the sun being only 36·6 times greater than that of the magnesium flame. The latter must therefore be useful in photographing by night or in any dark or subterranean locality; the evenness and remarkable tranquillity of the flame, especially commending it for this purpose.

The present high price of magnesium, it is true, must prevent any extended use of it for technical purposes. For example, Lenoir of Vienna charges 3 Florins, [1 Fl. =51 cts.] for a gram of it, hence the cost per minute of the light just described, would be 36 *Neukreutzer*, [1 ktr.= about $\frac{5}{8}$ of a ct.,] and the cost during ten hours, would amount to 216 Florins, while the ten kilogrammes of stearine could be procured for less than 14 Florins. But even at this price, it could still be used by photo-

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graphers, since it would only be required for exceedingly short intervals of time, and all unnecessary consumption of the wire might be prevented by stopping the clock-work.—From *Stamm's Illustr. Zeitschrift*, 1859, p. 332; in *polytechnisches Notizblatt*, 1860, xv, 56.

8. *Method of employing carbonic acid in connection with the hypochlorite of lime used for bleaching paper-stock.*—An apparatus devised by FIRMIN DIDOT and BARRUEL of Paris, for introducing carbonic acid, prepared by burning charcoal, into the solution of hypochlorite of lime, (bleaching salt,) while the latter is in contact with the fibre which is to be bleached, is described in the Nov., (1859,) No. of Barreswil's *Répertoire de Chimie*, Appliquée, vol. i, p. 457.

The carbonic acid on being introduced into the solution of bleaching salt, unites with the lime, thus setting free hypochlorous acid, the decolorizing action of which is infinitely more energetic when it is at liberty than when in combination with a base.

This process, says Barreswil, is of extreme simplicity, and one is at a loss to comprehend why it had not been sooner invented, in view of the fact that each and all of its phases have been so long and so well known.

In order to judge of the practicability of the new process—in so far as concerns difference of price, strength, and whiteness of the paper, and the duration of the operations in the two systems, (new and old,) of bleaching—comparative experiments were instituted, by the Messrs. Firmin Didot, upon carefully assorted rags. The cost of the chemicals and labor, and the amount of time required, having been exactly noted. After bleaching, the pulp was converted into paper. The different papers were then carefully tested. As the result of these experiments, it appeared that the new process was more energetic and more rapid than the old method, *au chlore*, [*chlorure?*] *liquide*, [with solution of bleaching salt,] and that in many cases it is also equally energetic with the process in which chlorine gas is employed. Over the latter it has the advantage of not destroying to so great an extent, the fibre of the pulp.

Since the details of the process, which for that matter consists merely of arrangements for thoroughly washing and cleansing the carbonic acid employed—the latter being then introduced into the bleaching vats, just as if it were steam, through coils of pipe pierced with holes, which are placed at the bottom of the vats,—cannot well be explained without a diagram, we must refer the reader who may desire these to the original article, in which the apparatus is figured. F. H. S.

9. *New "fusible metal."*—Dr. B. WOOD of Nashville, Tenn., has secured a patent (Weekly Scientific Artizan, Cincinnati, May 5th, 1860,) for an alloy composed of cadmium, tin, lead and bismuth, which fuses at a temperature between 150° and 160° F. The constituents of this *fusible* metal may be varied according to the other desired qualities of the alloy—viz: cadmium one to two parts; bismuth seven to eight parts; tin two parts; lead four parts. It is recommended as being especially adapted for all light castings requiring a more fusible material than Rose's or Newton's "fusible metal," it having the advantage of fusing at more than 40° F. lower temperature than these alloys, and owing to this property may replace many castings heretofore made only with amalgams. Its fusing point may be lowered to any extent by the addition of mercury, which

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may be employed within certain limits without materially impairing the tenacity of the metal. In a letter to the Editors, dated Nashville, June 9th, 1860, Dr. Wood says:—

"One point in particular that strikes me as being worthy of note is the remarkable degree in which Cadmium possesses the property of promoting fusibility in these combinations. The alloy of one to two parts *cadmium*, two parts lead, and four parts tin is considerably more fusible than an alloy of one or two parts *bismuth*, two parts lead and four parts tin; and when the lead and tin are in larger proportion the effect is still more marked. It takes *less* cadmium to reduce the melting point a certain number of degrees than it requires of bismuth, besides that the former does not impair the tenacity and malleability of the alloy, but increases its hardness and general strength.

Bismuth has always held a pre-eminent rank among metals as a fluidifying agent in alloys. Its remarkable property of 'promoting fusibility' is specially noted in all our works on chemistry. But I do not find it intimated in any that cadmium ever manifests a similar property. The fact indeed appears to have been wholly overlooked—owing perhaps to the circumstance that as an alloy with certain metals cadmium does not promote fusibility.

Cadmium promotes the fusibility of some metals, as copper, tin, lead, bismuth, while it does not promote the fusibility of others, as silver, antimony, mercury, &c., (i. e., does not lower the melting point beyond the mean.) Its alloy with lead and tin in any proportion and with silver and mercury, within a certain limit, say equal parts and especially of two parts silver and one of cadmium or two parts cadmium and one mercury are used, are tenaceous and malleable, while its alloys with some *malleable* metals, (gold, copper, platinum, &c.,) and probably with all brittle metals *are* 'brittle.'

I notice a great discrepancy among authors as to the melting point of this metal. It is usually put down the same as that of tin, (442° F.) Brande (*Dict. of Science and Arts*,) says it 'fuses and volatalizes at a temperature a little below that at which tin melts.' Daniell, (according to the *New American Cyclopedia*,) gives its melting point at 360° F., while Overman places it at 550° and gives 600° as the temperature at which it volatalizes.

The latter is doubtless the nearest the truth. The metal requires for its fusion a temperature too high for measurement by the mercurial thermometer, but from relative tests with other metals I should place its melting point in round numbers at 600° F. as it melts and congeals nearly synchronously with lead, the melting point of which is stated by different authorities as 594°, 600°, and 612° F. It volatalizes at a somewhat higher heat.

I draw attention to these facts believing that the metal possesses properties valuable to Art and interesting to Science, and that it merits more thorough investigation than appears to have been bestowed upon it."

[We have had time only to repeat a few of Dr. Wood's interesting experiments in regard to the remarkable influence which cadmium exercises in lowering the fusible point of various alloys. The alloy made by fusing together two parts of cadmium, two parts tin, four parts lead and eight parts bismuth melts at a temperature varying not far from 70° C. (158° F.) It may appropriately be called "*Wood's fusible metal.*"—Eds.]

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II. GEOLOGY.

1. *Note from* Dr. NEWBERRY, *in reply to* Mr. LESQUEREUX, (in a letter to the Editors).—I see by the note from Mr. Lesquereux, [contained in this Journal, xxix, 435,] that my letter from Santa Fé was unacceptable to him. This both surprises and grieves me, as the thought that he might be drawn into the controversy had not occurred to me; and I am sincerely sorry to learn that one with whom I have had so many years of friendly intercourse could so readily misconstrue both the statements and the spirit of my letter. Possibly its tone may have failed to reflect the great respect which I have had and still have for Prof. Heer; and to others than Mr. Lesquereux it may have seemed not altogether courteous. It should be borne in mind, however, that the discussion in reference to these fossil plants, and the age of the strata containing them, had already been repeatedly brought before the public; and that in this discussion the tone of the associate of Prof. Heer had been marked by a degree of arrogance difficult to bear patiently. Prof. Heer had called them *Miocene*—an error which with the imperfect material in his hands was natural enough; and one which should detract nothing from his high reputation—but by the testimony of several observers they had been proved *Cretaceous*. Ignoring their testimony, however, and adhering to his former opinion, a portion of his letter to Mr. Lesquereux was written to perpetuate what I knew to be a mistake. It also did me, as I conceive, manifest injustice. That letter reached me when I had been for months in exile, and where I was surrounded by proofs of the truth of the position I had before taken—circumstances favorable to the development of a little honest indignation. In the freshness of that feeling my reply was written, and I am willing to admit, if others think so, that it was not sufficiently respectful.

So much for the manner of my letter. In regard to its statements of fact I fear I shall be unable to make any such concession. On the contrary, my regard for truth requires that I should repeat each and all of them.

(1.) Prof. H. considered the plants in question Miocene. There is not the shadow of a doubt that they are Lower Cretaceous.

(2.) Prof. H. states that "*except Credneria* and *Ettingshausenia all the genera enumerated* (in my list) *are represented in the Tertiary and not in the Cretaceous.*" It will be observed that he does not say they are *characteristic* of the Tertiary, or "of the Tertiary," as Mr. Lesquereux quotes him—but distinctly affirms that they *are not represented in the Cretaceous.*" Hence there is no propriety in the remarks of Mr. Lesquereux on this point; and the error in the statement of Prof. Heer shown by reference to Stichler's paper, before quoted, remains unexplained. If that error was not accidental, it was designed. If accidental, as I cannot for a moment doubt, the offensive clause of my letter is no more than just. If designed, stronger language would be admissible. The appeal to "authority" has been nearly exhausted in this discussion, and the time has passed when personal influence could make errors pass for truths. Prof. Heer is a man of estimable character, of

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great learning and of world-wide reputation, and, I am sure, would be one of the last to ask us to believe a scientific statement simply because he had made it.

(3.) In my letter I made no supposition in reference to the Tertiary flora of Kentucky, Tennessee or Mississippi. I merely stated some facts in reference to the Miocence flora of the country bordering the Upper Missouri, 1000 miles distant from the nearest of these States. I also distinctly said that the absence of tropical plants from the collections made there, was only negative evidence. They may be found in that region to-morrow, but at the time of writing that letter they had not been found, and all the material in my hands indicates, as I then said, a Tertiary climate warmer than the present, but still temperate.

(4.) I was also fully aware that marine Tertiary deposits extend up the Mississippi even higher than stated by Mr. Lesquereux. I excluded them from "*the central portion of the continent*;" by this meaning, as I then explained, the region between the Mississippi and the Sierra Nevada. Here, too, the evidence is negative, but now stands just as I represented it.

(5.) Mr. Lesquereux says: "I cannot admit, as Dr. Newberry appears to do, that the fossil flora of the American Cretaceous, ought to be closely related to the European." My only reference to this question will be found on page 216 (Journal, March, 1860), where I say—"We may find hereafter, in other parts of the continent than those in which I have examined the Cretaceous strata, fossils which shall assimilate our flora of that period more closely to that of Europe, but, so far as at present known, our plants of this age present an *ensemble* quite different."

(6.) The statement made by Mr. Lesquereux that "the age of the strata from which American fossil plants have been taken is mostly uncertain," is manifestly incorrect. At least nine-tenths of the species enumerated are from the Carboniferous and Devonian rocks, whose place in the geological series is certainly well ascertained. Of those collected and not yet catalogued, perhaps an equal proportion have been obtained from the Cretaceous and Miocene strata, of which the places in the series have been as accurately determined, by the molluscous fossils which they contain.

(7.) It is true that in America fossil botany has had but few devotees, and doubtless all of them have at times keenly felt the want of more books and specimens bearing on their subjects of study. Still, I believe everything that has been published in reference to fossil plants is accessible to the American student within the limits of his own country. At the same time it is also true that a satisfactory comparison between the extinct floræ of Europe and America can only be made by means of full collections of well-marked *specimens*, many more than we yet possess.

Mr. Lesquereux is aware, as is every one who has given the subject any attention, that our knowledge of the floræ of the different geological formations has been limited, not so much by the want of learning and acuteness in the cultivators of fossil botany, as by the small number and imperfect preservation of the fossil plants collected. It could hardly

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be otherwise, then, than that in the whole New World material should be discovered which should throw new light on the ancient vegetation of the globe. The idea that no American can be qualified to make good use of such material, is another instance of the arrogance to which I have before alluded, and to which it would be unmanly tamely to submit.

III. BOTANY AND ZOOLOGY.

1. *Geological and Natural History Survey of North Carolina. Part III. Botany; containing a Catalogue of the Plants of the State, with Descriptions and History of the Trees, Shrubs, and Vines.* By Rev. M. A. Curtis, D.D. Raleigh, 1860: the First part only, the *Woody Plants of North Carolina.* pp. 123, 8vo.—We have turned over the pages of this popular exposition with much interest, and gleaned some valuable information. "Botanists will of course find fault with it," says the author, who we well know could write scientifically and profoundly enough, if he so pleased, but who has here come down to the level of his most unlearned readers, discoursed separately of trees, shrubs, and vines, and classified these in a fashion which might well shock the susceptibilities of a stickler for technical nomenclature and natural system in botany. Now, we are not shocked at all; indeed we quite enjoy a glimpse of Flora *en deshabille* and slip-shod, and are well aware how much easier it is, and how much better in such cases, to fit your book to its proper readers than to fit the readers to it. The fault we should find is not with the plan of this Report, but with the quantity. We could wish for more of it, for a volume as large at least as Mr. Emerson's Report on the Trees and Shrubs of Massachusetts. We quite like to see the popular names put foremost, but would suggest that the botanist who does this should *lead* as well as follow the indigenous nomenclature, so far as to correct absurd or incongruous local names. and introduce right or fitting ones as far as practicable. For instance *Virgin's Bower* is not a proper name for *Wistaria frutescens*, and is rightly applied to *Clematis Virginiana* over the leaf. (We venture to add, in passing, *C. Viorna* to the list, having gathered it in Ashe County.) And, although the people along shore call *Baccharis* by the name of the English annual weed, *Groundsel*, it were better to write it *Groundsel-tree*. *Yellow-wood* is the name of *Cladrastis*, rather than of *Symplocos*, which the Carolinians call *Horse-Sugar*. Dr. Curtis can coin a name upon occasion; for surely nobody in Carolina knows *Menziesia globularis* as *False Heath*, nor has it any scientific claim to this appellation. While in critical mood we may express a strong dissent from the proposition that *Rhododendron punctatum* is too inferior to the other two species "to attract or deserve much attention." With us, it is surpassingly beautiful in cultivation, none the less so because its habit is so different, having light and pendent branches, when well grown forming broad and thick masses, and loaded with its handsome rose-colored blossoms. While *Leucothoë Catesbæi* is called "a very pretty shrub," the far handsomer *Andromeda floribunda*, so much prized by our nurserymen, gets no commendation. *Magnolia Fraseri* may not only be "cultivated in the open air near Philadelphia," but is perfectly hardy near Boston, and the earliest to blossom; but we never noticed the fragrance of the flow-

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ers. On the other hand, as it is native as far south as Florida, it might thrive in plantations any where in North Carolina. The flowers of *M. cordata* are described as if larger than those of *M. Fraseri*, instead of the contrary; we could hardly say much for their beauty, except in comparison with those of the common Cucumber-tree. *Prunus Virginiana* is omitted; yet surely it is not wanting in North Carolina. And it is almost an excess of conscientiousness to leave out *Cladrastis*, the handsomest tree of the country, all things considered, when it is known to grow only a few rods over the Tennessee line.

On the other hand, we are disposed to doubt if the genuine *White Spruce*, (*Abies alba*,) occurs in North Carolina. At length we know this tree, but only in Canada and parts adjacent. It is more, instead of less northern in its range than *A. nigra*. But since President Wheeler has pretty nearly determined the existence of *A. Fraseri* on the Green Mountains in Vermont, we could not deny that *A. alba* grows with the latter on the high mountains of North Carolina. We make our little criticisms freely,—as we know the excellent author would wish,—for we think it likely that this part of the Report will pass to a second edition,—when we hope it will be largely augmented. A. G.

2. *Thwaites, Enumeratio Plantarum Zeylanicæ*, Parts I. II. 8vo, pp. 160. 1858–1859.—A complete enumeration of the known plants of Ceylon, with characters of new or little known genera and species, and numerous descriptions and critical remarks, the synonymy, &c., elaborated by Dr. Hooker. These two published parts extend from *Ranunculaceæ* to *Compositæ*; so that a good-sized volume will complete the work, and constitute an important adjunct to the great Indian Flora. A. G.

3. *Walpers, Annales Botanices Systematicæ*, continued by Dr. C. Müller, Berol.—Five parts of the fifth volume are published, extending to page 800, and to the order *Coniferæ*. A. G.

4. *Bueck, Index ad De Cand. Prodromum, etc.* Pars III. Hamburg, 1859. pp. 506.—This useful Index to De Candolle's Prodromus is here continued from the second part of the seventh to the thirteenth volume. As we may expect that at no distant period the Prodromus will be terminated, as announced, we trust that the next Index will combine the whole into one continuous alphabetical list. A. G.

5. *Synopsis Methodica Lichenum omnium hucusque cognitorum, præmissa introductione lingua Gallica tractata, scripsit* William Nylander. Fasciculus II, Parisiis ex typis L. Martinet via dicta Mignon, 2, 1860. 8vo. pp. 141–430.—We are glad to welcome another portion of the important work of Dr. Nylander, which is indispensable to every Botanical library. Beginning with the *Caliciei*, the present part embraces the *Bæomycei*, the *Cladonici*, the *Usnei*, and the *Parmeliei*, ending with the genus *Physcia*. The higher tribes of Lichens are by no means the least difficult, and nothing in the part before us is more acceptable than the author's elaboration of the genus *Sticta*;—disposed by him in *Sticta*, *Stictina*, and *Ricasolia*. The last general synopsis of Lichens, that of Acharius, was published nearly fifty years ago, and the vast amount of valuable matter, scattered in many publications, which has since been accumulating, has long needed to be brought together in one work. This Dr. Nylander proposes to accomplish, adding also the results of his investigation of all the

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most important collections; and disposing the whole in a system, which, while it aims to retain all that was most valuable in the old, gives us, as it should, the whole light of modern (microscopical) science upon both old and new. We have only room to add here that "*Usnea lacunosa, Willd.*," is a name found only in Willdenow's herbarium, and was anticipated in print by *U. cavernosa*, published by the present writer, in the appendix to Agassiz's tour to Lake Superior. Messrs. Westermann and Company of New York, will receive subscriptions to the synopsis, which is put at a moderate price for so handsome a book. E. T.

6. *Reports of Explorations and Surveys* to ascertain the most practicable and economical route for a Railroad from the Mississippi River to the Pacific Ocean, made under the direction of the Secretary of War, in 1853–6, &c., vol. x. Washington, 1859.—FISHES; by CHARLES GIRARD, M.D. Washington, D. C., 1858.

Of recent contributions to our knowledge of special Faunas, none have been of greater importance or interest than the report on the Fishes of Western North America. In this volume, Dr. Girard has incorporated almost everything known to the date of publication concerning the Ichthyology of our Pacific possessions.

In the introductory remarks, (which with some variations, are duplicated) a general view is given of the Piscine Fauna of Western North America. The families which are richest in genera and species, and which are most characteristic of the Californian Fauna especially, are those of the Cataphracti, the Blennoids, the Embiotocoids, and the Pleuronectoids. Of all these families, many new genera, previously indicated in the "Proceedings of the Academy of Natural Science of Philadelphia," are described and illustrated.

The classification of the late Johannes Muller is adopted, and the orders, suborders and families of which representatives are described, are all characterized; the genus *Amblodon*, however, is retained in the family of Sciænoids; this should in strict accordance with the principles of that classification, be transferred to the order of Pharyngognathi. Agassiz has demonstrated the union of the lower pharyngeal bones, the only character on which the order depends. Dr. Girard does not appear to have noticed this discovery as he has not adverted to it in the generic diagnosis of *Amblodon*.

Of the family of Percoids, representatives of only one genus are yet known as inhabitants of the Pacific coast. This genus has been described as new under the name of *Paralabrax*, and includes two species, both of which had been first referred to *Labrax* in the Proceedings of the Academy. It is here placed in the vicinity of *Serranus*.

Many fresh water Percoids are described, belonging to the genera *Dioplites* Raf., *Pomoxis* Raf., *Ambloplites* Raf., *Calliurus* Raf., *Bryttus* Val., *Pomotis* Raf., *Labrax* Cuv., and *Stizostedion* Raf. The species described have been collected in many distant places west of the Mississippi river, but one species (*Ambloplites interruptus*) having been obtained in California.

The name of *Dioplites* has been substituted for *Grystes* of Cuvier. It would by many have been deemed more proper to have applied Rafinesque's generic name of *Lepomis*. Rafinesque in his "Ichthyologia Ohi-

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ensis" has characterized the genus *Lepomis* and divided it into two subgenera, *Aplites* and *Dioplites*, giving at the same time, to each of the species, the generic name of *Lepomis*—*Aplites* and *Dioplites* having been separated in consequence of an error of observation, and not differing from each other, cannot be retained—*Lepomis* must be therefore used, as it should have been for one of the genera or subgenera, if both had been established on true principles. *Dioplites* is restricted by Dr. Girard to the species without teeth on the tongue.

The Cuvieran section of "Percoids a Joues cuuirassés" is retained as a "tribe" under the name of Cataphracti, and is divided into three families, *Heterolepidæ*, *Cottidæ*, *Scorpænidæ*. Perhaps the families so indicated are valid, but the characters given to them are vague and will require revision after a comparative study of the foreign genera.

The family called Heterolepidæ had been previously named by Swainson Chiridæ, and that name should have been retained, as well on account of its priority, as its consonance with the terminology of the other families.

In the family of Cottoids, the species are distributed into ten genera, all of which appear to be founded on good characters, but the names of some of which are objectionable.

Among the Salmonoids, the three genera of Valenciennes, *Salmo*, *Fario* and *Salar* are accepted, but we notice that Dr. Girard has named all the new species he describes as belonging to those genera, Salmo—"*Grd. MSS.*" in the synonymical lists of the species.

In the family of Clupeoids, the modifications of Valenciennes have not been adopted, and the genus *Hyodon* is interposed between *Meletta* Val. and *Engraulis*.

The genus *Anarrhichthys* of Ayres is adopted; its only species is called *Anarrhichthys felis* Girard. To this name we desire to draw the attention of our readers, as an important question of nomenclature is involved.

In the "Proceedings of the Academy of Natural Sciences of Philadelphia" for 1854, Dr. Girard mentions a fish which he calls *Anarrhicas felis*, and observes that two large specimens were received in such a precarious state of preservation that there was no probability of keeping them, and that having mislaid the notes, *no diagnosis could be given.*

Subsequently, Mr. Ayres, in the Proceedings of the Californian Academy, gave a full description of a species which he referred to a new genus and called *Anarrhichthys ocellatus*, which was chiefly distinguished from *Anarrhicas* by its anguilliform body, and the union of the dorsal, caudal, and anal fins. This is the species that Dr. Girard has claimed as his own *Anarrhicas felis.*

To this reference we would remark that as *Anarrhicas* has by all modern naturalists, been restricted to such species as had the dorsal and anal fins separated from the caudal, we would infer that any species placed without comment in the genus would have those characters. Dr. Girard's name of *Anarrhicas felis* was not only without any description whatever and therefore not established, but a statement by implication was made that the species possessed the stout body and fins of *Anarrhicas*, and was consequently in direct opposition to the characters of *Anarrhichthys ocellatus.* Such being the case, Dr. Girard's name cannot be adopted, and that of Ayres must be retained.

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The family of *Golidæ* is limited to the species with the ventral fins united in the manner of a funnel and thus excludes the *Eleotroids* as well as the *Cyclopteroids*. The latter are very properly regarded as distinct, but the propriety of excluding the former from the *Gobioids* is more doubtful.

The genus *Gobius* is limited to the species with cycloid scales. The name cannot be retained for such species, as the genus had by several naturalists been previously restricted to species with pectinated scales. Mr. Gill has framed for the two species thus referred to *Gobius*, a genus which he has called *Lepidogobius*.

We find that in the family of Cyclopteroids, the Gronovian name of *Cyclogaster* is substituted for the Artedian name of *Liparis*. *Liparis* has been almost universally accepted by naturalists, and being the prior name, should be adopted. No description is given by Artedi of the generic or specific characters of *Liparis*, but the references he has given are full and ample, enabling us without doubt to ascertain what is meant.

In the Proceedings of the Academy of Natural Sciences, Dr. Girard has described a genus under the name of *Homalopomus* which he has referred to the Trachinoids. This is now referred to the Gadoids, and a doubt is even expressed whether it is distinct from *Merlangus* or *Merlucius*. It does not appear to differ from the latter genus. The cause of the former erroneous reference is attributed to the broken tips of the rays of the specimen on which the species was founded.

The family of Embiotocoids is rich in generic forms, nine being described and illustrated, and references being made to the descriptions by Dr. Gibbons of five others which Dr. Girard was unable to identify with his. Some of the species described as new by Dr. Girard will probably be found to have been previously indicated by Gibbons.

In the remarks on the family, no mention is made of the presence of the two rows of lamellæ which are present on each of the branchial arches, and which was especially noticed by Prof. Agassiz. This character has been regarded as having considerable importance by many ichthyologists and being one of the best distinctive ones of the family, the fourth branchial arch of the Labroids having but one row of lamellæ.

Dr. Girard claims "that the real knowledge of the remarkable peculiarities concerning some of their habits was obtained in the spring of 1852, by Dr. Thomas H. Webb, while attached to the United States and Mexican Boundary Commission."

The genera described and illustrated are *Embiotoca* Ag. with seven species, *Damalichthys* Girard, *Phanerodon* Grd., *Abeona* Grd., *Rhacochilus* Ag., *Hysterocarpus* Gibbon, *Holconotus* Ag., each with one species, *Ennichthys* Grd., and *Amphistichus* Ag., each with two species.

These genera are chiefly characterized by the comparative size of the head; the character of the lips, and the attachment or non-attachment of the lower one by a frænum to the jaw, the comparative protractility of the premaxillaries; the number of rows of teeth on the jaws, and the outline of the dorsal fin.

Dr. Girard has given some information on the embryology of the Embiotocoids, which will prove of general interest. He denies the presence of any resemblance between their gestation and that of the marsu-

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pial mammals. Want of space forbids us to make extracts from the recorded observations, and we must remain satisfied with referring to the text and plates of the report.

Of the Cyprinoids, numerous genera and species are described from almost every portion of the west. The family is divided into the tribes of 1. *Cyprini* with teeth of the molar kind, of the grinding type, 2. *Catastomi*, with pectiniform teeth; 3. *Chondrostomi*, in American species of which there are no barbels, and the teeth are of "the grinding type and cultriform kind;" 4. *Pognichthyi*, in which barbels are present, the snout is prominent, and the pharyngeal teeth are more or less hooked; and 5. *Alburni*, which differ from the *Pogonichthyi* by the absence of barbels.

The author has devoted much time and research to these groups, and all of the genera may be good, but some of them appear to be distinguished on quite slight grounds, and many of the species are small and perhaps the young of others. But on this question we will not venture to disagree with Dr. Girard.

Most of the new genera have received Indian names, and although not cacophonous, some of those names appear strange to ears that have been mostly accustomed to Latin and Greek derivatives.

In *Dionda*, a genus of Cyprinoids, Dr. Girard has named two species collected by Capt. John Pope, *Dionda episcopa* and *D. Papalis*.

The genus "*Argyreus* Heckel" is synonymous with *Rhinichthys* of Agassiz. It is not probable that Dr. Girard will be sustained by Ichthyologists in this application of Heckel's name. An extract from the remarks of the learned Doctor himself on the nomenclature of the genus will show the history of the name *Argyreus*.

"Heckel includes in this genus two species which are generically distinct *Cyprinus atronasus* Mitch., and *Cypr. rubripinnus* Mus. Par. MS. But *Cyprinus rubripinnis* is identical with *Leuciscus cornutus*; and since *Leuciscus cornutus* is to enter the genus *Plargyrus* of Rafinesque, *Cyprinus atronasus* remains as the type of the genus *Argyreus* which again is identical with *Rhinichthys*. It must be recollected, however, that the teeth figured by Heckel under the name of *Argyreus rubripinnis* are those of *Plargyrus cornutus*."

The diagnosis and illustration of *Argyreus* having been by Heckel founded on "*Cyprinus rubripinnis*" and not answering to *C. atronasus*," the former species in the type of the genus, and if Rafinesque's name *Plargyrus* is adopted, Heckel's name must be treated as a synonyme. Heckel would scarcely have referred a species of *Rhinichthys* to the same genus as *C. rubripinnis* if he had known the pharyngeal teeth, and he could only have done so from an ignorance of the species. Because a species on which a genus is established belongs to a previously named genus, it by no means follows that the generic name has to be used for another species of the genus, when it proves to be distinct from the type.

If the above views are correct, *Rhinichthys* will have to be retained for the genus called by Girard *Argyreus*, and the two species described by him in his report must be called *Rhinichthys dulcis* and *R. nubilus*.

The Lepidostei have been separated apparently on good grounds into three groups distinguished by the comparative length and breadth of the

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snout, and the presence of one or two rows of teeth in the upper jaw. For two of these groups, Rafinesque's names of *Cylindrosteus* and *Atractosteus* have been adopted, although to Dr. Girard, the credit of first giving them valid characters is due.

In the Plagiostomes, we have some interesting additions. A second species of *Triakis* (*T. semifasciatus* Grd.) is described. Probably this is the species noticed as *Triakis californica* in the list of Chondropterygii of the British Museum, but as this name is unaccompanied by a description, Girard's name must of course be retained. A new species of *Heterodontus* is described which is called *Cestracion francisci* Grd. But the name *Heterodontus* of Blainville must be retained for the Cuvieran *Cestracionte*s, and the species must be consequently called *Heterodontus francisci*. The *Notorhynchus maculatus* of Ayres is referred to the genus *Heptanchus*, Raf.

Among the Rays, a second species of Muller and Henle's genus *Uraptera* is made known.

Among the Cyclostomi we perceive that Dr. Girard has not only retained the genus *Ammocœtes*, but he has even separated from it a new genus which he has called *Scolecosoma*. The researches of A. Muller have demonstrated that the *Ammocœtes* are only the young of Petromizontoids, and there is no reason to doubt his accuracy. Dr. Girard must have been acquainted with these researches, and it would therefore have been more advisable not to have added to the number of names, until it was certain, as may possibly be the case, that there are fishes of the *Ammocœtoid* type which are adult.

We have now concluded, and although we think that there is cause to dissent from the author in many cases, we most cheerfully bear witness to to the general ability with which the work has been performed, and to the very great advance in our knowledge of the Fauna of our Pacific possessions.

To the Smithsonian Institution, we are indebted for the accumulation of the materials which have been used in the elaboration of the report. Although published by the liberality of the General Government, it is one of the valuable "contributions to knowledge" which we owe to the fostering care of that Institution. T. G.

IV. ASTRONOMY AND METEOROLOGY.

Solar Eclipse of July 18, 1860.—From the numerous accounts which have reached us of the observations made upon this Eclipse we place the following before our readers.*

1. *Notice of the Astronomical Expedition to Cape Chudleigh* (or *Chidley*), *Labrador*, (in a letter to one of the Editors).—*Dear Sir*:—The American Astronomical Expedition despatched to the coast of Labrador for the purpose of observing the total solar eclipse of July 17th, (astr. time), 1860, sailed from the Navy Yard at Brooklyn on the morning of the 28th of June last. The expedition had been organized by the accomplished and energetic Superintendent of the U. S. Coast Survey, under

* For LeVerrier's account of the French Expedition, see *Postscript*, p. 309.

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authority of joint resolutions of Congress passed at the late session: and consisted of the following gentlemen, viz: Prof. Stephen Alexander, of the College of New Jersey, Prest. F. A. P. Barnard, of the University of Mississippi, Lieut. E. D. Ashe, R. N., director of the Quebec Observatory, Prof. C. S. Venable, of the College of S. Carolina, and Prof. A. W. Smith, of the U. S. Naval Academy at Annapolis. With these were associated the Commander of the Coast Survey steamer *Bibb*, conveying the expedition, Lt. Alexander Murray, U. S. N. Messrs. P. C. Duchochois, of New York, and J. P. Thompson, of the Coast Survey service, accompanied the corps for the purpose of taking photographic impressions of the eclipse; and Mr. W. A. Henry, of Washington City, attended as assistant to the chief of the corps.

In addition to the purely astronomical objects of the expedition, advantage was taken of the opportunity it presented, to secure determinations of the important magnetic elements at the observing station, as well as meteorological and other observations continued throughout the entire period of absence. The thermometrical and barometrical observations were kept up hourly from the time of sailing until the morning of the day on which the Bibb entered the harbor of Newport. On the day of the eclipse the intervals were reduced to a half hour. Frequent record was made of the surface and deep sea temperature of the water.

The magnetic observations were placed under the charge of Messrs. Edward Goodfellow and Samuel Walker, of the Coast Survey. The meteorological, under that of Prof. Venable, of the astronomical corps, assisted by Oscar M. Lieber, Esq., of South Carolina. Mr. Lieber also undertook such observations of the geology of the coast as the opportunities afforded would permit.

The history of the expedition and its results have been communicated by Prof. Alexander, the chief of the corps, to the Supt. of the Coast Survey, and will probably be, at a suitable time, presented to the public in full. The present memorandum is furnished by request, and with the permission of the Superintendent, with the view of presenting in concise form the facts of most immediate interest to the scientific public.

The track of the central eclipse left the eastern coast of Labrador in lat. 59° 51½′. On the evening of the 13th July, the expedition had reached this parallel; and was, for several hours, engaged in the endeavor to penetrate the seemingly unbroken and gloomily frowning barrier of precipitous cliffs which marked this rock-bound shore. The navigation at this point was of the most hazardous character. It was necessary to feel every foot of progress with the lead, and the successive soundings, which gave everywhere, a rocky bottom, showed the most singular and sudden variations of depth. One cast, for instance, would give ten fathoms, the next, no bottom with nineteen, and the next again, seven or eight fathoms. On every hand were seen rocky islets, rocks nearly submerged, or reefs and breakers.

Through such a sea, for five or six hours, from six o'clock till nearly 12 at night, the Bibb was engaged in cautiously seeking out for herself a harbor of refuge; and just about at the moment when the sun was passing his lower culmination, though a bright twilight still filled the atmosphere, she dropped her anchor in the inlet which divides Au-

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lezavick Island from the main land of Labrador. This was the point which had been previously assumed to be fittest for the purposes of the expedition; but so imperfect and even erroneous had all the charts of the coast been found to be, that even the existence of the island was regarded as doubtful; and the identity of the inlet was not fully recognized until further celestial observation and some hydrographic exploration and survey had established it.

During Saturday, July 14, a location was fixed on for the encampment, and some progress was made in the shore arrangements. The harbor, once entered, proved to be commodious and secure; and it was found practicable to moor the *Bibb* so near to the land as to enable a portion of the party to sleep on board of her, and thus to avoid some of the discomforts of camp life, which, in a region so bleak and dismal, are by no means trifling. Others, including the meteorologists, the magnetic observers, and the members of the astronomical corps in charge of the transit instrument, were compelled to make a larger sacrifice of their ease. What that amounted to may be appreciated, when it is mentioned that several of the tents were blown down almost immediately after their erection; and that a wind as disagreeable for its piercing chilliness as for its force prevailed with little intermission during the entire stay of the expedition, amounting to eleven days. The tents when reërected, were secured, or anchored, by piling rocks upon the margin of the canvass.

The exact latitude of the observing station was a few seconds short of 59° 48′; the longitude, by chronometer, $4^h\ 16^m\ 53^s$ west from Greenwich.

Hardly had the landing been effected when there commenced a storm of wind and rain which rendered any attempt to use the transit instrument impossible for two or three days. Even up to the Tuesday night preceding the eclipse, the clouds had been so persistent as almost wholly to prevent the observation of the stars. On that evening the sunset seemed to hold out better promise for the coming night and the ensuing day. But on Wednesday morning the prospect for the day was more than doubtful. Fleecy cumulus clouds made their appearance in numbers constantly increasing; and at the moment when the eclipse began, the sky was more than half covered. The sun was, however, totally unobscured at the beginning and for a large portion of the time during the progress of the eclipse. Clouds were, however, continually drifting over it, concealing it for brief intervals entirely from view. At the very close, the alternations of sun and shade were so rapid, that it became a question of doubt whether the final contact would be secured, and a flutter of excitement prevailed throughout the observing corps: but the last four or five seconds presented the sun's eastern limb entirely unobscured; and the desired point was satisfactorily gained.

Just previously to the time of the sun's total immersion, a thin veil of cloud intervened between it and the observers, not dense enough to intercept the direct rays of the luminary, but too dense to allow the corona surrounding the dark moon during total obscuration to be visible. Lt. Ashe was fortunate enough, however, to catch one point of brightness and to fix its position in this corona; and this may serve a useful purpose hereafter, in corroborating observations elsewhere made,

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under more favorable circumstances, in regard to the features of this beautiful and rare phenomenon. The bright point observed by Lt. Ashe was white and not ruddy. The expedition are unhappily unable to bear any testimony in regard to the roseate clouds which have been so often seen during total eclipses upon the moon's border. This has been a subject of great regret, the more so because the corona which was lost to the astronomical party, was not lost to such of the ship's company as remained on board of the Bibb. From the description given of it by these, the chief of the corps has prepared a drawing, in which there appear four principal radiant beams extending outward beyond the general limit of the coronal luminosity, in positions sufficiently well fixed to admit of comparison with observations made elsewhere. But as this class of observers were not furnished with instruments, they gave no testimony as to the positions, or even as to the presence of rosy clouds.

The whole astronomical corps observed the breaking up of the last line of solar light lingering before total obscuration, into the fragments commonly called "Baily's Beads," from Francis Baily, President of the Royal Astronomical Society, by whom they were described in the Mem. Astr. Soc. for 1837, as observed by him in the annular eclipse of 1836. These fragments were very evanescent, and were not preceded by those longer dark filaments or ligaments noticed by Mr. Baily on the same occasion, and more or less perfectly by others since. At the emergence of the sun, the beads were not noticed, owing probably to the veil of clouds. Only two of the observers attempted, in fact, to fix the exact second of emergence.

The darkness which prevailed during total obscuration was not as remarkable as had been anticipated by most of the observers. The present writer, for instance, found no difficulty in making pencil notes at this time, or in reading lines written in pencil in other parts of his note book. It was not necessary to bring the book nearer to the eye than usual.

The pallor or ghastly appearance which has been remarked at such moments in the human countenance by former observers, did not strike the members of this party, though it was looked for.

There was something indeed about the character of the gloom which was unusual and impressive, but it scarcely effected the tints of objects or rendered the face of nature very different from what it appears during early twilight. Clouds covered at the time almost nine-tenths of the heavens; and in the intervals of the clouds the blue of the sky was intensely deep and dark. On one side only was the horizon unobscured. This was on the north where the harbor opened out to the sea; and in this direction a beautiful rose and orange flush presented itself.

It would be easy to extend this notice to much larger dimensions, if space in the present number of the Journal was available.

The instruments employed in the astronomical observations, were a thirty-inch transit by Fitz, a fifty-one inch Fraunhöfer achromatic belonging to Princeton College, a forty-two inch by the same maker belonging to Columbia College, one of similar dimensions belonging to Lt. Ashe, a thirty-inch equatorial belonging to the Naval Academy at Annapolis, a three foot alt. and azimuth, belonging to the College of

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South Carolina, and a mammoth comet-seeker, of 7½ inches aperture, by Fitz. An equatorial belonging to Mr. Rutherford of New York city, a gentleman well known for his disinterested zeal and efficient labors in the cause of astronomical advancement, served the photographers of the expedition to fix from time to time the successive phases of the eclipse. Another comet-seeker was fitted up for the purpose of furnishing an image of the sun upon a white ground in a darkened chamber.

Fifteen auroras were observed during the absence of the expedition. In nearly every instance a corona was repeatedly formed, though many of the auroral clouds were exceedingly filmy and thin.

Atmospheric electricity was almost or absolutely *nil* during the entire stay of the expedition at the observing station. The passing of the shadow seemed to produce no change in this respect.

The diurnal magnetic variation was very large, varying from two to five degrees. During the eclipse the needle was more quiet than before or after. The surface temperature of the sea was very low from the Straits of Belle Isle northward. It was frequently down to 38° or 39°. But the lowest surface temperature at any time observed, was in the Straits themselves, on the return passage; when the thermometer marked, in the surface water, 32°.

In the harbor at the observing station, ice formed in the shoals near the beach on the 13th of July. On Sunday, July 22, there occurred a storm of snow and sleet, which covered the deck of the steamer, and wrapped the whole surrounding country so far as visible, in a mantle of white. On the return passage, this snow was observed still enveloping the mountains far down the coast.

Scarcely any quarter of the world presents, perhaps, more difficult or dangerous navigation than the coast of Labrador. The islands, islets, submerged rocks and reefs are absolutely innumerable, and icebergs swarm where these more fixed dangers are wanting. The month of July was pronounced by the hardy mariners engaged in the Labrador fisheries, to have been one of the most tempestuous ever known in those seas, and most prolific of disaster to their fishing vessels. It will therefore occasion no surprise to state that the Bibb has been repeatedly in positions of hazard, requiring all the resources of her officers to meet successfully. The members of the corps cannot but feel that the hand of a protecting Providence has been more than once distinctly visible in preserving them amid dangers, and delivering them from situations, to which they can hardly look back with tranquillity. F. A. P. B.

2. *Extract of a letter from the Superintendent of the Coast Survey to the Editors in relation to observations made on the Western coast of the United States, for the Coast Survey*, by Lieut. J. M. Gilliss, U. S. N.—Lieut. Gilliss arrived at the station selected by him for observing the eclipse, and which is near Steilacoom, Washington Territory, on the 9th of July. Here he encamped and made his preparations for observations of time, latitude, etc. These are not yet definitely worked up and hence I do not give the position of the station or the times of the different phenomena in detail at present. The following particulars from Lieut. Gilliss's report will be found of interest in anticipation of the time and longitude results:

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"For the first time after our arrival at the station, the sun rose clear on the morning of the 17th, nor was there at any time during that day more than two-tenths of the sky obscured by clouds. Yet, although the evening was absolutely cloudless, and the stars were shining with remarkable lustre after midnight, so fickle had been the climate during the preceding three weeks, that when we closed the tent, three hours before the eclipse would begin, I had no confidence that the next morning would be favorable for observation.

By 3½ A. M. we were up and had removed the meteorological instruments from camp to the knoll. At that time it was sufficiently light to write without artificial aid. Mt. Rainier was distinctly visible and sharply cut against the southeastern sky. Beyond it and towards the point at which the sun would rise, there was a stratum of vapor whose upper line was slightly inclined from mid height of Rainier towards the northern horizon. At that time the barometer stood at 29·698; att. thermometer 44°·5, the temp. of the air 45°·2, and there was only a very slight air from the southward. At ($0^h\ 17^m$, sid. chron.) the mist striæ became dense to the N. and E., and were more evidently in rays diverging from the point of sunrise to an elevation of some 25°. The air was so cool and so loaded with moisture that although the telescope had been out all night the object glasses were densely covered with dew immediately after the caps were removed. By ($0^h\ 30^m$) a part of the vapor in the N. and E. had condensed into little cumuli beyond the Cascade range each more light and feathery with distance from diverging point, though none of this series extended as far as Mt. Rainier, and it was only towards the north that a dense volume of vapor could be seen coming in towards the lower lands bordering on Puget's Sound. Two minutes later and the edges of the little flocculi were tipped with pink and golden hues increasing in brilliancy of color as the sun approached the horizon.

The eclipse had far advanced when the first cusp appeared above the horizon at ($0^h\ 39^m\ 38^s$). It was seen through a red screen glass and was sharp and without tremor. Indeed the atmosphere was so still that the rise of the second cusp over the distant ground line at ($0^h\ 40^m\ 58^s$) was observed almost with the precision of a transit of a limb over the wires of a telescope. But it was at once perceived that there was great disturbance of the lune, the lower half being flattened by the unequal refraction.

At this time I was again obliged to wipe the heavy drops of dew from the object glass of the telescope, and whilst so doing my attention was directed to the vapor near us. The whole northeastern portion of the prairie had apparently been converted into a placid lake with here and there a knoll projecting through and forming a minature isle, the illusion being enhanced by rapidly diminishing intensity of the light. At ($0^h\ 54^m$) distant objects could not be recognized more distinctly than during midsummer twilight at 8½ P. M.

At ($0^h\ 55^m$) the southern cusp had become rounded off and rugged as though the moon's edge was serrated. But had such been the case this portion of the lune would have broken into beads of light before the total obscuration took place, and that did not occur, the moon's disc equally and uniformly interposing between us and the sun until the last glimmer of light disappeared.

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I had turned off the red screen half a minute before and was surprised to behold quite distinctly the following segment of the lunar sphere. The periphery of this segment was more than 100°. Its color was uniformly shaded from an intense black at the centre of the lunar disc to a very dark grayish purple near the limb of the sun. It was still traceable during twenty seconds after the last glimmer of sunlight.

At the moment of totality beads of golden and ruby-colored light flashed almost entirely around the moon not constant even for a second at one point but fitfully flashing as reflection from rippled water, and as mutable in the respective places of the colors. This bead-thread could not have extended more than ten seconds beyond the lunar disc. It broke up suddenly at (1^h 16^m $21^s.2$ sid. chron.) and then for the first time protuberances were noted beyond the following limb of the moon. The position of its largest one was 75° or 78° W., and in the form of a flattened cone or pyramid of cumulus cloud about one minute in height and when first observed perhaps two minutes broad at base. The cloud was not a uniform mass but apparently an aggregation of small ones, and its general tint was a rosy pink with occasional spots and edges of yellowish white light as though sunlight shone obliquely through them. Except in the pink color it greatly resembled the protuberance noted during the total eclipse of the sun observed at Olmos, Sept. 7th, 1858. As the moon advanced this protuberance was probably broader at the base and brighter at the summit while its apparent elevation remained the same. This was an extremely beautiful sight, and I watched it closely, giving nearly all my attention to it during 15^s, yet at the same time I was able to perceive a lesser one of a more flattened appearance distant 10° or 15° towards the west, and several others yet smaller and one long one of a much darker color at different points of the disc. Intently occupied with the great protuberance, the corona had not been recognized up to this instant (0^h 58^m 10^s), interest in the former causing me also to drop the beat of the chronometer.

It was then so dark that I found it impossible to recognize the second's dial of the chronometer (the gold one) and Mr. James Gilliss was called to bring his lantern and read the time at which I should indicate the second internal contact of the limbs. Raising my face from the box on which the time keeper stood to the telescope a most extraordinary scene was apparent! Over the moon's black disc colors of the spectrum flashed in intersecting circles of equal diameter with that body, and each apparently revolving towards the lunar centre. The moving colors were not visible beyond the moon, but a halo of virgin white light encircled it, which was quite uniformly traceable more than a semi-diameter beyond the black outline. This corona was composed of radial beams or streamers, having slightly darker or fainter interstices rather than a disc of regularly diminishing or suffusing light; but the gorgeous appearance of the spectrum circles with their incessantly changing bands of crimson, violet, yellow, and *green*, thoroughly startled me from the equanimity with which the preceding phenomena had been observed. Nor were these colors physiological results from a change of position of the body, or of preceding strain of sight in efforts to recognize the division of the second's dial, in darkness, and subsequent direction of the eye towards the

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sunlight, for they continued visible with the telescope at least 10ˢ longer. As near as it was posible to estimate, the breadth of each spectrum circle was about two minutes. The green colors were not darker than the tint usually called pea green, and were on the edges farthest from their respective centres, but neither of the lines seemed to retain a definite position, and I was irresistibly drawn to their contemplation to the neglect of all the changes that might have been taking place in the protuberance and corona.

They vanished with the first appearance of sunlight beyond the western limb of the moon, their sudden obliteration causing me to utter an exclamation which was regarded as the signal for noting the time, a datum whose importance had been wholly forgotten in the fascination thus caused. I cannot liken them to anything so nearly as to the image seen in the kaleidoscope."*

3. *Observations made during the Total Eclipse of 18th July, 1860, on the summit of Mount Saint-Michel, in the Desert of Palmas, Spain;* by A. Secchi, S. J. Communicated to the French Academy of Sciences. (*Comptes Rend.*, li, p. 156, July 30, 1860.)—The place where I observed the eclipse was on the top of Mount Saint-Michel, in the desert of Palmas, at the same point chosen by Biot and Arago for their triangulation operations, and at a height of 725 metres (=2378 ft.) above the sea, commanding a very wide horizon. The weather was magnificent during the whole time of obscuration, notwithstanding we were tormented with a cruel anxiety up to a few moments of the time by parasitic clouds forming continually on the mountain and dissolving only when at some distance. But they happily disappeared just before the critical moment, and the sky was fine till evening.

I was accompanied by M. de Aguilar, director of the Madrid Observatory, and by Mr. Cepeda, lawyer from Valencia, a distinguished amateur.

The instruments were a Fraunhöfer telescope of 78 mm. aperture (about 3·04 inches) and 1ᵐ·20 (=47¼ in.) focal length, with powers of 60, 90, and 130 diameters. The two first powers gave the entire disc of the sun, and the three oculars being mounted on a slide could be changed with the greatest rapidity. The micrometer carried a system of six spider lines, with spaces of 6′ (invisible in the dark), and of four very fine platinum wires so disposed that there was a space just equal to one lunar diameter between the outer wires—the two intermediate wires were slightly inclined, subtending 1′ 30″ at the narrower, and 2′ 30″ at the larger angle; an arrangement designed to aid in obtaining a more exact estimate of the protuberances. The whole micrometer revolved on a transom, with a plate on which was a graduated circle and a sheet of white drawing paper upon which the angular position was marked by touching a pencil, carried by the fixed transom—thus reserving the reading until the observations were concluded. This instrument was mounted equatorially, was very stable, and had been adjusted the day before.

* Up to this date, Sept. 7th, the party sent to the Cumberland House, British Columbia, have not been heard from.—Eds.

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Some minutes before the commencement of observations I verified the position of the telescope and the commencement was marked by a Morse telegraph, kindly procured from Madrid by M. Aguilar, provided with a pendulum which marked the seconds. A simple mechanism marked the instant of observation. Some minutes after the commencement I sought for the disc of the moon outside of the sun but could not make it out. At $2^h\ 19^m$ I succeeded in seeing very clearly through an arc of about 10° or more, but some time after the moon disappeared, and after that it could be observed only for an instant. Is this due to the inequality of the portions of the solar corona, upon which the disc of the moon is projected? I observed with certainty that not only the edge of the solar crescent was more sharply defined upon the side of the concave phase, than upon its own proper border, but also that the field of the telescope was much more clear upon this side than upon that of the moon, and the same could be seen in projecting the solar image on white paper.

The cusps remained throughout very distinct, and the solar spots were successively eclipsed without any distortion as viewed with a magnifying power of 90. The lunar mountains were well outlined upon the solar ground, and indented the inner border of the limb. After the centre of the sun was hidden (and even a little more) the light of the horizon diminished suddenly in a decided and unexpected manner. Surrounding objects did not however noticably change color. As the eclipse was becoming nearly total, I took away all the fixed colored glasses, and followed the sun with a glass held in the hand. This was an excellent glass of neutral tint made by Lerebours, a graduated light, the lighter shade being very delicate. The slender crescent is now breaking into many parts near the cusps which still remain very distinct, and the corona begins to show itself even with the dark glass. The sun reduced to a simple thread disappeared without forming (*grains des chapelet*) Baily's *Beads*. Quickly taking away the colored glass I was surprised to see the sun yet white and its light so strong that it hurt my eyes, but its splendor visibly diminishing and changing to a purple light, which at once appeared to terminate in an infinity of purple points, which were as soon hidden, and then two great red protuberances appeared near the point of occultation. One of these was at least $2'\ 30''$ in height, and as large at the base as $2'$; it was conical in form, slightly tapering, curved at the top. The other was about half the height of the preceding, extending over a considerable arc, at least 5° upon the solar border. Its top was formed like very minute saw teeth, parallel to the border of the moon. I looked as soon as possible at the opposed margin of the sun, but nothing appeared. Returning to the first margin I saw that the protuberances were rapidly hid. During the whole time the corona was magnificent but most brilliant on the side on which the eclipse began. Its light was all around uniform and without interruption, of a beautiful silver white, shading off gradually from the margin of the moon to the distance of about the lunar radius or less. At this distance there began to be many interruptions; large sheafs of light appeared, those of the upper part were the largest, and extended out to a distance equal to a diameter and a quarter of the moon. On

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the lower part I saw only one of these long sheafs. With Arago's polariscope, already directed very near the sun, I ascertained that the two images were not of equal brilliancy, and that the corona in one was lengthened in one direction, and in the other in a direction perpendicular to the first, but I could only give some seconds to their examination.

Returning to the telescope, I regarded for an instant the imposing scene which was then displayed in all its majesty. The moon perfectly black showed itself with all the glory of its rays, which appeared lengthened below and to extend out to the distance of two solar diameters. The heavens were of a light ash color, but not of threatening aspect. Near objects were plunged into feeble twilight contrasting with distant objects not yet in shadow. All this unique scene remains profoundly engraven on my mind; the solemnity of the spectacle appeared forcibly to impress the assistants, who, though numerous, all remained in perfect silence. Not to lose these precious moments I returned immediately to the telescope. The aspect of the sun was much changed. The two great protuberances of which I have spoken had already disappeared, and a great number of others appeared from on all sides of the sun (this moment corresponding to the middle of the total obscuration), I was for an instant embarrassed to decide which to select for measurement of their angle of position; for it was useless to measure the size, which changed while looking at it. With the mechanism of the micrometer in a few seconds I determined six—although I counted at least ten—there was hardly any part of the surface of the disc where there was not a point, they seemed regularly distributed. These are the angles taken in reckoning from east to northwest. South: 39·0°, 75·0°, 116·0°, 173·0°, 211·3°, 310·0°.

A greater brilliancy of the corona on one side announced that the sun was emerging, then in directing my attention to this side I was astonished to see a very large number of very small protuberances, and above all of them a red cloud entirely detached which was suspended and separate from the rest and from the lunar margin by a marked white space. Its figure was elongated, about 30″ of length to 3″ of width, and its form somewhat tortuous and sharp at the extremities (I called to my companions to witness this). This cloud was not alone. I had the conviction that it was accompanied with many others which rested at nearly the same level as a series of cirrus. Their color was that of the protuberances, only a little more distinct.

During all this the number of protuberances increased greatly upon this side and soon assumed a continuous arc formed like a saw, which extended at least to 60° of the circumference, and which gradually lengthened while its central part increased in width and brilliancy. The purple color mingled with the white in gradual transition until the white became so strong that the eye could not sustain it; the protuberances then disappeared.

The sun then began to shine in the heavens like a point of true electric light, and made a singular contrast with the corona still remaining, and which (by hiding with my hand the bright part) I could see for 40 seconds longer. That which most struck me in the circumstances, was the immense quantity of red protuberances and their distribution; so

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that one could absolutely say they enveloped the sun. Those commonly observed are only the summit of the most elevated, and without doubt it is only in certain favorable circumstances that we can see the sun entirely crowned by them. This corona of light prevented accurate observations of time, and it would give a different solar diameter according to the depth of shade of glass employed.

The time of total obscuration was found by M. Cayetano de Aguilar to be $3^m\ 11^s$, but it passed like a moment and seemed to us, at the most, not more than two minutes.

My convictions upon the nature of that which I saw are that the phenomena were real and that I truly saw the flames in the solar atmosphere and clouds suspended in these flames; it would be impossible to imagine anything else, as for example, that it might be some phenomena of diffraction or refraction.

The clear graduation and distinct mingling of the peach blossom colored light with the white photosphere was of a character so distinct that it can never be mistaken by any phenomena of interference, of refraction, or any illusion whatever. I do not doubt that it really appertains to the sun, and the structure of these suspended clouds tends to strengthen my conviction. As regards the part of the corona more remote and those long bundles of rays, the thing does not appear to me so certain: they have too much the aspect of those seen through the clouds at sunset. Yet it is important to distinguish from these the true corona which was continued much beyond the protuberances.

M. Cepeda, however, who made his observations with an excellent telescope, having a large field, assures me that he saw a bundle of rays, curved and branched like the horns of a stag, at the upper part.

All these observations have been confirmed by photography.

The director, M. Antonio de Aguilar, had brought, for making the solar photographs, the large telescope of Cauchoix, mounted upon a solid cast iron foot, furnished with clock-work. Besides numerous proofs of the entire sun, he took 14 impressions on a larger scale, and 5 of the natural size of the focal image, 23 millimeters in diameter, and which represent all the phases of the phenomena. The examination of these photographs will be made under more favorable circumstances with proper instruments. At present, I will only say that the times of exposure varied from from 3^s to 30^s, that all the images are solarized in the protuberances, but the corona has an intensity differing according to the time. There was not the same intensity throughout but the most vivid parts do not correspond to the protuberances.

We notice also a greater intensity in the chain of protuberances toward the first and the last instant of total occultation. The force of the light of the protuberances is such that one impression is become triple by a momentary jar of the telescope. In this delicate operation M. Monserat, Professor of Chemistry in the University of Valentia, was charged with all photographic operations, and my compeer, P. Vinader, took charge of the regulation of the telescope. This communication has already become so long that I omit ordinary observations, and will only say that the light was strong enough to enable one to distinguish small objects, and to read without difficulty ordinary books, and without

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seeking I saw Jupiter, Venus, and some other luminaries. A portion of this light may have proceeded from the reflection of a thunder cloud, a short distance from and feebly lighted by the sun.

I give the result of the observations made with the thermomultiplier of Melloni by M. Botella, inspector of mines. In general the progress was very regular, as the figures show:

Commencement,	1h.	57m.	Galvanometer,	20·0
	2	11	"	18·3
	2	25	"	15·5
	2	35	"	11·5
	2	58	"	2·0
	3	5	"	1·5
Totality,	3	10	"	0·0
Emergence of the sun,	3	12	"	0·5
	3	20	"	1·0
	3	35	"	12·0
	3	55	"	15·0
	4	16	"	17·5
End,	4	30	"	20·0

A very sensitive declinometer of Jones, observed hourly by M. Mayo, engineer, showed no disturbance. Professor Barreda observed the solar spectrum at my request, and will give his report thereon in a special memoir.

4. *On the polarization of the light of the corona, and of the red protuberances, in total solar eclipses.*—M. Prazmowski observed at Briviesca in Spain, with special reference to these subjects, the total eclipse of July 18th. His observations seem to justify the following conclusions, viz.

(1.) The light of the red protuberances is not polarized. In this respect they resemble clouds in our atmosphere. May we hence conclude that these are solar clouds, composed of particles, not gaseous, but liquid or even solid? The high temperature of the sun leads us to infer that these clouds are constituted of very refractory matter.

(2.) The polarization of the corona proves that its light emanated from the sun and was reflected. The bright, very decided, polarization, proves also that the gaseous particles from which it was reflected send the light to us reflected nearly at the maximum angle of polarization. For a gas this angle is 45°; but in order to reflect light at this angle it must be near the sun. A solar atmosphere seems to furnish the necessary conditions.—*Comptes Rendus, Août* 6, 1860.

5. *Baily's Beads.*—Mr. Lespiault, who watched especially for this phenomenon, says—(Compt. Rend., li, 221)—some seconds before the first interior contact, the margin formed by the arc of the moon appeared irregular and trembling, but I did not see either the "*Baily-Beads*" or "*comb-teeth.*"

6. *Third Comet of* 1860.—A brilliant comet, with a tail several degrees long, was seen by many persons in different parts of our country, on the evening of June 21st and 22d, 1860. It was seen on the evening of June 20th, by Prof. Caswell of Providence, then on the deck of the steamship Arabia. The first public notice of the comet appears to have been made by Mr. C. W. Tuttle, assistant in the Harvard College Observatory. The comet continued vissible to the naked eye about two weeks.

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The following parabolic elements of its orbit were computed by Mr. Tuttle, from observations at Cambridge, Mass., of 21st, 24th and 27th of June.

Perihelion passage,	1860, June 16.06730.
Long. of perihelion,	161° 34′ 56″ } Mean eqx.
" " asc. node,	84 41 20 } Jan. 0.
Inclination,	79 18 11
Log. of perihelion distance,	9.46687
Motion,	*direct.*

Gould's Astron. Jour., No. 136.

7. *The Meteor of July 20th*, 1860.—This remarkable meteor was visible over a portion of the earth's surface at least a thousand miles in length (from N. N. W. to S. S. E.) by seven or eight hundred in width; or from Lake Michigan to the Gulf Stream and from Maine to Virginia. The newspapers have contained many notices of its appearance as seen at various places within these limits, but most of these accounts are too vague to be of any scientific value. We are not yet in possession of a sufficient number of good observations for a final discussion of the phenomena presented, and can only at this time notice briefly a few of the best that have come to hand, and state some approximate results derived from them respecting the height of the meteor above the earth, the direction of its path, &c.

At New Haven, it was seen, during a portion of its flight, by several members of the Scientific Faculty of the College at the house of Prof. J. A. Porter, and pains were at once taken to fix its apparent path by reference to parts of the building, tree-tops, stars, &c., near which it had been seen, and also to determine its time of flight, by noting the time required to repeat the various acts performed while it was in sight. The bearings and altitudes of the points noted for fixing the path were subsequently determined instrumentally. Independent data of the same kind were also obtained by going with many different observers to the places occupied by them at the time, and observing with compass and quadrant the path in the sky pointed out by each, and noting the time for each in the manner already indicated.

By laying down these bearings and altitudes on a globe, a normal or average path was obtained, which cuts the horizon at N. 62° W. and S. 62° E., and gives a maximum altitude of 53°, in a direction S. 28° W.

The time of flight for the different observers, determined as above stated, ranged from 10 to 20 seconds—giving an average of fourteen or fifteen seconds, which agrees with the careful estimate made at the time by the observers at Prof. Porter's.

Valuable observations have also been received from individuals in different places, some items of which we proceed to state. They will be given more fully hereafter.

Mr. J. D. Lawson, of New York, saw the meteor from the corner of Fourth street and Broadway, and has furnished data which give for maximum altitude (N.) 56¼°. Another independent observation at the same spot, as published in the *Journal of Commerce*, gives from data subsequently obtained by Prof. H. A. Newton, an altitude of about 55°. We use for N. Y. 56° as the mean of the two.

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Mr. F. Huidekoper, of Meadville, Pa., makes the altitude at that place 39° 30′ from the northern horizon; the point of disappearance at altitude 3° 30′, and 10° 45′ S. of east; time from crossing meridian till disappearance, 10 to 12 seconds.

Mr. W. King, a surveyor at Erie, Pa., makes the altitude 44°, and point of disappearance in a cloud due east at an altitude of 22°.

Mr. S. B. McMillan, of E. Fairfield, Ohio, reports it as having been seen "moving from a point about 10° E. of N. to within as much of a due east direction," attaining an altitude of 15°.

Rev. T. K. Beecher at Elmira, N. Y., saw it pass very nearly through his zenith, and "so very close to" α Lyræ "as to quench, if not eclipse it." This star was then about 11° from his zenith, and in azimuth S. 76½° E. The meteor separated into two parts with an explosion when near the zenith.

Other observations (not now at hand), which have been used in obtaining our results, have been received from Mr. B. V. Marsh, of Philadelphia, and Prof. Hallowell, of Alexandria.

A comparison of these observations, and a few of the best that have been published, give approximate results as follows:

(1.) The vertical plane in which the meteor moved cuts the earth's surface in a line crossing the northern part of Lake Michigan, passing through, or very near to, Goderich on Lake Huron, (C. W.), Buffalo, Elmira and Sing Sing, N. Y., Greenwich, Conn., and in the same direction across Long Island into the Atlantic.

(2.) In this plane the path that best satisfies the observations is sensibly a straight line approaching nearest to the earth (41 miles) at a point about south of Rhode Island, and having an elevation of 42 miles above Long Island Sound, of 44 over the Hudson, 51 at Elmira, 62 at Buffalo, 85 over Lake Huron, and 120 over Lake Michigan. The western observations, however, which are few and imperfect, seem to indicate a somewhat greater elevation than this for the western part of the path. Possibly, therefore its true form may have been a curve convex towards the earth, resulting from the increasing resistance of the atmosphere as the meteor descended into denser portions of it. The observations made this side of Buffalo, which are somewhat numerous and many of them good, are very well satisfied by the straight path already described. Further and more accurate observations beyond Buffalo are greatly needed for determining the true form and position of the orbit, both in respect to the earth's surface and in space.

(3.) The close approximation to parallelism to the earth's surface of the eastern portion of the observed path leaves it a matter of doubt, considering the imperfection of the observations, whether the meteor finally passed out of the atmosphere and went on its way in a disturbed orbit, or descended gradually into the Atlantic. The former supposition is perhaps the more probable, especially if the path was curved, as above suggested, instead of a straight line.

(4.) The meteor exhibited different appearances in different parts of its course. It seems to have been observed first as a single body, more or less elongated, gradually increasing in brilliancy, throwing off occasionally sparks and flakes of light, until it reached the neighborhood of El-

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mira, N. Y. Here something like an explosion occurred, and the meteor separated into two principal portions with many subordinate fragments all continuing on their course in a line behind each other, and still scattering luminous sparks along their track, until a point was reached about south of Nantucket, when a second considerable explosion took place, and afterwards the principal fragments passed on till lost to view in the distance. The most trustworthy observations represent the meteor as disappearing while yet several degrees above the horizon, (generally from 3° to 6° or 8°). Besides the actual changes of form which the body successively underwent, apparent changes would present themselves to each observer arising from change of direction in which the meteor was seen.

(5.) It is not easy, from the observations in hand, to determine with much accuracy the velocity of the meteor while passing through our atmosphere. The time of flight is doubtless largely overestimated by most observers, especially those unaccustomed to measure intervals of a few seconds. Timing with a watch a repetition of the acts performed during the flight of the meteor, usually reduces the interval to not more than one third, or even one fifth, of the observer's own estimate. From 15 to 30 seconds is a fair range for good observations, and probably to no observer was the meteor in sight over 45 seconds or a minute, although a minute and a half and two minutes are very common estimates. A comparison of the most probable estimates of time with the length of path observed, gives a velocity ranging from eight to fifteen miles a second. Probably 12 or 13 miles is a tolerable approximation. This, allowing for the earth's motion in its orbit, gives 26 or 27 miles a second as the actual velocity of the meteor in space. Its relative velocity may have been much greater when just entering the atmosphere, than after encountering its accumulated resistance.

(6.) The actual diameter of the luminous mass, taking its apparent diameter as nearly equal to that of the moon, (the estimate of many observers nearest its track) must have been from one-fifth to one-third of a mile. Many estimates would make it still larger. The two principal heads when passing New Haven must have been from one to three miles apart.

(7.) A report is mentioned by many observers as having been heard from one and a half to five minutes after the meteor passed. The least time in which such a report could have been heard, taking the usual constant for the velocity of sound (1090·47 feet a second) would be about three minutes and a half. This is a point of much interest, and needs to be investigated.

The "rushing sound" spoken of by many as heard while the meteor was passing, is of course to be attributed to imagination.

C. S. Lyman.

8. *The Meteors of August 2d and 6th*, 1860.—A meteor, rivaling in brilliancy that of July 20th, was extensively observed throughout the Southern United States on the evening of August 2d, between 10 and 11 o'clock, according to the local time. It appears to have passed from east to west vertically over Tennessee at ten minutes past ten, Knoxville time. "From three to five minutes after the disappearance of the meteor a report was heard like the discharge of an eight-pounder; which was fol-

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lowed by a long—*long* rolling, reverberatory sound of more than a minute's duration." This fact is mentioned by Mr. W. C. Kane writing from Knoxville to a friend in Hartford, Conn.

Another brilliant meteor was seen in the southwest, from New Haven and New York, between half past seven and eight o'clock, on the evening of August 6th. It passed from south to north, and notwithstanding the daylight still remaining attracted attention over a wide extent of country.

9. *Further Notice of the New Concord (Ohio) Meteor, of May* 1, 1860; by Prof. E. W. Evans.—Since writing my communication published in the July number of this Journal, on the path and height of the New Concord meteor, I have found some additional data, which I regard as important because they have been furnished by a good observer who saw the meteor under favorable circumstances. A single case of this kind is the more worthy of note because, owing to the cloudiness of the day when this meteor passed, there were but a few places from which it was seen at all. The observer referred to is D. Mackley, Esq., a lawyer of Jackson, Ohio, who at the time of the occurrence happened to be at Berlin, about six miles north east from the former place, and seventy miles from the nearest point under the meteor's path. He took pains to note all the facts as accurately as he could at the time; and he afterwards returned to the spot in order to determine more definitely the points of the compass. His testimony, in answer to my interrogatories, is substantially as follows:—

"The meteor first appeared to me at a point about 55° east of north. It moved northward in a line very nearly parallel with the horizon. When it disappeared it had described an arc of about 15°. It was in sight about 6 seconds. Its altitude was about 30°. In regard to its size, I have since looked at the sun through a thin cloud, and I think the apparent diameter of the meteor was one-half that of the sun."

These data give the meteor a height of 41 miles over the northern boundary of Noble county; a diameter of three-eighths of a mile; and a relative velocity of nearly 4 miles a second. The results agree sufficiently well with those before given.

The meteor was seen through openings in the clouds at various points along a line of 60 miles, extending from near Newport on the Ohio river to the neighborhood of New Concord. The evidence, upon the whole, does not indicate any descent of the body towards the earth between these limits, or any change in its size or appearance. From this fact, and the great height of the body, and the absence of all evidence that it was seen or heard in the northern part of the State or beyond, it seems probable that this meteor was not dissipated in the atmosphere, but passed out of it again. The shower of stones which came down near New Concord had probably been detached from the principal mass before the latter came into sight.

Marietta, Ohio, Aug. 20, 1860.

10. *Shooting Stars of August* 9–10, 1860.—Since the year 1837, at least, it has been found in the Northern hemisphere, whenever the weather has permitted observation, that shooting stars have been unusually abundant during a period of several nights in August, gradually increasing in numbers for a few days up to the 10th of the month,

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and then gradually diminishing in frequency. While every other meteoric period has intermitted, this of August holds out with little change.

The observations below stated were made in the open air, from the top of the southern tower of the Alumni Building of Yale College, by a corps of observers consisting of J. W. Gibbs, Jr., Charles Tomlinson, J. W. Johnson, W. C. Johnston, H. W. Siglar, J. B. Chase, with myself. During the hour from 2^h to 3^h A. M., there were but six observers. Each meteor was called aloud, and no one was reckoned twice. Between 10 P. M. of the 9th and 3 A. M. of the 10th of August, 1860, we observed *five hundred and sixty-five* different shooting stars, distributed as follows, viz:

Aug. 9.	10^h to 11^h P. M.	N. E.	29	11^h to 12^h P. M.	N. E.	14
		S. E.	30		S. E.	22
		S. W.	35		S. W.	47
		N. W.	11		N. W.	18
			105			101
Aug. 10.	0^h to 1^h A. M.	N. E.	19	1^h to 2^h A. M.	N. E.	24
		S. E.	27		S. E.	25
		S. W.	44		S. W.	31
		N. W.	17		N. W.	17
			107			97
	2^h to 3^h A. M.	N. E.	30			
		S. E.	20			
		S. W.	65			
		N. W.	40			
			155			

The meteors were reckoned in that quarter where they first became visible, but it is of course impossible to indicate satisfactorily by words their distribution on the face of the sky. The majority of them were inferior in brilliancy to stars of the second magnitude. Many however were as bright as stars of the first magnitude, and a few rivalled in splendor Mars and Venus. Several of them left luminous trains, but none appeared to explode. Their general Southwesterly direction was evident, and as nearly as we could estimate, at least three fourths of all conformed to the radiant of former years, in the vicinity of the sword-handle of Perseus.

During our watch the sky was clear except that between 1^h and 2^h A. M. there were some clouds about the west. The moon in her last quarter embarrassed our view after 11 P. M., and doubtless concealed about one third of all the meteors which might have been seen in her absence. At 3 A. M. mist was forming rapidly, but we saw after that hour *five* meteors not included above; and while we were arranging for the watch a few minutes previous to 10^h we saw *eighteen*, making a total of 588 meteors observed by us during five hours and ten minutes. Had the moon been absent we should probably have seen 800 during this period. This is not far from six times the common nightly average for a like interval.

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It is perhaps hardly worth mentioning that the Aurora Borealis was visible all night, being by turns quite active after twelve o'clock. This phenomenon has been seen here with uncommon frequency this summer, but it appears to have no special connection with shooting stars.

During the entire nights of the 8th, 10th, 11th, 12th and 13th of this month, our sky was wholly overcast. EDW'D. C. HERRICK.

Yale College, New Haven, August, 1860.

V. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *The Fourteenth Meeting of the American Association for the Advancement of Science*, was held, August 1st–8th, 1860, at Newport, R. Island, in the old State House, dating from the Colonial times under Queen Anne. The Hon. W. H. Cranston, Mayor of Newport, pronounced an address of welcome from the city of Newport, to the members of the Association. The general enjoyment of the occasion was amply secured by the kind and abundant hospitality of the city and its citizens, leaving on all who participated the most agreeable impressions of Newport. The weather was superb, neither fog or rain marred the pleasure of the unequalled drives by beach and shore over this American Isle of Wight, and the Geologists with Mr. Hitchcock's map* of the Island in hand found the conglomerates of 'Purgatory' and 'Paradise' and the graphitic anthracite of these most overdone and contorted coal measures a fruitful theme of discussion both in hall and field.

The numbers in attendance at the Newport meeting were small compared with most previous meetings. An erroneous impression appears unfortunately to have gone abroad that Newport was a very costly place to visit and that the 'height of the season' at a great watering place was not a time when science could hold ground against fashion. While all this was eminently untrue, the effect of such an impression was visible in the absence of numbers of familiar faces.

Neither can we, if we would, conceal the fact that while many papers of marked ability were presented, the character of this meeting was not in all respects creditable to American Science. A conviction prevailed among many who were present at Newport of a decadence in the scientific character of the Association, of a loss of tone which if not already a demoralization threatened soon to become such. There was a time when weak speculations—by whomever put forth—were promptly trampled on and their authors made to feel that they were answerable to a power of united public sentiment which bore rule with a righteous severity. The evidence of this power was wanting at Newport—signs of concession to scientific charlatanism were visible in quarters where we least looked for it, and we left the meeting with a persuasion that unless the old order of proceedings could be restored, the American Association must come to a speedy and disastrous end. We do not despair of the immediate restoration of a higher standard—the effort will certainly be made.

One of the bright points of the Newport meeting was the opportune return of the Labrador Astronomical Expedition, sent out under the auspices of the United States Coast Survey and under the immediate sci-

* Furnished gratuitously to all members by the City Government.

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entific leadership of Prof. Stephen Alexander of Princeton, to witness the total Eclipse of July 18th. As we elsewhere give a detailed statement of the results from the pen of one of the expedition, it is only necessary to add that the "*Bibb*" made Newport harbor on her return in season to secure an agreeable reunion on Tuesday evening, and that on Wednesday morning Prof. Alexander recounted the results of the Expedition to that land of 'monumental desolation' in a brief but fervid recital, deeply moving his audience by his peculiar eloquence. It was a happy circumstance that the meeting could close under so agreeable an impression.

The address of the retiring President, Prof. Stephen Alexander, was delivered on Wednesday morning, just before the adjournment, and will appear in the Transactions of the year.

Prof. Bache gave an interesting public discourse reviewing the history of discovery, and the method of investigation employed in regard to the *Gulf Stream*. This address we hope to present to our readers in full in a future number of this Journal, as also the substance of another public address by Prof. Henry, on Atmospheric Electricity, delivered as a popular evening lecture at Newport, by the distinguished Secretary of the Smithsonian Institution.

List of papers presented to the Association.

Section A.—Mathematics, Physics and Chemistry.

General Account of the Results of Part II of the Discussion of the Declinometer Observations made at the Girard College, Philadelphia, between the years 1840 and 1845, with special reference to the Solar Diurnal Variation and its Annual Inequality; by A. D. Bache.

On some of the Relations of the Violet and Green Modifications of Chrome Alum to Soda, Potassa and Ammonia; by E. N. Horsford.

Reflections on the Origin, Development and Changes of Languages; by Henry M. Harman.

The Great Auroral Display of August 28 and September 2, 1859; by Elias Loomis.

Brief Abstract of a Memoir on the Theoretical Determination of the Dimensions of Donati's Comet; by W. A. Norton.

On the Solution of Ice in Inland Waters; by B. F. Harrison.

On Natural Ice-Houses and on Frozen Wells; by Elias Loomis.

Description of a new Registering Thermometer; by James Lewis, of Mohawk, N. Y. Presented by W. B. Rogers.

On the Phenomena presented by the "Silver Spring," in Marion County, Florida; by John LeConte.

Influence of difference in the mean velocity of winds from the different points of the compass, in modifying the mean direction of the atmospheric currents over the United States; by James H. Claflin.

Can the Sudden Cooling of one Part of a Metallic Rod cause another Part, as a consequence, suddenly to become warm? by E. N. Horsford.

Improvement in Barometers; by H. A. Clum.

General Account of the Results of Part III of the Discussion of the Declinometer Observations made at Girard College, Philadelphia, between the years 1840 and 1845, with special reference to the Investigation of the Influence of the Moon on the Magnetic Declination; by A. D. Bache.

Some experiments and inferences in regard to Binocular Vision; by W. B. Rogers.

On the data and methods of the Hindu Astronomy; by W. D. Whitney.

On Atmospherical Electricity; by Joseph Henry.

Note on Sources of Error in the employment of Picric Acid to detect the Presence of Potash; by M. C. Lea. Presented by B. Silliman, Jr.

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On a series of new combinations of Ammonia Picric Acid and Metallic Bases; by M. C. Lea. Presented by B. Silliman, Jr.

On the Combustion of Wet Fuel; by B. Silliman, Jr.

Theory connected with the Solar Spots; by C. W. Hackley.

Description of a new Portable Coffer Dam for Foundations under water; by E. B. Hunt.

On certain Variable Stars; by B. A. Gould.

Modern Warfare; its science and art; by E. B. Hunt.

Abstract of the principal results of the observations of the Tides at Van Rensselaer harbor, made by the second Grinnell Expedition under command of Dr. E. K. Kane, U. S. Navy, during 1853, '54, '55, from a reduction and discussion by Charles A. Schott, Assistant Coast Survey. Presented by A. D. Bache.

On Catachroism, a new optical property belonging to certain Crystalline Surfaces, with some remarks on Polarization, by reflection from colored surfaces; by M. C. Lea. Presented by B. Silliman, Jr.

On a new theory of Light, proposed by J. Smith, of Manchester, England; by O. N. Rood.

Some reflections on the Observations of the Solar Spots and of the Magnetic Variations; by James Hyatt.

On the Actinism of the electric discharge in vacuum tubes; by W. B. Rogers.

Investigation of the problem regarding the existence of a lunar tidal wave on the great fresh-water lakes of North America; by J. D. Graham.

On the Hydrates of Sesquioxyd of Chromium; by E. N. Horsford.

On a series of investigations, on the assimilation of gaseous nitrogen of plants, conducted during the year 1857, 1858 and 1859, at Rothamsted, England; by E. Pugh.

Vital Statistics of the Blind, with an approximate Life Table; by E. B. Elliott.

Influence of difference in the mean velocity of winds from the different points of the compass, in modifying the mean direction of the atmospheric currents over the United States—discussed by the direction and at the expense of the Smithsonian Institution, from materials collected in its archives for the years 1854, 1855, 1856 and 1857; by James H. Coffin.

An attempt to estimate the Height, Velocity, &c., of the Meteors of the evening of July 20, 1860; by H. M. Harman.

On the meteors of 1859, Aug. 11, and 1860, July 20; by B. A. Gould.

On the Induction-Time in Electro Magnets; by A. D. Bache and J. E. Hilgard.

The loss by fire of Berdan's Mechanical Bakery in Boston, due to Spontaneous Combustion; by E. N. Horsford.

Observations on Hydraulic Cements; by Q. A. Gilmore. Presented by E. B. Hunt.

On the Motions of Uranus; by Truman Henry Safford.

Fire Damp Explosions in Collieries by lighting with coal gas; by E. B. Hunt.

The Meteor of July 20th, as seen from Washington, Dutchess County, New York; by James Hyatt.

Some remarks on the Open Sea of the Arctic Regions; by W. W. Wheildon.

On the Paradoxes of the Atomic Theory of Chemistry, with a proposition of a new Hypothesis; by Clinton Roosevelt.

On the production of Ethylamine by reaction of the Oxy-Ethers; by M. C. Lea. Presented by B. Silliman, Jr.

Abstract of the principal results of the astronomical observations at Van Rensselaer harbor and other places near the northwest coast of Greenland, made by the second Grinnell Expedition, under command of Dr. E. K. Kane, U. S. Navy, during 1853, '54, '55, from a reduction and discussion by Charles A. Schott, Assistant Coast Survey. Presented by A. D. Bache.

A new Ammonia Chrome Alum, and the Violet, Green and Red Modifications of Chrome Salt; by E. N. Horsford.

On the Time Necessary to Double the Pressure of Steam under certain circumstances; and the practical lessons therefrom; by James Hyatt.

Systematic Views on Mechanics and Mechanism, with inferences on Organisms; by E. B. Hunt.

On the nitric acid and ammonia in rain water, collected Sept., 1859, between New York and Liverpool about the middle line of the Atlantic Ocean; by E. Pugh.

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On the Relations of Salts of Zinc and Alumina to Soda and Potassa; by E. N. Horsford.

On our inability through the retinal impression alone, to determine which retina is impressed; by W. B. Rogers.

The Theory of Probabilities applied to determine the Identity of Words and Languages; by H. M. Harman.

On the possibility of expressing the polar co-ordinates of the Asteroids by converging series, admitting of tabulation; by G. W. Coakley.

Analysis of a Bituminous Earth from Brazil; by E. N. Horsford.

Section B—Natural History and Geology.

Remarks upon certain points in Ichnology; by Edward Hitchcock.

Synchronism of Coal Beds in the Rhode Island and Western United States Coal Basins; by C. H. Hitchcock.

Geology of Newport and Vicinity; by C. H. Hitchcock.

Additional facts respecting the Olathropteris of East Hampton; by Edward Hitchcock.

Description of brecciated trachytic Dykes in Shelburne, Vt., with special reference to their temperature, when formed; by Edward Hitchcock.

Upon a Diatomaceous Earth from Nottingham, Calvert Co., Md.; by C. Johnson of Baltimore.

Jottings on the Geology of the Eastern part of Maine, &c.; by W. B. Rogers.

On the recent discovery by Mr. Norman Eastop, of Fossils in the conglomerate of Taunton River; by W. B. Rogers.

On the origin and stratigraphical relations of the Trappean rocks of Lake Superior; by J. W. Foster and J. D. Whitney.

On the Lead Region of the Upper Mississippi; by J. D. Whitney.

On the arrangement of the Museum of Comparative Zoology at Cambridge; by L. Agassiz.

On the origin and distribution of the Sediments composing the stratified rocks of North America; by J. S. Newberry.

On the Surface Geology of Western America; by J. S. Newberry.

Some points in the Surface Geology of the Northwest; by J. D. Whitney.

Remarks on the distribution of Gold in Veins, with notes upon remarkable Gold Specimens from Georgia; by Wm. P. Blake.

On the Ethnological Value of the imitative faculty in relation to the characteristics of ancient and modern American Races; by Daniel Wilson.

On the Petroleum Wells of the Mississippi Valley; by J. S. Newberry.

Description of a new species of Trilobite of the Genus *Conocephalites*, from the Potsdam Sandstone; by F. H. Bradley, with notes by E. Billings. Presented by B. Silliman, jr.

On Certain Phenomena of the Great Dismal Swamp in Virginia and North Carolina; by Nathan B. Webster.

On methods in Zoology; by Louis Agassiz.

On the American Reindeer; by L. E. Chittenden.

On the food of Birds, with special results in the investigation of the food of the Robin, (*Turdus migratorius*); by J. W. P. Jenks.

On the later extinct Floras of North America; by J. S. Newberry.

Age of the so-called Taconic Rocks in Vermont; by C. H. Hitchcock.

On the origin of the Prairies of the Northwest; by J. D. Whitney.

In obedience to a pledge given in 1859, that the next meeting should be held in some Southern city, the Association adjourned to meet in Nashville, Tennessee, in April, 1861, the exact date to be fixed by the local Committee.

The Officers of the Nashville Meeting are, President, F. A. P. Barnard, LL.D., President of the University of Mississippi; Vice President, Dr. Robt. W. Gibbes, of South Carolina; General Secretary, Prof. J. W. Mallet, of Mississippi; Secretary, Dr. A. L. Elwyn, of Philadelphia.

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2. *Reclamation by Gov. Stevens of Washington Territory.*—Prof. B. SILLIMAN, Jr.—*Sir* :—I send you herewith a copy of my final Report and Narrative of the Exploration of the Northern route for a Pacific Railroad, and desire respectfully to ask you to examine it in connection with a volume entitled Natural History of Washington Territory, by Drs. Cooper and Suckley, also sent with this.

It is due to one of the authors (Dr. Suckley) that I should here remark that when he was informed of the injustice done me, he frankly admitted that I had a right to be dissatisfied and that nearly all the facts complained of occurred during his absence in California—that when he returned he found the title pages and the chapter on Meteorology had been printed under the direction of Dr. Cooper, my friend and neighbor, and that he supposed, from Dr. Cooper's relationship to me, that my permission had been given to the incorporation of the chapter on Meteorology. He also added that as an *amende honorable* he should send notices over his signature to every holder of the book and also one to be published in your Journal, stating that my name was inadvertently left out of the title page and that I was the *sole* author of the chapter on Meteorology. Therefore wherever Dr. Suckley's name is referred to, it is simply because he appears as joint author of the "Natural History," and not because I attach any blame to him in regard to the matter.

An examination of my report will show you that this "Natural History" is a portion of my own Report. I will first call your attention to their letter addressed to me transmitting their volume. You will find it at the close of the volume (Appendix C.). In that letter, as will appear from its contents, they transmit the portion of my Report immediately following the alphabetical Index and preceding the appendices.

If you will examine the "Natural History" you will find that it contains Chapter IV. of my Report on Meteorology, copied page for page; that this chapter is illustrated by several views taken from the body of my Report, and that my isothermal chart is to be found at the end of the volume. By turning to my own Report, it will be observed that this chapter on Meteorology is an integral portion of my own personal and official Report of the route, follows the geographical memoir and precedes the estimate of the cost of a railroad on the northern route.

You will also observe upon page viii. of the preface to the "Natural History" that the authors (Drs. Cooper and Suckley) state that none of the plates illustrating their volume have been before published in any of the series. If you will compare these pictorial views with those in my final Report and narrative, you will find them to be identically the same, having been struck off from the same stone, by the same person, and therefore cannot be new.

My object in addressing you is to expose the plagiarism of Dr. Cooper and to show his injustice to myself.

It being doubtful whether extra copies would be ordered by Congress, I gave my consent to the Government printer's striking off some extra copies of the Natural History Report for the use of Drs. Cooper and Suckley, they bearing the expense, and the pretended edition of Baillière Brothers was struck off in this manner. I did not give my consent to the incorporation of the chapter on Meteorology, or to the use of the

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plates, or to the use of the isothermal chart. It was done without authority, without the least consultation with me, nor was I aware till I saw the bound volume that they had any intention of the kind.

Now turn to their title page. 1. My name is altogether omitted from it, whereas since the work is simply a portion of my own report, my name should have been there. 2. The title states that Drs. Cooper and Suckley are the authors of the work, and of course the authors of Chapter IV. The following extract from the title makes this claim:

"Being those parts of the final Reports on the Survey of the Northern Pacific Railroad route, containing the Climate and Physical Geography, with full catalogues and descriptions, &c., by J. G. Cooper, M. D., and Dr. G. Suckley, U. S. A., Naturalists to the Expedition." 3. The title shows new matter added, which, under the arrangement referred to, was certainly inadmissable.

I will now call your attention to Baillière's advertisement which I send you with the "Natural History." It is there set forth that Chapter IV, on Meteorology, is by Gov. Stevens and Dr. Cooper.

By reference to the date of my letter to the Secretary of War, transmitting my Report, and the date of Drs. Cooper and Suckley's preface, it will appear that on the 7th of February, 1859, this chapter was sent in to the Secretary as my exclusive work and was so published by the Senate. In November following it is claimed by Drs. Cooper and Suckley in their title page as their work, and in Baillière's notice as the joint work of Gov. Stevens and Dr. Cooper. The priority of the Senate publication establishes the plagiarism.

The authorship of the chapter in question is exclusively my own. Dr. Cooper has no pretext to any portion of the authorship. He was employed by me under the authority of the Department, at the rate of $125 per month, to assist me in my Report. He did assist me on this chapter and in other portions of my Report. I prescribed the mode of investigation, supervised the work daily and prepared the Report. The mode of investigation, reasoning, deductions and conclusions are my own and not Dr. Cooper's. My attention had long been given to the subject. The Doctor was patient under my direction in preparing tables and collecting data. It is not only a most extraordinary case for an employé to assume the authorship of an article when he simply did the work of an employé, but it has been long since established judicially that all such claims are utterly untenable.

Why did not Dr. Cooper claim the authorship when I sent in the Report to the Secretary? Why did he not claim it when he was employed by me in assisting me in the tables to be found in it? He was during this whole time and for many months subsequently at my house every day. Why did he not claim the authorship when the Senate Report was published? It was published before the date (November) of their preface.

Why has it been done clandestinely, my first notice of it being seven months after the Senate publication?

I had a right to expect better treatment at the hands of Dr. Cooper than this, since at the time he was contemplating claiming authorship to an article of which he knew I was the author, he was pretending friend-

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ship towards myself, and afterwards did seek and obtain through my influence a position with a Government expedition.

I have respectfully to request that you will publish this communication in your valuable Journal. I am, Sir, very respectfully,

ISAAC I. STEVENS.

3. *Dr. Suckley's Disclaimer.*—Editors Silliman's Journal:—Gentlemen:—You will confer a favor by stating in the next number of your valuable Journal that the authorship of the Chapter on Meteorology in Cooper and Suckley's work on the Natural History of Washington Territory should be accorded to the Hon. Isaac I. Stevens, whose name was unfortunately omitted from the title-page. Notices to this effect have been sent to all the owners of copies.

Very respectfully, your ob't servant, GEORGE SUCKLEY.

New York, July 31, 1860.

4. *Stereoscopic Advertisements;* by ELI W. BLAKE, JR.—Prof. H. W. Dove, to whom we are indebted for so many beautiful stereoscopic experiments, in his *Optische Studien*,* gives a specimen of stereoscopic printing to illustrate the double refraction of Iceland Spar, as seen in *binocular vision.*

This effect is produced by printing for the left eye, lines in the ordinary manner, while for the right eye, the alternate lines are slightly advanced. Upon combining the two by means of the stereoscope, the printing appears to be in two planes, more or less distant from each other.

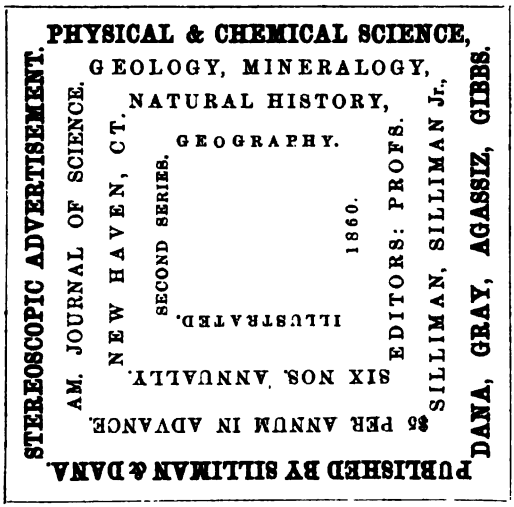
Should the "spacing" of the two not correspond *exactly*, single words will rise towards or fall back from the eye; thus by varying the spaces between the words even in the slightest degree, a marked effect is produced. In this age of advertising it might be worth the while of enterprising individuals to print their advertisements in a stereoscopic form. It would certainly gain for them a more general and careful study than this class of literature can generally command. I annex, as an example of this mode of printing, a stereoscopic advertisement of this Journal, which may be observed by simply placing the instrument over it.

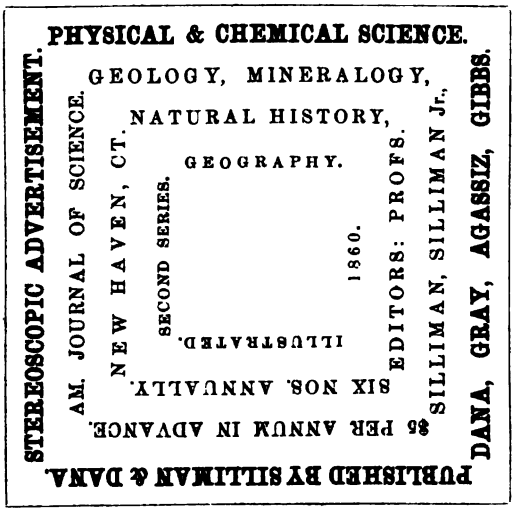
5. *Paraselena and Lunar Rainbow;* by Lieut. J. M. GILLISS.—When returning from Washington Territory and at 9 P.M. of July 26th, we were witnesses to atmospheric phenomena so rarely seen together that a brief notice is offered for record in the Journal.

The steamer's position was lat. 44° N.; long. 124° 30′ W., and, consequently, some 20 miles N.W. of Umpqua river, in California. The stars shone brightly overhead and the atmosphere was perfectly calm, but loaded with a wet fog or mist. At the time specified, the moon was 8¼ days old and its altitude about 20°. Its disc was enveloped in bright mist, which extended in a well-defined circle of 30′ to 32′ diameter. This mist-ring was surrounded by an exquisitely marked halo of about 3° diameter, whose colors were blue, pearl-white and reddish-pink, the blue color nearest to the moon. The halo lasted from 15^m to 20^m, its colors gradually fading, until they were no longer distinguishable, though the bright inner mist-ring continued much longer. When it had disappeared stars were visible to within 15° of the horizon.

* Optische Studien von H. W. Dove. Berlin, 1859.

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Prior to the formation of the halo, and for some time after, we ceased to see it, there was a perfectly formed fog or mist bow to the north of the ship. At times, a slight tinge of dirty red could be traced on its outer border and near the sea, though the predominant color was a murky white.

At 9^h 30^m a faint secondary bow was formed of 3° lesser radius, but it continued only a few minutes.

6. *Oil Wells of Pennsylvania and Ohio.*—A strong impulse has been given to the explorations for petroleum by the success of the well at Titusville, Pennsylvania, worked for some years past by the Pennsylvania Rock Oil Company, on Oil Creek. In a circuit of five miles from Titusville, there were early in July, over 400 wells in progress, and 100 at work, raising from ten to fifty barrels daily from depths varying from 40 to 300 feet. The crude oil is dichromatic, having by transmitted light a dark brown color, which by reflected light is greenish or blueish. Its density is 0·882. It is even in warm weather rather thick, and in cold weather is more stiff, but even at −15° is still fluid. Its odor is strong and peculiar from the Pennsylvania wells—but from the wells at Mecca, Ohio, it is nearly odorless. It boils at a very high temperature, but begins to distill a thin colorless oil, even at 212° F. By fractional distillation, I obtained from 304 grams of crude oil of Titusville:—

	Temperature.	Quantity.	Density.
1st Product	at 100°C. = 212°F. (acid water,)	5 Gms.	
2d "	" 140°C. to 150°C.=284° to 302°F.	26 "	·733
3d "	" 150°C. to 160°C.=302° to 320°F.	29 "	·752
4th "	" 160°C. to 170°C.=320° to 338°F.	38 "	·766
5th "	" 170°C. to 180°C.=338° to 356°F.	17 "	·776
6th "	" 180°C. to 200°C.=356° to 392°F.	16 "	·800
7th "	" 200°C. to 220°C.=392° to 428°F.	17 "	·848
8th "	" 220°C. to 270°C.=428° to 518°F.	12 "	·854

The *boiling points* of these several fluids present some anomalies, but are usually progressive, thus,

No. 2. common boiling at	115°C.=239°F.	remained constant at 228°C.=442°F.
3. "	" 120°C.=248°F.	" " 270°C.=518°F.
4. "	" 140°C.=284°F.	" " 290°C.=554°F.
5. "	" 160°C.=320°F.	and still rising at 308°C.=581°F.
6. "	" 135° C.=275°F.	rose rapidly above the range of mercurial thermometer.
7. "	" 135°C.=275°F.	and still rising at 305°C.=305°F.

The first five products remained entirely fluid at the low temperature of −15° F.

At the town of Mecca, Trumbull County, Ohio, many Oil Wells have lately been bored. We learn from Dr. J. S. Newberry, who has lately visited them, that only two were as yet considerably productive. One at 'Powers Corners,' which yields three or four barrels of crude oil, daily,—and the second has rewarded the industry of two poor Germans by the unexpected yield of twenty-five gallons hourly, or from twelve to sixteen barrels daily. Over fifty wells were already in progress. Many of them yielding oil from the sand near the surface, and all easily bored—often for a well to the depth of fifty feet at a cost not above fifty dollars—without the pumping apparatus and lining. Oil springs have been known in both

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these districts for over fifty years, and were regarded by the agricultural population with disgust, as spoiling their water—until they found them an unexpected source of wealth. Both these districts are within the coal region—but the Pennsylvania locality, at least, is below the coal-bearing rocks.—s.

7. *Artesian Well at Columbus, Ohio,—rate of increase in temperature at 2,575 feet depth.*—In a letter to the Editors from T. E. Wormley, Esq. dated Columbus, Ohio, August 6th, 1860. Dear Sirs:—I herewith send you the temperature of the artesian well in our city, at a depth of 2,575 feet.

A few days since a Walferdin's Thermometer, placed in a glass tube filled with water, and this enclosed in a strong iron case, also filled with water, was lowered to a depth of 2,475 feet, where it remained for twenty-five hours, it was then sunk to the bottom of the well, a depth of 2,575 feet, where it remained for forty minutes. Upon the withdrawal of the instruments, it was found to have registered 88° F. Assuming this to be the temperature at the bottom of the well, and also assuming as correct data, that the temperature is uniformly 53° F. at a depth of 90 feet, we have an increase of 1° F. for every seventy-one feet.

8. *Salt Wells in Michigan.*—At *Saginaw*, Michigan, a well 669 feet deep has been sunk, passing through the coal measures and reaching about to the top of the Devonian. Its water is half saturated with salt and has the temperature at surface of 54° F.

At *Grand Rapids*, Kent County, Michigan, on the Grand River, the salt well is 661 feet deep—but as it begins 445 feet lower down in the section than the Saginaw well, the whole section presented by the two is over 1,100 feet. Careful sections of all the beds passed through have been preserved by Mr. George A. Lathrop, our informant, who will communicate them to the State Geologist.

It would be interesting to record the bottom temperature of all artesian wells, by a Walferdin's thermometer, and we trust our correspondents will take care to send us such results.

Book Notices—

1. *A Treatise on Elementary and Higher Algebra;* by Theodore Strong, LL. D., Prof. of Math. and Nat. Philosophy in Rutgers College, etc. New York. Pratt, Oakley & Co. 1859.—This work does not belong to the class of elementary compilations with which our schools and colleges are so abundantly supplied, but is a fresh and original treatise, stamped on every page with the peculiar impress of the author's mind. It contains moreover several positive additions to the Science while many of the demonstrations of well known propositions are new and highly suggestive. The leading idea of the author appears to have been to set forth the theory of the resolution of numerical equations in a new manner; and accordingly this subject is entered upon at as early a stage of the book as possible. Thus he begins its discussion under the head of *Multiplication*, continues it successively under the heads of *Division*, *Geometrical Progressions*, and *Undetermined Coefficients*, and finally completes it in two chapters, near the close of the volume, on the "Limits of the Real Roots of an Equation of the *n*th Degree,"—and the "Development of the Real and Imaginary Roots of an Equation of the *n*th Degree." This distribution of the subject, however, has not conduced to clearness, while it has given the work a character which renders the word

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"Elementary," introduced into the title, quite inappropriate. We should not have thought it necessary to make this remark had not the author in his preface declared that the work was intended "to give a full view of the present state of the science," and also to be adapted "to the wants of teachers and pupils not only in our Colleges and Academies, but *in schools of a primary character.*" Dr. Strong has risen to such heights in the science and has so long been an industrious explorer on its confines, that he has lost sight of the paths by which the novice must necessarily be introduced. But assuming the reader to have acquired a fair knowledge of algebra, including the general theory of equations, we would recommend him to read this work as a *review* in which the science is rearranged in a special order, and a new light is thereby thrown upon many of its processes. For Dr. Strong is a master of his subject and handles the most difficult parts of it with something of that kind of giant ease which we feel and admire so much in the great Euler.

From this statement, the reader will readily understand that a minute review of this work, pointing out all that is peculiar in methods, or in arrangement of methods, would be a profitless and superfluous labor. We will make but a single remark upon that part of the book which is prominently set forth as new and important both by the author and his reviewers—namely: the Solution of Cubic Equations "by pure algebra." The author's solution is certainly new, (at least new to us,) and extremely ingenious; but we can hardly concede that it is the only solution "by pure algebra," as appears to be claimed. For if the term "pure algebra" is used to distinguish the solution from that which is obtained by trigonometry, then we have other methods of solving the "irreducible case of Cardan's rule" by "pure algebra," quite as complete on this score as that of Dr. Strong. He obtains the roots only by a series of successive approximations, and does not present them in the form of a finite algebraic formula. Horner's method of solving equations appears to us to meet the case as well as Dr. Strong's, both being based upon successive approximation, and in practice Horner's method will generally be far more expeditious. Again, the roots may be developed in converging series (as was done by Nicole as long ago as 1738) and thus obtained by "pure algebra." As compared with the method by series, Dr. Strong's solution possesses the decided advantage that it *always* approximates with tolerable rapidity, the rate of approximation being nearly the same in every case; while the series of Nicole converge with more or less rapidity according to the relation existing between the coefficients of the given equation, and in certain cases the convergence is so slow as to render the series *practically* useless. For example, the terms of the series which express the roots of the equation $x^3 - 23x = 30$, will depend upon the successive powers of the fraction $\frac{24300}{24389}$, which differs so little from units, that one thousand terms of the series would barely suffice to determine the first decimal place of the root, whereas some ten or twelve approximations according to Dr. Strong's method will determine the roots accurately to six decimal places. This method, therefore, may be justly claimed as an addition to the science, as well as the solution of Binomial Equations, the new demonstration of the Binomial Theorem, the development of the roots of equations, series, etc., all of which will be read with pleasure by advanced students.

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2. *Contributions to the Palæontology of Iowa, being descriptions of new species of Crinoidea and other fossils,*—(supplement to vol. i, part ii, of the Geological Repository of Iowa); by JAMES HALL.—Eight sheets of an early copy of these contributions have reached us from the Author in advance of publication. As we hope soon to receive a complete copy, we reserve a list of its contents for a second notice.

PERSONAL.—Prof. DANA returned to the United States early in August, improved in health by his European visit, but not yet in a state to permit the resumption of his accustomed labors. LADY FRANKLIN was passenger on the same ship.

Prof. C. U. SHEPARD, and Prof. ALEXIS CASWELL of Brown University are among the American tourists in Europe this season.

Prof. ELIAS LOOMIS, LL.D., late of New York University, has been elected to the chair of Natural Philosophy and Astronomy in Yale College made vacant by the death of Prof. D. Olmsted. Prof. Loomis is now in Europe for the purpose of making additions to the physical cabinet of Yale College.

JOSEPH E. SHEFFIELD, Esq., a citizen of New Haven, distinguished for his enlightened liberality, has at his sole charge, prepared a large and commodious building for the uses of the YALE SCIENTIFIC SCHOOL, embracing ample apartments for the Engineering, Mechanical and Physical departments—a perfectly appointed chemical Laboratory fitted for thirty special students, and private laboratories for the Professors—a Metallurgical Laboratory—Lecture rooms, both private and public—and two museum rooms—forming altogether a scientific Establishment, second to none in this country. It will be ready for occupation at the opening of the fall term.

TRANSACTIONS OF THE AM. PHILOS. SOCIETY, Phila., vol. xi, new series. Part III, 1860.—Art. xvi, p. 187–258, Revision of Buprestidæ of the U. S., with a plate. By *John L. LeConte, M.D.*—xvii, p. 259–402, Analytic Orthography; an Investigation of the sources of the voice and their Alphabetic notation. By *Prof. S. S. Haldemann.*

PROCEEDINGS OF AM. PHIL. SOC. PHILAD., 1860, January to June.—p. 174, Deaths of members announced—p. 175, Phosphoresence of the Diamond; *Dr. Emerson.*—p. 176, Gale of July 9th and 10th, 1860—Effects upon Philadelphia Gas Works; *Prof. Cresson.*—p. 177, Sanscrit and English analogues; *Pliny E. Chase.*—Geology of the Arctic Archipelago drawn from Mc Klintock's narrative, *J. P. Lesley.*—p. 295, Registering Thermometer; *James Lewis.*—p. 297. Biographical notice of the late THOMAS NUTTALL—p. 320. Optical education: *Dr. Emerson.*—Obituary notice of JOSEPH ADDISON ALEXANDER; *John Leyburn, D.D.*

TRANSACTIONS OF THE ACADEMY OF SAINT LOUIS, (Mo.;) vol. i.—p. 583 Observations on the cretaceous strata of Texas; *B. F. Shumard.*—p. 606, Descriptions of new cretaceous fossils from Texas; *B. F Shumard.* The author describes the following: *Nautilus Texanus; Janira Wrightii; Ostrea quadriplicata; O. bellaplicata; Cidaris hemigranosus*—p. 624, Description of five new species of Gasteropoda, from the Coal measures, and a Brachiopod from the Potsdam Sandstone of Texas, *B. F. Shumard*, viz: *Pleurotomaria Brazoensis; P. tenuistriata; P. Riddellii; P. glandula; Murchisona Texana; Orthis Coloradoensis.*

MÉMOIRES DE L' ACADÉMIE IMPÉRIALE DES SCIENCES, ARTS ET BELLES-LETTRES DE DIJON, second series, Tome vii, 1858–1859. Dijon, 1859, 800 pp., scientific contents *catalogues des Insectes coléoptéres du département de la Cote-d'or (suite), par M. Rouget.*

Une visite á la grotte de Fouvent (Haute-Saône): Ossements fossiles et debris de l'industrie humain; memoire posthume de *M. Nodot.*

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POSTSCRIPT, September 7.

LeVerrier's Report on the Solar Eclipse of July 18, 1860, *at Tarazona in Spain*, (*L'Institut*, Nos. 1387–88–89. Aug. 1–16.)—[At the last moment, and after our notices of this phenomenon were printed (see pp. 281, 285, 288), we have received LeVerrier's Report of the Observations of the French Expedition to Spain, made to the Minister of Public Instruction, which we hasten to lay before our readers, slightly condensed, although other matters which some of our correspondents will naturally look for here are thereby displaced. The interest with which LeVerrier's new views of the physical constitution of the Sun will be read is our apology to all such.]

LeVerrier was accompanied to Spain by Messrs. Yvon Villarceau, and Chacornac, who were occupied chiefly with determining the height and position of two or more of the luminous appendages. M. Foucault studied the corona, and made the photometric and photographic experiments. M. LeVerrier observed the astronomical phases of the phenomenon, and was also charged with the duty of obtaining an exact description of the whole scene. Two telescopes on Foucault's plan were devoted to the measurements, being provided with micrometers of peculiar construction, devised by Yvon Villarceau for rapid and easy manipulations in the dark. Two excellent telescopes of 6 inch aperture (one for the use of the Spanish observers) were also provided, to which must be added the photographic apparatus, a meridian circle, chronometers, barometers, seekers, and lastly the great meridian instrument belonging to the War Department, and with which the longitude was determined—forming a grand total of scientific baggage which on the 28th of June was dispatched for Spain. The outfit of the English expedition was even yet more considerable. Early in July Mr. Yvon Villarceau joined the instruments at Tudela in the center of Spain, on the banks of the Ebro, and immediately proceeded with them to Tarazona and to the chosen station called the *Sanctuaire*, 1,400 metres (=4592 feet) above the sea. M. LeVerrier and Foucault, fearing clouds, descended on the morning of the 18th to a plateau near the cemetery of Tarazona where the weather was magnificent during the whole eclipse. Passing the description of the contacts and observations for time, &c., we note that at totality they found the general illumination of the atmosphere much greater than the relation of former observers of total eclipses had led them to expect, so that they could read and write easily without using their lamps.

Says LeV.: "The first object which I saw in the field of the telescope after the commencement of totality was an isolated cloud separated from the moon's border by a space equal to its own breadth, the whole about a minute and a half high by double that length. Its color was a beautiful rose mixed with shades of violet, and its transparency seemed to increase even to brilliant white in some parts. A little below on the right two clouds lay superimposed on each other, the smaller above, and the two of very unequal brilliancy. The rest of the western edge of the disc and the lower part showed nothing more than the corona, the light of which was perfectly white and of the greatest brilliancy. But 30° below the horizontal diameter on the east I discovered two lofty and adjoining

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peaks, the upper sides both tinted with rosy and violet light while the lower sides were brilliant white. I do not doubt that the toothed form I assign to these peaks is real, which as it contrasted with that of the first appendages I have described, I verified with great care; moreover, in shifting the telescope, whose high power permitted a sight of only a small part of the solar disc at one time, I saw a third peak a little higher, also tooth-formed, and resembling the two others in color and form, differing only in its larger dimensions. The remainder of the disc offered nothing remarkable, and on returning to the upper region I found the two first described clouds unchanged. As the moment of reappearance of the Sun approached, and while waiting for the first rays, I made, during about 20^s, perhaps my most important observation. The margin of the disc which two minutes before was entirely white was now tinged by a delicate fillet of unappreciable thickness of a purplish red—then as the seconds glided by, this fillet enlarged by degrees and formed soon around the black disc of the moon, over a breadth of about 30° a red border perfectly defined in thickness, crescent-formed and with an irregular outline above. At the same instant the brilliancy of the part of the corona which during the last second emerged from behind the moon's disc was exalted so rapidly that I was in doubt if the sun's light was not returned. It was only on the reappearance of the direct rays, the brightness of which obliterated in turn the corona, that I was sure of the nature of three phenomena present at the same time, which I thus sum up.

1. The visible part of the emergent Sun over its whole breadth and up to the height of seven or eight seconds was covered by a bed of rosy clouds, which appeared to gain in thickness as they emerged from behind the disc of the moon. Must we believe that the entire surface of the sun is overspread at a small elevation as it is strewn with faculæ, and that the roseate clouds are emanations, appearing like spots on the sun's disc?

2. The intensity of the light in the corona which is always white, varies with great rapidity in the immediate vicinity of the Sun's disc.

3. The reappearance of the direct sunlight was at 3^h 0^m 49^s·0. Total obscuration continued 3^m 14^s·3. The disc of the moon completely cleared the Sun at 4^h 6^m 20^s."

[M. Foucault's interesting observations on the photographs, etc., are unavoidably postponed for want of room.]

LeVerrier goes on to state that the observation of his party authorize, in his opinion, important modifications in the generally received notions respecting the physical constitution of the Sun. Arago in his notice of solar eclipses—says, "where exist the reddish flames with well defined outlines which during the total eclipse of the 8th of July, 1842, passed considerably beyond the outlines of the lunar disc? These flames were either in the Moon, or in the Sun, or in our atmosphere; unless, indeed, denying their actual existence, we regard them as an effect of light, for example as phenomena of diffraction."

The two last suppositions have found few partisans. Before adopting any hypothesis it is necessary to decide by observation a certain feature of the phenomenon. During the eclipse, the disc of the moon moves across the disc of the Sun. But do these reddish clouds follow the moon in

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its movement? or does each cloud remain invariably above the same point on the solar disc? In the first case the origin of the luminous clouds is to be sought in the Moon; in the second case, these clouds belong to the Sun. For clearness sake, assume the latter supposition, and observe what appearances should present themselves when the lunar disc passes like a screen over the whole. Consider first a cloud situated on the east and adherent to the Sun's limb. This object will be visible at the instant when totality commences. The advancing moon will regularly, at the rate of a half second of arc in a second of time, cover with its limb successively the lower then the middle and lastly the higher portions—thus constantly diminishing the height of the cloud. For a cloud situated on the west, these appearances will be reversed, its magnitude increasing as the moon gradually uncovers it. If then the roseate appendages seen during a total eclipse, depend on the sun, the fact should appear by the variation in height between those which appear in the east and the west. The phenomena will appear otherwise if the clouds appertain to the moon.

In the absence of equatorial solar clouds, the question in dispute can still be decided by observations on those seen on the south or north of the disc. The height of these clouds ought not to vary, it is true, whether they belong to the moon, or to the sun, but in the latter case, carried away by the sun they would be displaced on the lunar disc with a certain velocity, while if they are adherent to the moon's disc, they would not be so displaced Hence the study of the height of the luminous clouds, whether east and west or north and south, has the highest interest. All the elements of the desired demonstration are found in the Spanish observations. In my first report, I mentioned the successive increase in thickness of a band of rosy clouds visible from the east to the end of the eclipse. Messrs. Yvon Villarceau and Chacornac have carefully noted the motions of a cloud, situated on the north. This cloud, according to M. Villarceau, in two minutes' time was displaced $3\frac{1}{2}°$ on the moon's disc, in moving to the west. The measurements of M. Chacornac cover an interval of over six minutes in time, in which the cloud moved $11\frac{1}{2}°$.

Beautiful observation, and one which could not have been hoped for! We see that the duration of the motion studied by Chacornac, much exceeds the time of total obscuration. The last measure was made more than three minutes after sun-light had reappeared! It is important to note, among other points that this was not done with a cloud vaguely seen after the return of sunlight, but fortunately it was a measurement so carefully made as to be a guaranty against the possibility of illusion. It should be added that the displacement of the luminous cloud determined by the observations made at the *Sanctuaire*, is precisely equal to that required by calculation, assuming the cloud to belong to the Sun. There remains, then, no foundation for a doubt, as to the nature of the rosy clouds which have been variously called flames, mountains, protuberances, and clouds.

The observation on one of the appendages, perfectly isolated from the disc of both the Sun and Moon and of a sharply pronounced character, and on the other the appearance of a rosy band on the west at the moment of emersion, and the rate of motion of a second appendage, fixed by Villarceau and Chacornac, prove that these objects belong to the Sun.

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Let us then hereafter give the name of *solar clouds* to the rosy appendages which become visible when the solar light is sufficiently dimmed.

A few words more will finish the description of the phenomenon and of the observations. Ismaïl-Effendi, a young Egyptian attached to the Paris Observatory for three years past, a very expert astronomer, and who accompanied the French expedition to Spain, has sent me a drawing which proves the appearance of luminous clouds in the east immediately before the commencement of the eclipse. The clouds in question form a slightly elevated but continuous band embracing 90° of the outline of the Sun. This band was not long visible, but was eclipsed behind the lunar disc, and it had in effect ceased when I passed over this region in exploring the whole periphery with a power which allowed me to see only portions successively.

The magnetic observations were made at Paris, the variations being sensibly simultaneous for the whole of Europe, and M. Desains, who took note of the magnetic observations, detected no perturbations during the eclipse.

Physical constitution of the Sun.—A reconstruction or even a complete abandonment of the theory hitherto prevalent as to the physical constitution of the Sun appears to me essential. It must give place to one far more simple.

We have been hitherto assured that the Sun was composed of a central dark globe; that above this globe existed an immense atmosphere of sombre clouds, still higher was placed the photosphere, a self-luminous, gaseous envelope, and the source of the light and heat of the sun. Where the clouds of the photosphere are rent, says the old theory, the dark body of the Sun is seen in the spots which so frequently appear. To this complex constitution must be added a third envelope formed of the accumulation of roseate clouds.

Now, I fear that the greater part of these envelopes are only fictions—that the Sun is a body, luminous, simply because of its high temperature, and covered by an unbroken layer of roseate matter whose existence is now proved. This luminary thus formed of a central nucleus, liquid or solid, and covered by an atmosphere, falls within the law common to the constitution of celestial bodies.

[M. LeVerrier goes on to discuss with some detail the solar spots in the light of these new views, but this we must defer for another occasion. It is certain that a subject of so much interest will command much consideration from physicists and astronomers and we shall take care to give it the attention it deserves.

Nor will the question be settled peaceably—already M. Faye (*Comptes Rend.*, Aug. 13) in presenting to the French Academy a long letter from Baron Feilitzsch with an account of his observations (also in Spain), declares it to be his opinion as well as that of Baron F. that the eclipse of 1860 furnishes the most decisive evidence in favor of the opinion which refers the corona and the luminous clouds to simple optical appearances, and not to the essential constitution of the Sun or of his atmosphere. M. Faye adds that the opinion appears to be confirmed by a comparison of the results of other observers—that the Sun has no atmosphere and that the appearances recorded are purely optical!—Eds.]

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THE

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[SECOND SERIES.]

ART. XXVIII.—*Lecture on the Gulf Stream, Prepared at the Request of the American Association for the Advancement of Science;* by A. D. BACHE, Supt. U. S. Coast Survey.*

[Delivered before the American Association for the Advancement of Science, at Newport.]

BY request of the Association, at their last meeting, at Springfield, I now present a summary of the results of the Gulf Stream explorations made by the officers of the Coast Survey.

The Gulf Stream is the great hydrographic feature of the United States coast, and no survey of the coast could be complete for purposes of navigation, without it. Hence the explorations have been early undertaken and thoroughly carried on. But as it required peculiar means and special adaptation in the officers to this line of research, and did not require a continuous survey, the work has been executed from time to time, as means and officers could be had without interference with the more regular operations of the hydrography.

An act of Congress which refers to this Survey, requires the immediate presentation of its results to Congress and they have therefore been discussed as soon as procured and have been given to the public.

This is the great sea mark of the coast of the United States, both Gulf and Atlantic, and its qualities as hindrances and aids to navigation require that the navigator should be well informed in regard to it.

* Communicated by the Author.

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In order to present an intelligible summary of the results obtained by the Coast Survey in the short time allowed for a lecture, it is necessary to condense the subject very considerably, to omit matters at all extraneous to the subjects in hand, and to confine myself to a brief and direct statement of *the means employed in examining the stream from its surface to its depths, the method of studying the results*, and *of the results themselves.*

The temperatures in and near the Gulf Stream, are among its most striking peculiarities, and therefore have formed one principal object of observation. It will be necessary in order to bring the subject within limits, to confine myself chiefly at this time to the consideration of this class of facts and to the results and laws connected with them.

I shall proceed therefore to consider the subject under the following heads:

1. The Instruments for determining depths and temperatures and for obtaining specimens of the bottom.
2. The plan of research.
3. The method of discussion of the results.
4. The results, consisting of type-curves of the law of change of temperature with depth, at several characteristic positions. Type-curves showing the distribution of temperatures across the stream, represented by two sets of curves, one in which the variable temperatures at the same depth is shown, and the other in which the variable depth of the same temperature is represented. Upon the diagrams showing these latter curves, the figure of the bottom of the sea is given, where it has been obtained.

Discussion in regard to the cold wall, which is one of the most interesting features of the approach to the Gulf Stream.

5. The limit of accuracy of the results.
6. The figure of the bottom of the ocean below the Gulf Stream.
7. The general features of the Gulf Stream as to temperature.

These points are illustrated by diagrams, enabling the eye to follow the results as they are stated.

I. Instruments.

1. *For Temperatures.*—The instrument for determining temperatures should fulfill the two conditions of registering its indications and of being unaffected by pressure. The common mercurial thermometer, while it answers perfectly for the determination of temperatures at the surface, fails in both the conditions stated. The ordinary self-registering thermometer, or self-registering metallic thermometer, in the watch form, as made by Breguet, Montandon, and Jürgensen, when provided with a suitable cover to protect it from pressure, answers a good purpose, and has been extensively applied in the course of the observations. As

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a rule it is only the minimum temperature thermometers that must be used, as the temperatures decrease generally in descending. An ordinary self-registering minimum thermometer placed in a glass globe, was successfully used by Commander Charles H. Davis, and by Lieut. G. M. Bache. It has the disadvantage of taking the temperature slowly, and of being inapplicable below a certain depth. Small hollow cylindrical brass vessels which were divided in two parts closely fitted by grinding, and within which the Breguet thermometers of the watch form were placed, were an improvement upon the glass globe, as taking the temperature of the sea more rapidly, but besides the difficulty of making the joint tight, they were crushed by the pressure, at even moderate depths. The substitution of a globe, for the cylinder, extended the range of these instruments, but the thermometers were often crushed or injured by the access of sea-water to the interior of the globe. Six's self-registering thermometers as bearing considerable pressure without injury and without rendering the indications erroneous, and as requiring no case to enclose them, except to prevent breaking from accidental knocks in handling, are very useful. They are still favorites with many of the officers, though others complain of their great liability to derangement, especially if the mercury is not perfectly clean, when the mercurial column easily separates and some skill is required to bring it together. These instruments are from their cheapness still furnished to the parties and are used successfully at depths reaching about one hundred fathoms, and on occasions, considerably lower. Keeping them in order requires the skill of an experimenter, rather than that of an observer, and hence they do not satisfactorily fulfill the conditions of the problem. The metallic thermometer of Joseph Saxton, Esq., of the U. S. Office of Weights and Measures, is a compound coil resembling somewhat the well known instrument of Breguet. In its construction, two stout ribbons, of silver and platinum—carefully united by silver solder to an intermediate thin plate of gold—are coiled with the more expansible metal in the interior. The gold serves to prevent the tendency of the silver and platinum to separate. The lower end of this coil is fastened to a brass stem passing through the axis of the coil, while its upper end is firmly attached to the base of a short cylinder. The whole motion of the coil as it winds and unwinds with variations of temperature, thus acts to rotate the axial stem. This motion is magnified by multiplying wheels contained in the short cylinder at top, and is registered upon the dial of the instrument by an index, which pushes before it a registering hand, moving with sufficient friction merely to retain its place when thrust forward by the index hand of the thermometer. These instruments are

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graduated by trial. The brass and silver portions receive a thick coating of gold by the electrotype process to prevent the action of the sea-water upon them.

When kept clean by frequent washing in fresh water, and in good order and frequently compared with the standards to guard against accidental derangements, these thermometers answer admirably all the required conditions. The length of the coil measured along its axis should not be less than six inches, as the interposition of wheels to magnify the motion, should as far as possible, be avoided. The water being all around the coil, which is a good conductor, and has a low specific heat, the instrument readily feels the temperature of the part of the sea where it is exposed, and registers it to less than half a degree (say 0·2) with certainty. The box which covers the coil and indicating part of the thermometer is merely to protect it from accidental injury, and is open so as to permit the sea-water to pass freely through it. Plate IV gives a view of Saxton's metallic thermometer, and of its various parts in detail. Although there seemed no reason to doubt that this instrument was free from any effects of pressure, it was deemed desirable to actually try it by extreme pressure and a series of experiments made by J. M. Batchelder, Esq., showed that at pressures less than that corresponding to 600 fathoms, the effect was less than one degree (0°·25 to 1°) and at pressures from 600 to 1500 fathoms the change amounted to little more than from 7° to 9° Fahr., the index returning when the pressure was removed. For great depths the effects of pressure must be ascertained, as it is specific in each instrument and probably depends chiefly upon some mechanical defect in the construction, perhaps in the soldering.*

The apparatus used in these experiments on the effect of pressure, was a very ingenious one for testing hydraulic engines by Mr. Thomas Davison of the Novelty Iron Works of New York. Fig. No. 12, Plate IV.

2. *For Depths.*—Where the depth becomes considerable the usual sounding line fails entirely to give it, especially if there is a current and more especially if there is besides, a counter-current. The amount of "stray line" is very variable. This subject has been ably examined of late years by Commanders Maury and S. P. Lee, Lieuts. Berryman, Brooke and others of our navy, and by Commander Dayman and others of the British navy, and especially by Prof. Trowbridge of the Coast Survey in his memoir read before the Association ("Deep Sea Soundings," by W. P. Trowbridge, Assistant U. S. Coast Survey,) at the meeting in Baltimore and re-published in the American Journal of Science and Arts, vol. xxviii for the year 1858.

* Gulf Stream Explorations; Third Memoir Proceedings Amer. Assoc. Adv. Sc., 13th Meeting, Springfield, 1859, and this Jour., [2], vol. xxix, 1860.

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The use of Ogden's or Ericcson's leads to 100 fathoms is still continued by some of the officers of the survey, though, at such depths, nothing better than the common sounding line is in fact required. Massey's lead with Woltman's wheel, as an indicator. has been extensively used of late years. Mr. Saxton's indicator is more simple than Massey's, but acts upon the same principle. To remedy the defect of the turning of the cord of the lead line, two indicators are applied, one on each side of the axis. Prof. Trowbridge's lead modified somewhat from that described at the last meeting of the Association in Baltimore, has recently been tried with good success by Lieut. Comdg. Wilkinson in the last soundings across the Straits of Florida for the telegraph to Havana. The most reliable observations heretofore made in the Coast Survey have been with Massey's indicator, the errors are not such as to affect the development of the laws of change at the moderate depth reached in most of the observations, and at great depths the changes are very slow. The new apparatus has the advantage of saving a great deal of time and therefore inaccuracies from change of position during the sounding are avoided.

3. *For obtaining specimens from the bottom.*—The only satisfactory test of having reached the bottom of the sea at considerable depths being the bringing up of a specimen, this has been a subject of constant study with us. The different instruments invented by Lieut. Stellwagen, Commander Sands, Lieut. Craven, Lieut. Berryman, Lieut. Brooke and other officers of our navy, are all in use for different kinds of bottom, and according to the preference given by different hydrographic chiefs. The one most commonly used in these explorations has been Lieut. Stellwagen's invention; a cup placed below the sounding lead, covered by a dick or valve of leather which slides up the stem of the cup and opens when the lead is descending, closing when it is raised. The weight of the lead and the turning of the cord generally suffice to sink the cup into the bottom, filling it, and when the valve is made to close tightly by a piece of flexible leather below the stiff disk, the specimen is not washed out as the lead is drawn up. In Commander Sands' sounding apparatus a spring keeps an outer cylinder over an opening in an inner hollow one, until it reaches the bottom, when the outer cylinder is forced upwards and the opening at the side of the inner one, which, having a conical termination, penetrates the bottom, permitting a specimen of the bottom to pass in. On raising the lead the spring forces the outer cylinder over the opening, preventing the specimen from being washed out. The only very deep soundings being, as a general rule, in soft bottom, Sands' specimen-cylinder is admirably adapted to that class of work.

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II. Plan of the Work.

The plan of the work was simple. The temperatures were to be ascertained at various depths, at different distances from the coast, on sections as nearly at right angles with the stream as practicable, the sections starting from some point well known in position. The temperatures were to be taken at distances diminishing as the changes of temperature were more rapid. So in regard to the depths, the observations were to be multiplied in the strata of rapidly varying temperatures near the surface. So in regard to position, when the cold water near the coast was rapidly exchanging for the warm water of the Gulf Stream, the sections diminishing in distance as the source of the warm water was approached.

The vessel's position was determined with reference to some prominent point, Sandy Hook, or Cape May, for example, the course run was perpendicular to the supposed axis of the stream, S.E., several positions were taken up in succession and at each the temperatures ascertained at the surface, at 5, 10, 15, 20, 30, 50, 100, 200, 300, 400, 600 fathoms, or depths found to apply more satisfactorily under the general rule, to the position and section. Having crossed the stream any position found to be desirable, could be assumed on returning, and the extreme position reached was verified by the return to the coast.

The summer season was selected for the standard observations for various reasons, but chiefly for two, namely, that the weather permitted more accurate work, and the phenomena were more likely to be those of equilibrium, when the surface water was more slowly changing its temperature. Our little vessels could not, without considerable danger, be exposed to the roughness of the wind and water in the Gulf Stream in winter, and when we attempted comparative winter observations, disappointment was often the result. The loss of one valuable officer and ten of his crew, and the extreme peril of another in autumnal explorations of the stream, has but too fully justified these precautions. The propriety of selecting the summer for making the observations was completely proved by the success in determining the laws of temperature.

These observations were but incidental to the hydrography of the coast, and hence were prosecuted only when means could be spared from other more pressing and regular parts of the work. It was only a favorable conjuncture with regard to officers, means, weather, adaptation of vessel, and the like, which gave results even when attempted. Too much credit cannot be assigned to those who have succeeded in this laborious and perilous work, and their names have been kept in close connection with their results, whenever and wherever brought before the public, and they have been carefully preserved in the archives

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of the Survey. Charles H. Davis, George M. Bache, S. P. Lee, Richard Bache, John N. Maffitt, T. A. Craven, Otway H. Berryman, B. F. Sands and John Wilkinson make up the list of our successful observers in this field within the last sixteen years. Their names you will see attached to the sections run by them on the general chart of the Gulf Stream presented to you this evening.

The first was run in 1844, from Nantucket south and eastward, by Commander C. H. Davis, now the accomplished Superintendent of the Nautical Almanac, and the last in 1860 by Lieut. John Wilkinson, from the Tortugas, southeast to the coast of Cuba. The work still goes on perseveringly.

The number of sections run has been fourteen, the number of positions on these sections occupied 300, and the number of observations made for temperature 3600. The limits below which the stream and the adjacent waters have been explored for temperatures are from latitude 23° N., to 41° N., and from longitude 83° W. to 66½° W. from near Havana to near Cape Cod, and from the Tortugas to 9½° E. of Cape Henlopen. The distance along the axis of the Gulf Stream to the most northeastern point in the North Atlantic, measures nearly 1400 nautical miles.

III. Method of Discussion of the Results.

These have generally been discussed by diagrams, sometimes by analytical formulæ; the former method is generally best adapted to the character and degree of accuracy and circumstances of the observation; the diagrams finally adopted after trials were chiefly of three different kinds, one for the discussion of the change of temperature with depths, the two others for the change of temperature with position as well as depth. Of the first of these diagrams Nos. 1 and 2, Plate I, are specimens. The depths constitute the ordinates and the temperatures the abscissæ of a curve, showing the law of change of temperature with the depth. Upon the horizontal lines at the top of the paper the temperatures from ten degrees to ten degrees Fahr. are written and on the vertical line at the side, the depths. The separate observations being represented by dots; the curve is drawn with a free hand among them.

The next two classes of diagrams give the distribution of temperatures across the sections. In the first the temperature corresponding to the same depth; in the second the depths corresponding to the same temperatures. In this latter the figure of the bottom is shown when ascertained. In both classes the distances from the cape, or headland, city or inlet, which is the origin of the section is marked, and the several positions occupied for observing, so that the abscissæ of the curve are the distances from

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the point of beginning. In the first (see diagram No. 4, Plate I,) the temperatures are marked on the vertical lines at the left side of the diagrams, the ordinates of the curves thus corresponding to temperatures. In the second (see diagram No. 9, Plate I,) the depths are similarly written, the ordinates thus corresponding to depths. The notes or legend, show in the first case to what depths the curves correspond and in the second to what temperatures. The observations at each position being plotted according to its temperature or depth in the two classes of diagrams, the curve is drawn with a free hand among the points.

It should be observed that the discussion of each season's observations was in general made separately, and that the result of one, two or three seasons, grouped, were announced separately, leaving to the new observations to confirm, or refute, the conclusions drawn. It is a remarkable fact that with such difficulties in the way, in the character of the phenomena to be observed, in the diversity of seasons and of observers, the phenomena have always been readily deducible from the observations, and that the separate discussions have been confirmations, the following of the preceding; in short that the nature of the medium in which the work has been performed in its relations to heat, has more than compensated for other difficulties and that the results are more accordant than the elaborate ones obtained from the progress of temperature below the surface of the ground by the experienced and skillful observers who have made them. Few observations have been rejected in the whole series.

I need not notice special diagrams which will be explained when your attention is called to them.

When the character of the diagrams to be made had been definitely fixed, they were prepared under the direction of the chiefs of the parties, so that I was relieved of the personal labor of representing the results. In the subsequent general discussion I was greatly assisted by Prof. Pendleton, U. S. N. and by Prof. W. P. Trowbridge, Assistant U. S. Coast Survey, who has made a general review of the whole of the results preparatory to their publication in a volume of the Records and Results of the Coast Survey.

IV. Results.

1. *Type-curves of law of temperature with depths at the most characteristic positions.*—The two most characteristic positions are in the cold current between the land and the Gulf Stream and in the axis of the stream itself.

1. Diagram No. 1, Plate I, is a specimen of the type-curve in the cold current. The long tongue from the surface to about 50 fathoms in depth is the overflow of the warm water of the Gulf Stream, the temperature varying from 81° to about 55°. The

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temperatures in the mass of water from 50 fathoms down to 500 fathoms are just such as would take place in a mass of water heated by conduction from the surface, the law is that of a logarithmic curve, in which the conducting power of sea water is the modulus of the system.

A comparison of many of these curves with the logarithmic form showed that it was applicable to them within the limits of the probable error of the observations. Taking the warm stratum from the Gulf of Mexico above and the cold polar stratum below, the mass of the water between is heated by conduction. The bottom of the sea has not been reached under the axis of the Gulf Stream, north of Cape Lookout on the North Carolina coast.

This form of curve was deduced in 1844 from the observations of Commander Charles H. Davis and was the first discovery made in connection with the then recently commenced systematic exploration of the Gulf Stream by the Coast Survey.

2. Nos. 2, 3, and 3 bis, Plate I, are specimens of the type-curve in the Gulf Stream, taken from the sections off Cape Henry, Cape Hatteras and Charleston, being characterized by the comparatively short beak or projection and the persistence of the higher temperature to great depths as 55° to 425, 450, 550 fathoms giving the peculiar shape to this curve between 50 and 500 fathoms.

II. Type-curves of distribution of Temperature across the Stream.

(*a.*) *Curves of temperature at the same depths.*—The sections made are the following, beginning the enumeration at the Gulf of Mexico: 1. Tortugas to Havana. 2. Sombrero Key to Salt Key. 3. Carysfort, L. H., to Cuba. 4. Cape Florida to Bemini. 5. Off Cape Cañaveral. 6. Off St. Augustine. 7. Off St. Simon's, Georgia. 8. Off Charleston. 9. Off Cape Fear. 10. Off Cape Hatteras. 11. Off Cape Henry. 12. Off Cape May. 13. Off Sandy Hook. 14. Off Cape Cod, being on the average one to each hundred miles along the axis of the stream. These are marked on the general chart, Plate III, the names of the explorers being stated in the column which gives the point of origin of each section.

The Sandy Hook curves, Nos. 4 and 5 Plate I, are among the best of the type-curves of temperature at the same depth, though among the earliest determined. The overflow of the Gulf Stream into the long space occupied by the cold current between it and the shore, mixing in a degree with the cold water, is well shown by the curves *a*, *b* and *c* at the surface, 5 and 10 fathoms, and the still greater admixture with the cold water at 20, 30 and 50 fathoms (*d*, *e*, *f*.) The whole space from the shore to 240 miles, is occupied, however, with comparatively cold water. Then is

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met the sudden rise to the Gulf Stream shown especially below 50 fathoms and termed so appropriately by Lieut. George M. Bache the "cold wall," that navigators have not hesitated to receive the term into use; next the hot water of the Gulf Stream, rising to a maximum of 82°, then falling to a minimum of 80°, rising to a second maximum of 81¼°, falling to a second minimum of 78° and rising from this toward a third maximum. With these results the curves at 5 and 10 fathoms and those at 20, 30, 50, 70, 100 and 150 fathoms agree and, with characteristic differences, those of 200, 300, 400 and 500 fathoms.

The cold wall at 20 fathoms shows a rise of 19° in 25 miles, three quarters of a degree to a mile, and at 200 fathoms of 16°, in the same distance; at the surface it is nearly 8° in 50 miles. The cold water between the Gulf Stream and the shore has two well marked maxima and two minima in it, of which one seems to correspond in position to the sudden deepening of the water 100 miles from Sandy Hook, as shown by the Coast Survey off-shore chart between Gay Head and Cape Henlopen.

These results are more distinctly seen by grouping the curves into natural groups and taking the mean of their indications. Diagram No. 5 Plate I, gives the group of six curves from the surface to 30 fathoms, of four curves from 40 to 100 fathoms, both inclusive of 200, 300, and the single curve at 400.

Similar groups are shown on Diagram No. 6, Plate I, from Cape Henry, the cold wall, three maxima of temperature and three minima being very distinctly seen. The results of three different explorations of this section, by three different officers, in three different years, are shown upon the same diagram. The coincidence of result could hardly be better. The average of the whole of the observations is shown in No. 6 bis, Plate I.

The cold wall here gives a change of 22½° in 50 miles from the curves between 0 and 30 fathoms and 18° in 50 miles in the mean of 200, 300 and 400 fathoms.

The average of the three years comes out beautifully on Diagram No. 6 bis, Plate I. The Charleston curves are shown upon No. 7, Plate I. They are less regular than those just given, for reasons which will appear, when I come to speak of the second class of diagrams.

The conclusions deduced from the examination of all the sections between Cape Florida and Sandy Hook is, that the Gulf Stream is divided into alternate bands of hot, or warm and cool or cold water, the most distinct of which is that containing the axis of the Gulf Stream.

That between the stream and the coast there is a fall of temperature so sudden that it has been aptly called the cold wall, less distinct at the surface and where the overflow from the Gulf Stream passes furthest toward the shore, but still distinctly marked even at the surface.

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Navigators have noticed these changes of temperature and have supposed themselves at each occurrence of warmer water to be in the hottest water of the stream and so have been greatly embarrassed and have deemed the phenomena and limits of the Gulf Stream to be very irregular.

The cold water between the Gulf Stream and the shore has also bands less regular than those beyond the axis of warmer and cooler water.

The intrusive cool water in the Gulf Stream on the Sandy Hook section was distinctly recognized in 1846 by Lieut. Geo. M. Bache, who from the facts observed, supposed it to represent a division of the warm water of the stream into two branches.

Passing through the Straits of Florida between the keys and reefs and the coast of Cuba we have after going beyond Cape Florida, a different type-curve. The cold wall is less distinctly marked and the rise of temperature is less marked. It rises however to an axis near the coast of Cuba. Throughout the length of the Strait there is but one maximum of temperature and the bands belonging to the Atlantic regimen do not occur in the straits. (See diagrams Nos. 3, 4, 5, 6, Plate II.) The cause of this change of regimen will be seen in presenting the other form of diagram.

(*b.*) *Curves of depths at the same temperature.*—I have selected curves from the southern portions of the work, partly because the bottom has been struck in the sections and the diagrams show its sections as well as those of the stream, and partly to show how fully the deductions in regard to the divisions of the stream, apply to these, as well as the more northern sections. The Charleston section of Lieut. Maffitt is given on diagram No. 9, Plate I. The surface curve, notwithstanding the disturbance by a storm, shows the cold wall, (see also No. 7) the axis and two other maxima, the corresponding minima, a maximum within the cold current which is not therefore, as has been supposed, cut off at Hatteras, the curve of 72° reaching to the coast and 77° nearly reaching it. The Cape Florida diagrams (Nos. 3 and 7, Plate II.) give two maxima with indications of a third and the corresponding minima. The cold wall cannot be recognized upon it, probably for the want of one or two, more positions.

The form of the bottom delineated on these two sections, namely the Charleston and Cape Florida sections, is remarkable and applies to the sections between them as far as explored. First is a gentle slope, then a sudden descent, a second steep pitch to a considerable depth, a range of hills, a valley and a second range.

The correspondence of these features with the bands of temperature is plainly marked. The cold water lies in the valleys and passing along the bottom rises upon the tops of the hills. The discovery of this range of hills was made at nearly the same time

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by Lieut. Maffitt on the Charleston section and by Lieut. Craven on the St. Simon's section. Diagram No. 9, Plate I, shows this connection in a very striking manner as does also No.7, Plate II, and the figure of the bottom of the straits of Florida, shows why there are no bands formed prior to passing Cape Florida, in other words, why the regimen of the stream is different in the straits and in the Atlantic. In the straits we see (No. 9, Plate II) that after leaving the United States shore and the comparatively flat surface extending to the reefs, there is a rapid descent toward the Cuban side of the strait, the axis of the Gulf Stream being found in the deep hollow of that side of the strait.

These results, with a more elaborate discussion of them, were presented at the last meeting of the Association. It would seem from the configuration of the bottom, that the cold stream at the bottom of the straits of Florida divides, one portion passing to the north and west into the Gulf of Mexico and the other around the western end of the Island of Cuba. That the polar stream still occupies the bottom of the strait is shown by temperature of 35° Fahr. being reached at 600 fathoms from the surface off Havana.

Do these bands correspond throughout their length to the form of the bottom of the sea? This is not yet made out, many as have been the attempts to reach the considerable depths off the more northern sections. Three officers have attempted to sound out the Cape Cod section, but the cold wall is all that has been reached thus far. The range of hills nearest to the coast, has been traced from the coast of Georgia by Commander Sands to off Cape Lookout.

III. The Cold Wall.

The cold wall extends with varying dimensions and changes of its peculiar features, all along the coast where the stream has been examined. A diagram showing the features of the cold wall on the various Atlantic sections and those of the straits of Florida is given in No. 10, Plate I. Table No. 1 shows the distance of the cold wall from the coast and the dimensions of the Atlantic bands of the Gulf Stream.

The table shows that at Cape Florida and Cape Hatteras the cold wall is nearest to the coast. The distance of the axis of the stream from the coast will be found by adding half the numbers in the second column to those in the first column. It is obvious from these numbers, when taken in connection with the longitudes of the points where the sections originate, that the earth's motion is not the sole determining cause of the direction of the axis of the stream, a result which a more elaborate investigation of the movements from parallel to parallel confirms. In the portions of its course between Cape Florida and Mosquito inlet ($3\frac{1}{2}$° of latitude) the curve is actually slightly to the westward.

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TABLE 1.—*Distance of the cold wall from the shore, and widths of the several bands of cold and warm water in the Gulf Stream, measured on the lines of the Sections.*

Names of Sections.	Distance of cold wall from shore in miles.	Width of 1st maximum or warm band.	Width of 2d minimum or cold band.	Width of 2d maximum.	Width of Gulf Stream, proper.	Width of 3d minimum or cold band.	Width of 3d maximum or warm band.	Width of 4th minimum or cold band.
Sandy Hook.....	240	60	30	37	127	60	50	In 'ef.
Cape May.....	125	55	30	40	125	70	65	70
Cape Henry.....	95	45	32	47	124	80	60	50
Cape Hatteras.....	30	47	25	45	117	37	75	70
Cape Fear.....	60	30	20	37	87	30	60	25
Charleston.....	62	25	15	30	67	26	35	—
St. Simons.....	87	25	13	20	58	25	25	—
St. Augustine.....	70	20	13	12	47	22	20	—
Cape Cañaveral.....	35	20	—	—	35	14	12	—
Cape Florida	10	25	—	—	25	5	—	—

Note.—The width of the bands beyond the 2d maximum, and north of Cape Hatteras are somewhat indefinite.

The table shows a width in the Gulf Stream proper along the Atlantic coast of from 25 miles off Cape Florida to 127 miles off Sandy Hook. The warm water at say fifteen fathoms, is from 30 to 150 miles in width. The stream widens each way from Cape Florida. These several divisions of the Atlantic stream lose a portion of their distinctness as we pass northward and eastward, the stream widening.

IV. LIMIT OF ACCURACY OF THE DETERMINATIONS.

There are two modes by which the limits of accuracy of these results may be tested, by one of which their permanency is also tried. In this latter mode the sections are run over in different years, or in the same year by different officers, so as to connect the observations of one year with those of the next, or of one officer with that of another. Table No. 2 shows that the relative results are reproduced from year to year with less variability than those of the mean temperature of the section; and hence the permanency of the bands and the possibility of observing them with the requisite precision must be admitted. On the Cape Henry section which was explored three times, the position of the cold wall and of the axis of the stream were reproduced within $5\frac{1}{2}$ miles and those of the succeeding points of maximum and minimum temperatures within $7\frac{1}{2}$ miles. As the positions at sea are liable to an uncertainty of some three to five miles it must be admitted that the permanency of the bands and the accuracy of the observations of them are fully proved.

The Cape Henry section was run over by Lieuts. G. M. Bache, S. P. Lee, and Richard Bache, the Hatteras section by Lieuts. Richard Bache and J. N. Maffit, and the Charleston section by Lieuts. J. N. Maffit and T. A. Craven.

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TABLE 2.—*Table showing the probable uncertainty in the determination of maximum and minimum points, by running the same section over in different years by different observers.*

CAPE HENRY SECTION.

Dates and names of observers.	Mean distances from the shore in miles from the curves representing the groups.						
	Cold wall or 1st min	Axis or 1st max.	Second min.	Second max	Third min.	Third max	Fourth min.
Lt. G. M. Bache, 1846	93	135	187	218	260	32	369
" S. P. Lee, 1847	91	146	185	215	2·1	33	338
" R. Bache, 1848	97	146	180	19	287	3·8	7
Means for three years.	84	142	184	210	279	3 8	370
Probable error for each year.	5·85	4·27	2·42	7·62	11·51	5·71	7·18

CAPE HATTERAS SECTION.

Lt. R. Bache, 1848	—	90	134	162	214	286	355
" J. M. Maffit, 1853	—	75	125	157	211	256	322
Means for two years	—	82	129	159	212	266	338
Probable error for each year	—	6·4	4·3	2·4	1·5	15	16
Means for both sections	5·85	5·3	3·4	5·0	6·4	1·04	1·16

Average uncertainty of maxima and minima, 6·9 miles.
" " cold wall and axis, 5·5 "
" " all the other points, 7·4 "

The other mode of testing the result is by the comparison of the remarkable points in the different sections, each one belonging to a different position and therefore being entirely independent of the other in its determination. It is established as a general law that this cold wall and axis of the hottest water change their position from the surface to the depth of six hundred fathoms slowly and by an ascertained progression, and that the succeeding maximum and minimum points are at the same distance from the shore, nearly, at different depths, or in a vertical line at all the different depths. The positions of these points as shown by the observations at different depths become thus the test of the permanency of their positions and of the accuracy with which they have been ascertained. Table 3 gives the probable error of the mean of the determinations of each point including the cold wall minimum, the axis maximum, and the successive minima and maxima to the fourth minimum inclusive. These results show that the cold wall minimum is ascertained, on the average, within 0·83 mile, the axis maximum within two miles and a half, the second minimum within two miles and a half, the second maximum and third minimum and third maximum, within four miles, and the fourth maximum within eight and a half miles, all being satisfactory except the last, which of course is in reality loosely defined. The Hatteras result for the axis of the stream, makes the probable error considerably larger than it would otherwise be, probably from the

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fact that the proximity of the bottom of the sea, makes the result less permanent than in the other cases. Without this result the mean probable error would be 1·1 mile.

TABLE 3.—*Recapitulation, showing the value of the probable error of determination of the bands for each section and the average of the whole.*

Sections.	1st min	1st max	2d min.	2d max.	3d min	3d max	4th min.
	Probable errors.						
Sandy Hook		·75		3·94	7·99		
Cape May	·82	1·25	2·54	1·57		4·03	4·87
Cape Henry, 3 years	·84	·61	·55	1·70	1·06	·94	3·42
Cape Hatteras, 2 years		6·77	6·36	9·31	5·69	6·23	
Cape Fear		1·25			2·98	3·49	13·37
Charleston	1·25	1·57	·72	2·09	2·40	·82	
St. Simons		·74	1·27	·41			
St. Augustine	·52	·51	·44	·44	·55		
Cape Cañaveral	·95	1·69	·39				
Mean probable error	·83	2·49	2·49	4·00	4·01	3·71	8·45

While these results are so permanent, the mean temperatures of the sections change considerably from year to year. The average temperature between the surface and 400 fathoms beyond, or outside of the cold wall on the Sandy Hook section in 1846, was as high as that on the Cape Henry section in 1848, and that on the Cape Fear section in 1853, within a degree of that of the St. Augustine section in 1853, while the Cape Hatteras section in 1848 and in 1853, differed two degrees in mean temperatures. Again the temperatures from the surface to 30 fathoms just below the axis of the stream in the Sandy Hook section in August 1846 was either as high or higher than those on the Cañaveral section in June 1853. In general the Cape May section in 1846 and the mean of the Cape Henry section of 1846, 1847 and 1848 are warmer at the same depths than the sections south of it were in 1848 and 1853.

These results show that there are great changes in temperature from year to year, and probably from season to season. Some progress has been made in connecting these results in a general way with the changes of weather in the Gulf of Mexico.

The depths at which the results are easily determined and where they are characteristic and as permanent as the phenomena permit is thirty fathoms.

V. FIGURE OF THE BOTTOM OF THE SEA, BELOW THE GULF STREAM.

We have seen that in cross sections there is a great resemblance in the bottom of the sea off our coast to the region of land more removed from the coast-line in the interior. The top of the first range of hills, (see Diagram No. 9, Plate I,) is 1500 feet above the valley to the eastward of it, distant 12 miles; and the top of the second range 600 feet above the same valley, distant 15 miles. The first slope is 125 feet, and the second is 40 feet,

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to the mile. The bottom of the sea from the Tortugas section to that of Cape Florida, rises from 800 to 325 fathoms, and from the same point descends, in passing northward and eastward. The Cape Florida section showed that there then was present a ridge of comparatively cold water since the division into bands should apply along the stream as well as in the direction of its cross sections. The temperature of 40° is in fact reached on that section at 300 fathoms, and, as well as can be judged from the results in the separate sections there are divisions of this sort. The diagram No. 2, Plate II, shows where the curves of 50° and 45° are found upon the different sections and indicates a rise on the Charleston section and a sharp descent from Charleston to Cape Fear.

VII. General features of the Gulf Stream.

Upon the general diagram now presented to the members, (Plate III,) the general features of the Gulf Stream are represented from the Tortugas to the Cape Cod Section. Passing along the Cuban coast the temperature in June was found to be about 84° or 8° above the mean temperature of Key West, as given by the Surgeon General's report. The current here is feeble, but sufficient to cause it to be sought by sailing vessels making to windward and even by steamers. Issuing from the straits of Bemini, the stream is turned northward by the land which confines and directs its course. Its effective velocity is not derived from difference of temperature, as the observations abundantly show, the greatest relative differences being in fact crosswise of the stream. The direction is here a little west of north and the velocity is from 3 to 5 miles per hour. The temperature bands now begin. The bottom of the sea which was one slope and counter slope, across the Florida Straits, is here corrugated; the depth instead of being unfathomable, as has heretofore been supposed, is but 325 fathoms, in which depth the two currents, from the poles near the bottom and from the Gulf at the top, must pass each other. While the surface water is above 80° that near the bottom is as low as 40°.

The stream just north of Mosquito inlet begins to bend to the eastward of north, and off St. Augustine has a decided set to the eastward. While flowing thus onward the warm water seeks the sides of the channel overflowing towards the coast of Florida, and towards the Bahamas, but not as rapidly as it moves on north. Between St. Augustine and Cape Hatteras the set of the stream and the trend of the coast differ but little, making 5 degrees of easting in 5 degrees of northing. At Hatteras it curves to the northward and then runs easterly, making about 3 degrees of northing in 3 degrees of easting. In the latitude of Cape Charles it turns quite to the eastward having a velocity of between one and one mile and a half the hour.

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That this curve follows the general sweep of the coast under water, appears most probable, the coast line, the curve of 100 fathoms and the ranges of hills discovered by Lieuts. Maffitt and Craven all seem to indicate it. That the direction of the stream is given in a general way by the configuration of the bottom of the sea, it is hardly possible to doubt, while admitting that it receives modification from other, and perhaps more general, causes. The after progress of this mighty stream, and of its branches if it does divide, remains yet to be traced and and so also its heading in the Gulf of Mexico.

I forbear to mingle doubtful speculation upon causes, with the inductions in regard to temperatures, which it has been the object of these observations to supply and of this lecture to bring to your notice.

ART. XXIX.—*On Fermented and Aërated Bread, and their Comparative Dietetic Value;* by J. DAUGLISH, M. D.*

[Extracted from the London Medical Times and Gazette, vol. i, p. 468, 1860.]

SINCE the new process of preparing bread has been introduced,—a process which effects the raising of bread wholly by mechanical means, imparting to it the most perfect vesicular structure, while it leaves the constituents of the flour wholly unchan-

* As most of our readers are doubtless aware, Dr. Dauglish is the author of a new system of bread making that has excited considerable interest among chemists during the last twelvemonth.

An extended description of this method was read at the Aberdeen meeting of the British Association in Sept., 1859, by Dr. Odling, from whose paper we take the following extracts.

"It is well known that the vesicular character of ordinary bread results from the development of carbonic acid gas uniformly throughout a mass of fermenting dough, whereby a loose spongy texture is imparted to what would otherwise be a dense sodden lump of baked flour and water. In fermented bread the carbonic gas thus generated within the substance of the dough is a product of the transformation or degradation of one of the constituents of the flour, viz., of the starch or sugar.

"In the plan of Dr. Dauglish the carbonic acid is produced independently and superadded to the flour which consequently need not undergo any degradation whatever. Water charged with carbonic acid (common "soda water") is mixed under pressure with the flour and the resulting dough, which becomes vesicular when the pressure is removed, is divided into loaves and baked in the ordinary way.

"The advantages claimed for the new process, are, 1st. Its cleanliness. Instead of the dough being mixed with naked arms or feet, the bread, from the wetting of the flour to the completion of the baking is not, and scarcely can be touched by any one. 2d. Its rapidity. An hour and a half serves for the entire conversion of a sack of flour into baked loaves, whereas in the ordinary process, four or five hours are occupied in the formation of the sponge, and a further time for the kneading, raising, and baking of the dough. 3d. Its preventing deterioration of the flour. In making fermented bread from certain varieties of flour, not in themselves unwholsome, the prolonged action of warmth and moisture induces a change of the starchy matter of the flour into dextrine, whereby the bread becomes sodden and dark colored. This change is usually prevented by the addition of alum, which is indeed

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ged and uncontaminated,—there has not been wanting those who doubt whether the process of fermentation, by which bread has been hitherto prepared, is not really beneficial in other respects than that of imparting the vesicular structure to it; whether, in fact, the changes which the constituents of the flour —especially the starch—undergo, are not essential to healthy digestion in the stomach.

Although I believe there are few members of the Medical profession who will be prepared to maintain that fermentation is beneficial, still, as some do hold such an opinion, and have asserted likewise that starch which has not undergone the fermentive process is wholly unfit for human food, I am desirous of stating what I believe are good reasons for rejecting the process of fermentation for the new one which I have introduced.

In order to dispose of the assertion that starch requires to be prepared by the fermentive changes to render it fit for human food, it is but necessary to remark, that the proportion which the inhabitants of the earth, who thus prepare their starchy food, bear to those who do not, is quite insignificant. Indeed, it would appear that the practice of fermenting the flour or meal of the cereal grains is followed chiefly by those nations who use a mixed animal and vegetable diet, while those who are fed wholly on the products of the vegetable kingdom reject the process of fermentation entirely. Thus, the millions of India and China, who feed chiefly on rice, take it for the most part simply boiled; and that large portion of the human race who feed on maize, prepare

an almost necessary ingredient in the manufacture of bread from glucogenic flour. But in operating by the new process, there is no time for glucogenic change to take place, and consequently no advantage in the use of alum, with any description of flour. 4th. Its certainty and uniformity. Owing to differences in the character and rapidity of the fermentation, dependent on variations of temperature, quality of the yeast, &c., the manufacture of fermented bread frequently presents certain vagaries and irregularities from which the new process is entirely free. 5th. The character of the bread. Chemical analysis shows that the flour has undergone less deterioration in bread made by the new, than in that made by the fermented process. In other words, the percentage of extractive matters is smaller. The new bread has been tried dietetically at Guy's Hospital and by many London physicians and has been highly approved of. It is well known that for some years past, the use of fermented bread in dyspeptic cases, has been objected to by members of the medical profession, the debris of the yeast being considered unwholesome and liable to induce acidity. 6th. Its economy. The cost of carbonic acid is alleged to be less than the cost of yeast. Moreover, in making fermented bread there is a small but necessary waste of the saccharine constituents, which is avoided by the new process. 7th. The saving of labor and health. It substitutes machine labor for manual labor of a very exhausting kind. The sanitary condition of journeymen bakers was investigated some time ago by Dr. Guy, and found to be most lamentable, from their constant night work and from the fatiguing and unwholesome character of their labor, particularly the kneading In a politico-economical point of view, the process is important as removing bread-making from a domestic manual work, to a manufacturing, machine work."

From the character of the apparatus, the process can only be used profitably on a large scale, and not in small bakeries. F. H. A.

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it in many ways, but they never ferment it. The same is true with the potato-eater of Ireland, and the oatmeal-eater of Scotland. Nor do we find that even wheat is always subjected to fermentation; but the peculiar physical properties of this grain appear to have tasked man's ingenuity more than any other, to devise methods of preparing from it food which shall be both palatable and digestible. In the less civilized states, a favorite mode of dressing wheat grain has been, by first roasting and then grinding it. On the borders of the Mediterranean it is prepared in the form of maccaroni and vermicelli, while in the East it is made into hard thin cakes for the more delicate, and for the hard working and robust into thicker and more dense masses of baked flour and water. Even in our own nurseries wheaten flour is baked before it is prepared with milk for infants' food. The necessity of subjecting wheaten grain to these manipulations arises from its richness in gluten, and from the peculiar properties of that gluten. If a few wheaten grains are taken whole and thoroughly masticated, the starchy portion will be easily separated, mixed with the saliva and swallowed, whilst nearly the whole of the gluten will remain in the mouth in the form of a tough tenacious pellet, on which scarcely any impression can be made. A similar state of things will follow the mastication of flour. In this condition, the gluten is extremely indigestible, since it cannot be penetrated by the digestive solvents, and they can only act upon its small external surface; hence the necessity to prepare food from wheat in such a manner as shall counteract this tendency to cohere and form tenacious masses. This is the object of baking the grain and flour as before mentioned, of making it into maccaroni, and of raising it into soft spongy bread; by which latter means the gluten assumes a form somewhat analogous to the texture of the lungs, so that an enormous surface is secured for the action of the digestive juices; and this I believe is the sole object to be sought in the preparation of bread from wheaten flour.

Wheat is said to be the type of adult human food. It supplies, in just proportions, every element essential to the perfect nutrition of the human organism. And yet in practice we find that the food which we prepare from it, and furnish to the inhabitants of our large towns and cities, is quite incapable alone of sustaining the health and strength of any individual. This is the more remarkable, since in Scotland we find that the food prepared from the oat, a grain possessing the same elements of nutrition as wheat, though in a coarser form,—furnishes almost the exclusive diet of a very large number of the hardiest and finest portion of the population.

In the large towns of France wheaten bread certainly forms a very large proportion of the diet of the laboring classes, but not

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so large as oatmeal does in Scotland. And yet it has been remarked by contractors for public works on the Continent, that the chief reason why the Englishman is capable of accomplishing double the work of a Frenchman is, that the one consumes a very large proportion of meat, while the diet of the other is chiefly bread. In Scotland, however, the laboring man is capable of sustaining immense fatigue upon the nourishment afforded by oatmeal porridge.

The deficiency in wheaten bread in affording the nourishment due to the constituents of the grain, is to be attributed solely to the mode of preparing the flour, and the process followed for making that flour into porous bread.

The great object sought after both by the miller and the baker, is the production of a white and light loaf. Experience has taught the miller that the flour which makes the whitest loaf is obtained from the centre of the grain; but that the flour which is the most economical, and contains the largest portion of sound gluten, is that which is obtained from the external portion of the grain. But while he endeavors to secure both these portions for his flour, he takes the greatest care to avoid as much as possible, by fine dressing, etc., the mixture with them of any part of the true external coat which forms the bran, knowing that it will cause a most serious deficiency in the color of the bread after fermentation.

It is generally supposed that the dark color of brown bread —that is of bread made from the whole wheaten meal—is attributable to the colored particles of the husk or outer covering of the grain. But such is not really the case. The colored particles of the bran are of themselves only capable of imparting a somewhat orange color to bread, which is shown to be the fact when whole wheaten meal is made into bread by a process where no fermentation or any chemical changes whatever are allowed to take place. Some few years since, a process was invented in America for removing the outer seed coat of the wheat grain without injuring the grain itself, by which it was proposed to save that highly nutritious portion which is torn away, adhering to the bran in the ordinary process of grinding, and lost to human consumption. The invention was brought under the notice of the French Emperor, who caused some experiments to be made in one of the government bakeries to test its value. The experiments were perfectly satisfactory so far as the making of an extra quantity of white flour was concerned, but when this flour was subjected to the ordinary process of fermentation and made into bread, much to the astonishment of the parties conducting the experiments, and of the inventor himself, the bread was brown instead of white. The consequence, of course, has been that the invention has never been brought into practical operation.

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It has been estimated that as much as ten or twelve per cent of nutritious matter is separated adhering to the bran, which is torn away in the process of grinding, and until very lately this matter has been considered by chemists to be gluten. It has, however, been shown by M. Mège Mouriès to be chiefly a vegetable ferment, or metamorphic nitrogenous body, which he has named Cerealin, and another body, vegetable Caseine.

Cerealin is soluble in water, and insoluble in alcohol. It may be obtained by washing bran, as procured from the miller, with cold water, in which it dissolves, and it may be precipitated from the aqueous solution by means of alcohol; but, like pepsin, when thus precipitated it loses its activity as a solvent or ferment.

In its native state or in aqueous solution, it acts as the most energetic ferment on starch, dextrine, and glucose, producing the lactic and even the butyric changes, but not the alcoholic.

It acts remarkably on gluten, especially when in presence of starch, dextrine or glucose. The gluten is slightly decomposed at first, giving ammonia, a brown matter, and another production which causes the lactic acid change to take place in the starch and glucose. The lactic acid thus produced immediately combines its activity with that of the cerealin and the gluten is rapidly reduced to solution.

The activity of the cerealin is destroyed at a temperature of 140° Fah., according to M. Mouriès, but my own experiments show that it is simply suspended even by the heat required to cook bread thoroughly; thus bread made without fermentation, of whole wheaten meal, or of flour in which there is a large proportion of cerealin, will, if kept at a temperature of about 75° to 85° Fah., pass rapidly into a state of solution, if the smallest exciting cause be present, such as ptyaline or pepsin, or even that small amount of organic matter which is found in impure water—while the same material, when it has been subjected to the alcoholic fermentation, will not be affected in a like manner.

The activity of cerealin is very easily destroyed by most acids, also by the presence of alum; and while it is the most active agent known in producing the earlier changes in the constituents of the flour, it cannot produce the alcoholic, but as soon as the alcoholic is superinduced the cerealin becomes neutralized and ceases to act any longer as a solvent. M. Mouriès, taking advantage of this effect of alcoholic fermentation, has adopted a process by which he is enabled to separate from the bran all the cerealin and caseine which are attached to it. He subjects the bran to active alcoholic fermentation, which neutralizes the activity of the cerealin, and at the same time separates the nutritious matter; and then having strained this through a fine seive, he adds it to the white flour in the preparation of white bread, by which an economy of ten per cent is effected, and the color of the bread is not injured.

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The peculiar action of cerealin as a special digestive solvent of the constituents of the flour—gluten and starch—has been practically tested by Mr. Stephen Darby, of Leadenhall-street, in a series of careful experiments. He found that when two grains of dry cerealin were added to 500 grains of white flour, and the whole digested in half-an-ounce of water at a temperature of 90° for several hours, ten per cent more of the gluten, and about five per cent more of the starch, were dissolved than when the same quantity of flour was subjected to digestion without the addition of cerealin, but in which of course there was a small amount of cerealin that is present in all flours. The action of cerealin upon the gluten of wheat is precisely similar to that of pepsin on the fibrine of meat. Pepsin, acting alone on fibrine dissolves it, but very slowly, but if lactic acid be added solution takes place very rapidly. In like manner the starch present with the gluten of wheat is acted upon by the cerealin, and produces the necessary lactic acid to assist in the solution of the gluten by cerealin.

With the knowledge thus obtained of the properties of this substance cerealin, it is not difficult to understand why the administration of bran-tea with the food of badly-nourished children, produces the remarkable results attributed to it by men both experienced and eminent in the Medical profession; and why, also, bread made from whole wheaten meal, which contains all the cerealin of the grain, should prove so beneficial in some forms of mal-assimilation, notwithstanding the presence of the peculiarly indigestible and irritating substance forming the outer covering of the grain.

It will be seen that in all the methods of bread-making hitherto adopted, the peculiar solvent properties of this body, cerealin, have been sought to be neutralized simply because it destroys the white color of the bread during the early stages of panary fermentation. It is by thus destroying the activity of the special digestive ferment which Nature has supplied for the due assimilation by the economy of the constituents of the wheaten grain, that wheaten bread is rendered incapable of affording that sustenance to the laboring man which the Scotchman obtains from his oatmeal porridge. Although the new bread has been as yet but little more than experimentally introduced to public consumption, I have already received from members of my own profession, who have recommended it in their practice, as well as from non-professional persons, accounts of the really astonishing results that have followed its use in cases of deranged digestion and assimilation. Private gentleman have sought interviews with me to record the history of their recovery to health, after years of suffering and misery, by the simple use of the bread as a diet. Children that have been liable to convulsive attacks from

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an irritable condition of the alimentary canal and nervous system, have been perfectly free from them immediately the new bread was substituted for fermented bread. And cases are now numerous that have been communicated to me by medical men of position, in which certain distressing forms of dyspepsia, which had remained intractable under every kind of treatment, have yielded as if by magic almost immediately after adopting the use of the aërated bread.

The delicate flavor of the new bread renders it peculiarly grateful to the stomachs of invalids and children, as well as of those whose tastes have not become vitiated by the habitual use of baker's bread, which is slightly sour, and tastes of yeast. The new bread was supplied to two wards in Guy's Hospital in place of the ordinary bread (which is of a very fine quality, made on the premises,) for two months, and in no case were there any pieces left in the wards unconsumed, while of the fermented bread large quantities of scraps were collected daily, for the consumption of which the appetites of the patients have been deficient.

That persons who have been long used to the strong yeasty-flavored bakers' bread should consider the new bread tasteless at first is not to be wondered at, since the delicate sense of taste is of all other senses the most easily lost by rough usage. Hence the argument put forth in defence of adulteration by some London tradesmen, especially the beer sellers, that the public will not buy the pure article, as it is wanting in the flavor to which they have been accustomed; and hence, also, the dislike of the Viennese of the fresh oysters supplied to them when the railway was completed, as they deemed them insipid, after the habitual use of oysters slightly decomposed, with which they had been supplied when it required a lengthened period to transport them from the sea.

I am disposed to attribute the beneficial effects of the new bread to two causes. The one to the *absence* of the prejudicial matters imparted to ordinary bread by the process of fermentation, and the other to the *presence* in the bread, unchanged, of that most essential agent of digestion and assimilation, cerealin.

I believe the prejudicial matters imparted to bread by fermentation to be chiefly two—acetic acid and the yeast-plant. The first is produced in large quantities, especially in hot weather, by the oxydation, by atmospheric contact, of the alcohol produced. The second is added when the baker forms his sponge, and is also rapidly propagated during the alcoholic fermentation, and cannot of course be afterwards separated from the other materials in the manner that the yeast and the other *débris* of fermentation separate themselves from wine and beer by precipitation in the process of fining. Nor is the life of the yeast-plant gene-

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rally destroyed in baking, because it requires to be retained at the boiling point for some time before it is thoroughly destroyed; and bread is generally withdrawn from the oven, for economical reasons, even before the centre of the loaf has reached the temperature of 212°. It is not difficult to understand how the most painful and distressing symptoms and derangements may follow the use of bread in which the yeast-plant is not thoroughly destroyed previous to ingestion, and in those cases of impaired function in which the peculiar antiseptic influence of the stomachal secretions is deficient, and is incapable of preventing the development of the yeast-plant in the stomach, and the setting up of the alcoholic fermentation to derange the whole process of digestion and assimilation.

The presence of cerealin in bread is as beneficial as that of acetic acid and the yeast-plant is prejudicial. Digestion, or the reduction of food is evidently essentially dependent on the action of a class of substances which chemists, for want of a better term, have called ferments—to these substances belong pepsin, ptyaline, emulsin, diastase and cerealin; these are evidently types of a very numerous class, which act by producing those molecular changes in organic substances in which digestion consists: and since the purpose of digestion or solution is to prepare from heterogeneous substances taken as food a chyle, which shall not only when absorbed present all the elements of healthy blood, but shall previous to absorption, possess the properties which will constitute it the proper stimulus to the functional activity of the lactaels, it would appear to be necessary that each distinct substance taken as food should be furnished, not with its simple chemical solvent, but with that peculiar form of solvent or ferment which alone can carry it through those molecular changes which shall terminate in the production of healthy chyle. Hence we should infer that a substance was digestible or indigestible just in proportion to the provision that is made for its reduction to the standard of healthy chyle, and that substances which have hitherto been incapable of affording any nutrition whatever, may at some future day be rendered highly nutritious, simply by adding to them suitable ferments, artificially obtained or otherwise, that shall secure their passage through the proper molecular changes. Indeed, I think this subject opens up to us that very wide field of inquiry, as to whether the cause and prevention of disease, and the beneficial administration of remedies may not, for the most part, if not entirely, be dependent on the action of substances analogous to such bodies as ptyaline, pepsin, cerealin, etc., acting in concord with, or retarding and opposing the vital functions of tissues; and that by more profound inquiry in this field of research, the physiologist and the pathologist may not at a future day lay the foundation of true scientific Medicine.

Tunbridge Wells.

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Art. XXX.—*Additional Note on the Potsdam Fossils;* by E. Billings.

Since my note to Mr. Bradley's paper was written, he has collected quite a number of new specimens of *C. minutus* at the same locality. At his request I have examined them and find that they exhibit several of the parts not preserved in those upon which the original description was founded.

Fig. 4, (nat. size).



Fig. 4.— *a*, A detached cheek showing the small spine of the posterior angle.
b c, Two specimens of the glabella, showing the spine on the neck segment.

1. The posterior angles of the head are produced into short spines, as we supposed, but these spines, instead of being elongate-triangular are sub-cylindrical or needle-shaped and projected outwards at an angle of 45° or thereabouts, to the longitudinal axis of the body. The cheek does not appear to be striated but rather smooth. These two characters furnish additional grounds for separating the species from *C. antiquatus* (Salter), which has the cheeks striated and the posterior angles of the head only slightly produced into short broadly triangular terminations.

2. The neck segment bears a short broad-based spine. The first specimens collected do not exhibit this, but on reëxamining them I think I can see traces of it. Some of the specimens of *C. coronatus* (Barrande) lately collected in the Primordial Zone of Spain have a spine on the neck segment of the same form as that of *C. minutus*, while others (according to the figures) have not; and it may be that individuals of our species will yet be discovered in which the absence of the spine can be clearly established. This remark is made here because on comparing the figures of the Bohemian and Spanish specimens of *C. coronatus* it would appear that the presence or absence of a spine on the neck segment is not always of specific importance and should some of those from Keeseville turn out to have only a plain neck segment we would not perhaps on that ground alone be authorized to constitute two species.*

* Compare the article, *Sur l'existence de la faune primordiale dans la chaine cantabrique*, par M. Casiano de Prado; suivie de la *Description des fossiles*, par MM. de Verneuil et Barrande. Bulletin Geol. Soc. France, 2e Series, vol. xvii, p. 516, (1860). And also Barrande's *Système Silurien*, plate 13.

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3. Mr. Bradley's new specimens also show that there are three pairs of glabellar furrows, the anterior being represented by two small indentations just in advance of the points where the ocular ridges reach the glabella; and further that the course of the facial suture is the same as it is in *C. striatus*, (Emerich). The pygidium is more obtusely rounded than is represented in our Figure 2.

As to the correctness of the generic reference of this species it may be remarked that Barrande is of opinion that no less than eleven of those which Angelin has figured under the genera *Solenopleura*, *Eryx*, *Conocoryphe* and *Harpides* should be placed in *Conocephalites*. In this view of Barrande's, Angelin has concurred.* The genus has thus been greatly extended and judging from the form of the head (and more particularly of the glabella) of Angelin's species *C. brachymetopus*, *C. homelotopus* and *C. canaliculatus* I think we are perfectly justified in referring this species to *Conocephalites*. The genus is most closely allied to *Calymene*, having the same number of segments in the thorax —the same number and arrangement of pieces in the head and the same general form and lobation of the glabella, the differences between the genera consisting principally in certain characters of the pleuræ and hypostoma† to which may be added the ocular ridge which although not a constant character in *Conocephalites* may be regarded as of some generic value as it does not occur at all in *Calymene*. I would also state that since examining Mr. Bradley's recent collection, I have been strongly impressed by the resemblance between the form of the cheek and small needle-shaped posterior spines of *C. minutus* and the same parts of the head of the Quebec species which I have called *Menocephalus globosus*, and it appears to me that *Menocephalus* must be regarded as another closely allied genus. If we except those two genera, *Calymene* and *Menocephalus*, there is no other but *Conocephalites* to which our new trilobite bears any near affinity.

* See Barrande's "*Parallèle entre les dépôts Siluriens de Bohême et de Scandinavie*, p. 19; and compare the tables on p. 17 and 35 of the same work. See also Angelin's *Palæontologia Scandinavica*.

† See Barrande, "*Système Silurien du centre de la Bohême*," p. 417–419.

Montreal, 20th Sept., 1860.

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ART. XXXI.—*The Great Auroral Exhibition of Aug. 28th to Sept. 4th*, 1859.—6TH ARTICLE; by Prof. ELIAS LOOMIS.

SINCE our last Auroral article was prepared for the press, the following letter has been received from the Secretary of the Smithsonian Institution.

Washington, June 6th, 1860.

Dear Sir:—Some time since, you wrote us in regard to the aurora of September, 1859, and I now write to inform you that we have a very large collection of materials in regard to this interesting meteor which in justice to the writers ought to be published. It is, however, a pity that the data for scientific deductions in regard to this interesting phenomenon should be scattered, and we will therefore present the whole to Silliman's Journal, provided the Editors will publish it. Will you take charge of it, and prepare it for the press?

JOSEPH HENRY, *Secretary S. I.*

The editors of this Journal have accepted the liberal proposition of Prof. Henry, so far as to publish whatever might be supposed to be of importance in an investigation of the theory of the Aurora. A considerable portion of the materials collected by the Smithsonian Institution have already appeared in former numbers of this Journal, and many of the reports are from neighboring stations where the Auroral appearances are almost identically the same.. From the entire mass of materials we have aimed to select those Reports only which were the most complete and elaborate, and which were so distributed in geographical position as to afford a correct idea of the appearances throughout the entire area of the United States.

OBSERVATIONS OF THE AURORA OF AUGUST 28TH, 1859.

Selected from the Smithsonian Papers.

1. *Observations at Burlington, Minnesota*, (lat. 47° 1′, long. 92° 30′), *by* A. A. HIBBARD.

The aurora of Aug. 28th, commenced at 8 P. M., and increased very rapidly until 8½ P. M., when it came to a centre directly over our heads. It went about three-fourths the circumference of the horizon at the base, and completely round at the top, and down about one-third on the south side. Stars very bright and light enough to read very fine print. At 9 P. M. it had nearly disappeared over head; and but few beams to be seen in any part of the horizon, although new beams were forming in the north. At 9½ P. M. the beams were very light, and somewhat scattered. At 10 P. M. it had entirely disappeared.

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2. *Observations at Marquette, Michigan*, (lat. 46° 32′, long. 87° 41′), *by* Dr. G. H. BLAKER.

Aug. 28th, at 8 P. M., a bright crown overhead, with beams or streamers extending to every point of the compass, but soon lost in a white haze. In the course of an hour these streamers extended to the horizon in every direction, with bright streams of white light shooting up towards the crown, all of which became perfectly red, or of a bright crimson color over head. This continued to increase in brightness until after midnight, with floods of white light at the horizon, all passing into crimson fleecy vapor in the zenith.

3. *Observations at Winona, Minnesota*, (lat. 44° 3′, long. 91° 36′), *by* T. F. THICKSTUN.

Aug. 28th, an auroral bank and a few pencilled streamers had formed at 9½ P. M. At midnight the streamers and corona filled the whole heavens except the N. E. portion. During the whole night the light was equal to that of a half moon.

4. *Observations at Green Bay, Wisconsin*, (lat. 44° 30′, long. 87° 56′), *by* D. UNDERWOOD.

Aug. 28th, about 7½ P. M., the aurora was visible in the northern part of the heavens, but did not attract particular notice until about 9 P. M. Soon after eight the sky began to redden, and became nearly of a blood-red color. Soon the streaks were observed shooting upward from all points of the horizon, and concentrating in a large luminous mass in mid-heavens. The greatest intensity of color was at the zenith. Rays were constantly shooting up from all points of the horizon and the colors constantly changing. The rays emitted an intense red light for about half an hour, when they began slowly to fade away in the north and south, but in the east and west they continued to glow until 10 P. M., when they began to fade away. Flashes of white light appeared among them, commencing from the horizon and moving upwards, following each other in rapid succession like the waves of an immense sea of light. They grew brighter as the red color disappeared, and when this was wholly gone they also gradually faded away.

5. *Observations at Milwaukee, Wisconsin*, (lat. 43° 3′, long. 87° 57′), *by* Prof. E. P. LARKIN.

Aug. 28th, at 8 P. M., an aurora commenced. About 8½ P. M. an arch formed from S.W. by the north to the S.E., with dark broken clouds below. The streamers now commenced, principally in the N.W. and N.E., and were surprisingly beautiful, of crimson, purple, peach and orange; the crimson predominating. At $8^h\ 45^m$ a perfect corona formed a few degrees south of the zenith.

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At 9 P. M. the aurora began to fade, and at ten had nearly disappeared. At 10½ P. M. the north gave indications of another aurora which occurred about 12 o'clock, nearly equal in splendor to the first, and still another occurred about 3 A. M. There were also auroras late in the night of the 30th and also of the 31st.

6. *Observations at Burlington, Wisconsin*, (lat. 42° 39′, long. 87° 44′), *by* D. MATHEWS.

Aug. 28th, at 8 P. M., the appearance was that of a large luminous ring surrounding the zenith; but this form was very transient, the light becoming concentrated in the west. Between 8 and 9 P. M. there were two arches formed in the north, the first almost 30° in altitude, and the second about 40°. From the outer edge of the larger arch darted a succession of streamers or rays of light. At $8^h\ 15^m$ a perfect flood of light came up in the east, not in streams, but like the dawn of day, just before sunrise. This appearance lasted about half an hour. At 9 P. M. streams of light radiated in every direction from a point about ten degrees south of the zenith, covering the whole heavens except a space in the south. One broad belt of red light extended from near the zenith to the horizon at a point a little north of west.

7. *Observations at Dubuque, Iowa*, (lat. 42° 30′, long. 90° 52′), *by* ASA HORR, M.D.

Aug. 28th, the aurora began with floating irregular masses of auroral clouds in the north, which soon spread over the sky, terminating in a broad zone of light spanning the heavens from E. to W. and reaching to 20° south of the zenith. At 8 P. M. many of the luminous clouds became distinctly crimson, with the deepest hue near the horizon. At $8^h\ 15^m$ a distinct arch formed in the north crowned with tall flickering white streamers. The crimson clouds now dissolved into paler streamers; at $8^h\ 35^m$ the streamers subsided; and at $8^h\ 40^m$ the red clouds near the horizon and the diffuse light in the north appeared again. From this time till 9 P. M. a crimson shade was spread over the whole of the broad belt, with varying hue and brightness. The arch in the north formed again with whitish streamers which remained moderately bright until 11 P. M.

8. *Observations at Waltham, Massachusetts*, (lat. 42° 24′, long. 71° 14′), *by* Rev. THOMAS HILL.

Aug. 28th, at 7½ P. M., there were visible some splendid masses of rose-colored light in the east and west near the horizon; that in the west being nearly obscured by twilight. At $7^h\ 45^m$ a well defined arch passed south of the zenith, and all the sky north of it was filled with light, radiating toward the pole of the

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dipping needle. I watched the aurora from 9 to 10½ P. M., and at 3 A. M. got up again to look. It was then very brilliant and rosy all along the southern horizon.

9. *Observations at Monroe, Michigan*, (lat. 41° 56′, long. 83° 22′), *by* Miss HELEN J. WHELPLEY.

Aug. 28th, about 8½ P. M., there was a broad line of intensely yellow light extending from east to west. In a few moments the whole sky to within 20° of the south, seemed to be permeated with a clear whitish light; yellowish rays constantly shooting up from the east, north and west horizon to the zenith. At the same time appeared a bright rose colored mass in the east, which gradually enlarged until it covered nearly three-fourths of the heavens, and about 9 P. M. the rays met and formed a perfect corona a little south of the zenith.

10. *Observations at Willow Creek, Illinois*, (lat. 41° 45′, long. 88° 56′), *by* E. E. BACON.

Aug. 28th—aurora first seen at 8 P. M., corona and beams at $8^h\ 40^m$, arch $8^h\ 45^m$. Beams of red very brilliant in the east and west, and at the corona from $8^h\ 35^m$ to $8^h\ 55^m$. At 9^h the beams and corona had disappeared, and a broad red belt extended across the heavens, passing over to the south. Two distinct arches were formed in the north. At $9^h\ 15^m$ bright beams in the southeast, the red belt disappearing in the east. At $9^h\ 25^m$ redness nearly gone. At $9^h\ 28^m$ arches broken. At $9^h\ 35^m$ brilliant spot in N.E. At $9^h\ 40^m$ arch reformed, but not so brilliant. At $12^h\ 30^m$ beams with far greater grandeur than at $8^h\ 40^m$. Beams streamed from all round the horizon to the zenith. In the southern half of the sky, the beams flashed like the blaze of a great fire. At $12^h\ 45^m$ a bright belt from E. to W., in the south, with a dark belt something like a cloud under it. At $1^h\ 12^m$ bright belt in the south gone. Aurora lasted till daylight.

11. *Observations at Sandwich, Illinois*, (lat. 41° 31′, long. 88° 30′), *by* Dr. N. E. BALLOU.

Aug. 28th, at 7½ P. M., there was a bright luminous band, a degree in width, spanning the heavens. Soon it became tortuous; and in the zenith it pointed southward half way to the horizon. At 8 P. M. red gleams shot up in the N.W. directly to the zenith. At 8½ P. M. the same appearance sprung up in the N.E. At 9 P. M. the entire northern half of the sky was brilliantly red, with gleams which soon culminated in a corona. At 9½ P. M. the whole was tinged with red, alternating with beams of light. At 10 P. M. the red tinge floated away to the south. At midnight it presented much the same appearance as before.

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12. *Observations at Sag Harbor, New York*, (lat. 41° 0′, long. 72° 20′), *from the Sag Harbor Express.*

Aug. 28th—between 8 and 9 o'clock rays of clear, white light arose from every part of the horizon, and soon met in a common focus at a point about 15° S.E. of the zenith, amid the four stars forming the trapezium of the Dolphin. At 10 P. M. a more dense light appeared at two points on the horizon a little north of east and west. These were at first of a bright yellow light; but at 10½ P. M. that in the east assumed a dark crimson hue, and the light in the west was tinged with a paler red. At this time began the waves of undulating light, their motions being upward and across the radial columns in a direction from east to west.

At midnight the brightness of the columns increased, the undulations increased, especially in the south, and their motions became exceedingly rapid. The usual dark bank now appeared upon the horizon in the south, well defined, but occasionally broken by minor columns of light. The whole heavens continued thus glowing until the aurora was eclipsed by the light of day.

13. *Observations at Kanosha, Nebraska*, (lat. 40° 51′, long. 95° 44′), by Bela White.

There was a very brilliant light of a pink color from about 1 A. M. Aug. 29th until daylight in the northern half of the hemisphere, shooting upwards to the zenith, and passing off to the south. It was so light as to enable a person to read coarse print.

14. *Observations at Great Salt Lake City, Utah*, (lat. 40° 45′, long. 111° 26′), *by* W. W. Phelps.

Aug. 28th between 9 and 10 P. M. a palish light wavered up about 30° towards the zenith; thence it spread east and west with increasing grandeur, till about 11 P. M. when the perpendicular streams beautified a large portion of the northern hemisphere. The light about 20° above the horizon, decreased upward and downward, so that at nearly midway to the zenith, the light changed into a fine yellow green, which was joined by a rich livery of crimson, spanning the whole heavens from east to west, as a belt several degrees wide. This magnificent phenomenon continued in varying hues, until about 1 A. M.

15. *Observations at New York City*, (lat. 40° 43′, long. 74° 5′), *by* Prof. O. W. Morris.

Aug. 28, 8 P. M. there was a bright band above the horizon, which spread upwards with rays. At $9^h\ 10^m$ the light spread over the heavens, occupying the zenith and either side with white, and interspersed with pink colored portions. At $9^h\ 20^m$ it formed an arch in the south with a dark segment below, while the northern portion of the sky was perfectly clear. At $9^h\ 25^m$

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the beams shot up from all sides to the zenith, forming a beautiful corona which lasted 5 or 8 minutes, sometimes putting on the appearance of conflicting waves. Nearly the whole sky was of a pink, and some portions of a dark red color. It faded gradually away, first disappearing in the south, and at 10 P. M. only a bright band across the northern portion without a dark segment. About $10^h\ 30^m$ occasional rays shot up to the zenith. At 11^h it was very light, so that objects could be seen at a distance. The aurora continued with varied colors and brightness until the dawn of the 29th.

16. *Observations at Pekin, Illinois*, (lat. 40° 36′, long. 89° 45′), *by* J. H. Riblet.

Aug. 28, $8^h\ 20^m$ P. M. a white band running from N. W. to E. with two columns shooting up, one by the north star the other through Ursa Major. $8^h\ 30^m$ it is passing westward and a high column is passing about 5° east of the north star, and about 10° above it. At $8^h\ 40^m$ two columns passing nearly to zenith on the east side of north star. At $8^h\ 46^m$ the column by the north star increased in width, the top bent over forming a semicircle to west. Color white in north, changing to a rose at its upper edge, and a red in the east. At $8^h\ 55^m$ arches forming; the lowest about 10° above the horizon, and of a pale color. The second about 30° above the horizon, and of a pale orange color. Streamers running from the lower arch through it and about 30° above it. The color was most intense in the east. At $8^h\ 57^m$ both arches better defined. The streamers passing from the lower through the upper to a point about 10° west of the zenith. At 9 P. M. less brilliant and ceased observing.

17. *Observations at Urbana, Ohio*, (lat. 40° 6′, long. 83° 43′), *by* Prof. M. G. Williams.

Aug. 28th, at 9 P. M. columns of white and yellowish light shot from all points of the arch which extended from N. 80° E. to N. 75° W. Many of the corruscations passed beyond the zenith; in the east the light was pink and deep crimson forming a mass about 30° broad and 60° high reaching down to the horizon. The color was sometimes almost blood red. At $9^h\ 10^m$ a similar mass formed in the N.W. At $9^h\ 15^m$ a remarkably beautiful column shot up at N. 50° W. having a breadth of 10° and reaching to the zenith. The colors were white, yellowish, pink and crimson. About $9^h\ 20^m$ an arch was formed in the south, having an altitude of 40° at the centre. A few minutes later, the crimson light extended down from the zenith, quite to this arch, so that most of the sky was covered with colored light. At $9^h\ 50^m$, a beam 2° broad, shot up from S. 80° W. passing 20° or 30° beyond the zenith. In a few minutes the beam seemed to be broken up into fragments of 5° or 6° in length, and presently

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vanished. At 10 P. M. the brightness was much diminished; at $10^h\ 15^m$ revived; the northern arch very brilliant and slightly tinged with crimson. At $10^h\ 30^m$ the arch extended from E. to N. 85° W. At midnight the exhibition was still fine. From $2\frac{1}{2}$ to 3 A. M coruscations of white light shot up all along the horizon from E. to S. 15° W. The aurora continued to decline, till the dawn of day.

18. *Observations at Henry Co., Indiana*, (lat. 40°, long. 85° 15′), *by* WILLIAM DAWSON.

Aug. 28th, about 9 P. M. a red cloud covered a large portion of the eastern sky with a similar one in the N.W. and several large luminous beams extended from the north point to the zenith. Soon the red disappeared, and the bright streaks grew much shorter, leaving a bright cloud brilliantly fringed with white, near the northern horizon. After some minutes, several small bodies, like long white clouds, started from about 25° S.E. of the zenith, and moved slowly towards the west, disappearing about the same distance nearly S.W. of the zenith. About midnight, a dark cloud decked with immense streamers of white, glaring light, rested on the northern horizon, when suddenly it burst forth into streaming corruscations of red, purple and white lights, shooting to a point 15° or 20° south of the zenith, where these flashing lights presented the appearance of a cloud, tinted with vermillion and purple. At $12\frac{1}{2}$ A. M. fully two-thirds of the heavens were wrapt in flashing torrents of streamers directed towards this point. This tremulous illumination lasted about an hour, when it partly vanished; but soon the sky was again covered with darting streamers nearly as before, and the light seemed more vivid than at $12\frac{1}{2}$ A. M. At 2 A. M. the light had in a great measure passed away.

19. *Observations at Gettysburg, Pennsylvania*, (lat. 39° 49′, long. 77° 15′), *by* Rev. M. JACOBS.

Aug. 28th, the aurora was visible in the evening twilight especially to N. and N.E. At 9 P. M. the whole sky as far south as within 30° of the southern horizon was covered with alternate bands of luminous matter like cirrus cloud. At $9^h\ 25^m$ the luminous band within about 20° of the northern horizon began to shoot up streamers, and soon became a mass of streamers which filled the whole northern sky, darting up to a point about 2° east of Aquila, where they crossed, some radiating thence southward. The streams were visibly wafted round on the east and west, to the S.E., S.W. and even south. The streamers were mostly of orange, white below and crimson red above. At 10 P. M. the sky was mostly clear; the bank of vapor, dark below but luminous above, with a few streamers occupied the northern horizon, rising to the height of 20° or 25°. The light was equal to that of the moon at quadrature.

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20. *Observations at College Hill, Ohio*, (lat. 39° 19′, long. 84° 26′), *by* Prof. J. H. WILSON.

Aug. 28th, the aurora commenced at 8 P. M. and continued with increased splendor till 1 A. M. The corona formed about 10 P. M. with arch and beams. At 1 A. M. the coruscations darted from the horizon upward to the zenith and about 20° beyond. The light was equal to the clearest full moon.

21. *Observations at Wyandott City, Kansas*, (lat. 39° 7′, long. 94° 44′), *by* JOHN H. MILLAR.

Aug. 28th, at 11½ P. M. a diffused light was observed in the N.N.E. gradually increasing until at 1 A. M. the whole northern sky from N.W. by W. to S.E. by E. was covered with rays and streamers of moderate brightness, shooting up to within 30° of the zenith, and changing rapidly, from a uniform white to a tinge of purple. The aurora passed off about 1½ A. M.

22. *Observations at St. Louis, Missouri*, (lat. 38° 37′, long. 90° 15′), *by* A. FENDLER.

Aug. 28th, at $8^h\ 30^m$ P. M. I observed in the northern part of the sky some large patches of a deep red color, and the horizon towards the north was filled with a white light. The white as well as the red light rose gradually up to 45°. At $8^h\ 35^m$ the upper part of the light to the right was of an unusually deep red, while that to the left was greenish white. The aurora increased in splendor till $8^h\ 50^m$, rising to the zenith, with red streamers running nearly N. and S. The aurora now declined in the north, but spread its red color to about 10° south of the zenith. By 9 P. M. the red color was gone, only a faint white light remaining near the northern horizon, which continued till $9^h\ 20^m$ when it became obscured by clouds.

Aug. 29th at 1 A. M. I awoke and found the aurora more brilliant than last evening. It reached 45° south of the zenith, and had a corona 15° or 20° south of the zenith, of a deep fiery red, sending rays in all directions from this point towards the horizon. Towards the north the light was white, and close to the northern horizon the sky was blue. By $1^h\ 20^m$ the aurora had retired to within 80° above the northern horizon.

23. *Observations at Fredericksburg, Virginia*, (lat. 38° 30′ long. 77° 30′), *by* CHARLES H. ROBEY.

Aug. 28th, a most brilliant light appeared 8 P. M. and disappeared about $4^h\ 15^m$ A. M. It was generally diffused over the face of the heavens with a brightness exceeding that of the full moon, the brightest part being N.E. and N.W. About N.E. it was of a red color.

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24. *Observations at the eastern base of the Sierra Abajo, Utah*, (lat. 38°, long. 110°), *by* Dr. JOHN S. NEWBERRY.

Aug. 28th—being at an elevation of about 7000 feet above the sea, from 11 to 12 o'clock the aurora appeared remarkably brilliant, the entire northern heavens being covered with a diffused red flush, with flashes of deeper red and white light. The auroral flush was noticeable in the north before 10 o'clock, but was not conspicuous before 11.

25. *Observations at Santa Clara, California*, (lat. 37° 18′, long. 122° 0′), *by* OLIVER S. FRAMBES.

Aug. 28th, at 9 P. M., about 10° E. of north the sky seemed tinged with red light. In half an hour several columns of mellow light were formed which rose to a height of 40°, and the colors became very bright. The light gradually moved to the east, and after two hours or more gradually faded away.

26. *Observations at Paducah, Kentucky*, (lat. 37° 5′, long. 87° 21′), *by* A. MATTISON.

Aug. 28th, at midnight, the clouds cleared off and showed the most beautiful aurora I ever saw in this latitude. Sometimes it was red, and sometimes it sent up streamers overhead. Aug. 29th, 1 A. M., aurora very bright, but the sky became overcast. The light continued till near day break.

27. *Observations at Monterey, California*, (lat. 36° 36′, long. 122° 54′), *by* Dr. C. A. CANFIELD.

Aug. 28th—a very brilliant aurora from 9½ P. M. to 11 or 12 P. M.

28. *Observations at Raleigh, North Carolina*, (lat. 35° 40′, long. 78° 52′) *by* WILLIAM H. HAMILTON.

Aurora appeared at 9 P. M. and lasted till 11 P. M. As light as if the moon was shining.

29. *Observations at Dallas, Texas*, (lat. 32° 45′, long. 96° 46′), *by* JOHN M. CROCKETT.

Aug. 28th, at dark the northern sky had the appearance of bright twilight. It continued to brighten until it extended from N. to N. E., and assumed a red tinge, with columns perpendicular to the horizon, and extending more than half way up the heavens. About the time of the greatest illumination, a round body in the N. E., about 15° in diameter, became of a beautiful bright scarlet, which in 20 or 30 minutes moved slowly towards the north, displacing the columns as it went. The whole scene occupied about one and a half hours and was constantly changing. The aurora continued with diminished brightness till near daylight.

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30. *Observations at Selma, Alabama*, (lat. 32° 25′, long. 86° 51′), *by* S. K. Jennings, M.D.

Aug. 28th, about 8 P. M., there was a very well defined arch of red fleecy looking clouds extending up about 20°, that was beautifully brilliant for half an hour. From 8½ till after 10 P. M. there were incessant flashes that might have been taken for diffuse lightning.

31. *Observations at Cahawba, Alabama*, (lat. 32° 19′, long. 87° 16′), *by* Matthew Troy, M.D.

Aug. 28th, from 8 to 9 P. M., a bright light was visible a little east of north. It was brightest near the horizon, and extended to a height of about 30°, gradually fading at its upper border.

32. *Observations at Jacksonville, Florida*, (lat. 30° 15′, long. 82° 0′), *by* Dr. A. J. Baldwin.

Aug. 28th, about 8 P. M. was seen a remarkable aurora which continued until 2 A. M. Aug. 29th. At times it was of a vivid red, with streamers radiating towards the zenith. The brightest were from N.W. to N.E. The color would almost fade out at times, and then lighten up the heavens again with a brilliancy which was majestic. About 9 o'clock there was a dark cloud in the north extending from N.W. to N.E.; and the auroral display was beautiful along the fringe of this cloud.

33. *Observations at Micanopy, Florida*, (lat. 29° 35′, long. 82° 18′), *by* James B. Bean.

Aug. 28, just after dark I noticed a luminous appearance in the north, which at times disappeared and then reappeared with increased brightness, till 9 o'clock, when it exhibited streamers shooting up toward the zenith, and sometimes a deep red glare of rosy light toward the N.E. At 10 P. M. the streamers were quite distinct, presenting beams of gray and purple light. It disappeared about 11 P. M. but reappeared with more beauty between 1½ and 2 A. M. The streamers were very distinct, exhibiting various colors, and shooting at times within 10° or 15° of the zenith. The luminous haze in the N.W. continued till it was obscured by day-light.

34. *Observations at Cedar Keys, Florida*, (lat. 29° 7′, long. 83° 2′), *by* Judge Aug. Steele.

Aug. 28th, occurred an aurora, brightening up the northern horizon with most beautiful coruscations. It extended from N. to N.E. and upward to about 30° above the horizon, and exhibited frequent streaks of reddish yellow, shooting upward in pointed forms of the most dazzling brightness. It ceased about 9½ P. M. but reappeared the next morning in still greater brilliancy and continued until overpowered by the light of day.

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35. *Observations at Corpus Christi, Texas*, (lat. 27° 45′, long. 97° 30′), *by* A. M. Lea.

Aug. 28th, about 9 P. M. an aurora reddened the sky in the north, through about 90° of the horizon, and rising about 40° above it, with columns of light stretching from the horizon towards the zenith.

36. *Observations at Key West, Florida*, (lat. 24° 33′, long. 81° 48′), *by* William C. Dennis.

Aug. 28th the aurora was faintly visible soon after sun-down and did not increase materially in brightness till 8½ P. M. At 9 P. M. the color was of the most fiery red. The direction of the middle point of the aurora was N. 10° E. and both horizontally and vertically it subtended an angle of about 30°. There was a narrow line of immovable clouds along the northern horizon in the direction of the aurora. At 9 P. M. the aurora commenced fading rapidly, and had disappeared at 9½ P. M.

Observations of the Aurora of Sept. 1st and Sept. 2nd, 1859.

Selected from the Smithsonian Papers.

1. *Observations at St. Johns, Newfoundland*, (lat. 47° 35′, long. 52° 38′), *by* E. M. J. Delaney.

Sept. 1st, an Aurora of various colors appeared in the west, moved towards the zenith and disappeared.

Sept. 2d at 8 P. M. an Aurora of various colors appeared in the north, south and west, moved towards the zenith and disappeared.

2. *Observations at Burlington, Minnesota*, (lat. 47° 1′, long. 92° 30′), *by* A. H. Hibbard.

Sept. 2d, Aurora commenced at 8 P. M. in the north, and N.E.; increased very fast until 8½ P. M. when it formed a perfect arch frrom N.E. to S.W. and ran about one-third of the way down from the zenith on the south side. The east and west parts were very red; the east part flashy like lightning. At 9 P. M. nearly disappeared. At 9¼ P. M. commenced in the north with very bright and flashy light; then very bright streamers ran up instantly almost to the zenith. At 9½ P. M. nearly disappeared; only bright flashy spots E. and N.W.

Sept. 3d, Aurora commenced about 8½ P. M.; beams ran up very rapid but very dim; formed perfect at about 9½ P. M.

3. *Observations at Princeton, Minnesota*, (lat. 45° 50′, long. 93° 45′), *by* O. E. Garrison.

Sept. 2d, a bright aurora extending from the N.W. to the E. and culminating in the zenith. It was a bright display of streamers in bands varying from a bright white to a red flame

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color; the portion on the eastern extremity was flashing rather than streaked; that on the west was of a reddish flame color. The bands were about two degrees wide reaching from the horizon to the zenith. The central portion of each band being brightest, diminishing to a slight light on the edges. The first appearance was at dark, and it was still bright at 10 P. M.

Sept. 3d, the Aurora was repeated, but much less brilliant and and only about 45° elevation with a dark arch beneath about 20° above the horizon. Disappeared before 9 P. M.

4. *Observations at Gardiner, Maine*, (lat. 44° 11′, long. 69° 46′), *by* R. H. GARDINER.

Sept. 1st, brilliant aurora over dark arch.

Sept. 2d, very remarkable aurora. Colored streamers with constant, and very brilliant flashes of light at north and east, and reaching south of zenith. At 9½ P. M. two very bright arches at the north, one about 12° the other about 25° above the horizon, the upper one being extremely bright; the sky between them and below the lowest, being of a dark purple.

Sept. 3d, very brilliant aurora with colored streamers in all parts of the sky.

5. *Observations at Ogdensburg, New York*, (lat. 44° 43′, long. 75′ 26°), *by* W. E. GUEST.

Sept. 2d, at 1 A. M. a splendid aurora. The light continued for nearly two hours.

Sept. 2d, 9½ P. M. A few faint streamers shot up in the east and at the same time there was a faint rose colored light in the west, when all at once there commenced on every side a display of waves of auroral light. It was an undulating motion commencing near the horizon, and waving up gradually toward the zenith. In ten or fifteen minutes it had reached the zenith, and a corona was formed, its rays of different lengths pointing downward. It disappeared almost as rapidly as it came on, and a faint light was spread on all sides. A few minutes before ten a large arch was formed, one extremity resting in the east, and the other in the N.W. Its base was dark, yet the stars were glittering through its whole length. The arch was somewhat irregular in form in the N.W. It rose gradually until a faint double arch was formed. The streamers were quite stationary, without any motion from right to left. In less than an hour it had lost its form, and the light was diffused throughout the glittering dome. There was some light continued through the night.

6. *Observations at Salem, Oregon*, (lat. 44° 58′, long. 123° 4′), *from the Statesman.*

Sept. 1st, about 8 P. M. a faint radiance was observed shooting up from the northern horizon, and gradually the whole heavens

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from north to south were covered with a delicate rosy tint, bright and glowing in the zenith, and decreasing in brilliancy near the horizon. It was most brilliant about 11 P. M. at which time it yielded sufficient light to read common print quite easily. It continued to shine with gradually decreasing splendor, and finally yielded as morning approached.

7. *Observations ot Fort Umpqua, Oregon*, (lat. 43° 48′, long 124° 0′), *by* HENRY OATLEY, U. S. Army.

An aurora was observed on the night of Sept. 1st. The light was most intense about midnight, and was sufficient to enable one easily to read print.

8. *Observations at Rochester, New York*, (lat. 43° 8′, long. 77° 51′), *by* M. M. MATHEWS.

Sept. 2d, $1^h\ 45^m$ A. M. the southern sky was one entire sheet of red light, extending from near the zenith, quite down to the horizon, and reaching laterally from S.E. to N.W. At 2 A. M the redness gathered intensity, and divided off into two nearly equal portions, one occupying the S.E. and the other the N.W. section of the sky, and for half an hour assuming a deep cherry red hue, with an occasional streamer of white light ascending nearly or quite to the zenith.

At 3 A. M. the whole sky from N.W. around to the S.E. became one entire blaze of deep red, and began sending off from all portions of its lower margin, the most brilliant streamers of white light which waved and flickered in front of the dark red back ground. They converged to a point just south of the zenith. This corona was most distinct at $3^h\ 15^m$ A. M. when it presented the appearance of an immense fan resting on the horizon. At the north lay a heavy bank of cloud rising about 2° above the horizon, and during the most brilliant display at the south, the upper edge of these clouds was tinged with a most beautiful orange color. At $3^h\ 30^m$ A. M. the redness had become comparatively faint, and the streamers had entirely disappeared.

Sept. 3d, about 8 P. M. was another auroral display confined mostly to the northern sky, and consisting principally of white streamers that were constantly flickering and dancing. At 9 P. M. they reached a point south of the zenith, and were attended by flashes of extreme brightness. At 11 P. M. the light had become quite faint, and the the streamers had disappeared.

9. *Observations at Ostego, Michigan*, (lat. 42° 28′, long. 85° 42′), *by* MATTHEW COFFIN.

Sept. 2d, aurora brightest about 2 A. M. when there was a beautiful corona a little S.W. of zenith, and rapidly shooting rays from N.W. and N.E. meeting at that point.

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10. *Observations at Riley, Illinois*, (lat. 42° 11′, long. 88° 33′), *by* E. Babcock.

Sept. 1st, 11 P. M. aurora displaying beautiful red and white streamers covering all the northern half of the heavens. At midnight the whole north was covered with beautiful streamers of varied colors. At $12^h\ 15^m$ A. M., streamers shot up from the north; the whole moved south and rested about 20° above the southern horizon, at which time a dark belt appeared under the white. Immediately streamers shot up from all around the horizon centering near the zenith. The deep red prevailed at S.E. and N.W. In less than two minutes, the whole became a broken mass, and the streamers disappeared. The dark belt still rested upon the southern horizon, and the light continued all over the heavens.

11. *Observations at Davenport, Iowa*, (lat. 41° 30′, long. 90° 38′), *by* H. J. Finley

Sept. 2d, 8 P. M. aurora in brilliant reddish parallel rays running east and west about 45° above the northern horizon. Corona pale and but few rays.

12. *Observations at Camp, No.* 33, *Nebraska*, (lat. 42°, long. 109° 50′) *by* Capt. J. H. Simpson.

Sept. 1st, at 11 P. M. about two thirds of the whole southern heavens appeared one sheet of beautiful roseate light. For a while, the light continued in a state of repose; the most concentrated portion forming a limiting belt on its northern side, and extending from a point on the horizon about 10° north of east, across the heavens to a point on the horizon about due west. From this belt, the light with its roseate hue was diffused southwardly all over the heavens down to the arc of a circle whose plane was inclined to the horizon about 10°. At length the light assumed a more intense form and shot up in whitish coruscations to the apex of the illuminated portion which was about 20° south of the zenith. My assistant observed this aurora at 10 P. M., and as it disappeared about midnight it must have lasted about two hours.

13. *Observations at Great Salt Lake City, Utah*, (lat. 40° 45′, long. 111° 26′), *by* W. W. Phelps.

An aurora was seen here on the evening of Sept. 1st and morning of the 2d. A little after midnight, it spread from north to south, from east to west; and by its light I could tell the time on my watch. There was a beautiful center equally rayed near the zenith. At times the southern hemisphere began with pale red at the zenith, and faded down to a dark orange horizon, while the northern hemisphere glowed with yellow and green. I continued my observations till past 2 A. M.

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Sept. 3d, about 8 P. M. a faint light sprung up in the east, and rose about 45° high. At $9^h\ 15^m$ it glowed beautifully. After 10 P. M. it grew fainter, and disappeared a little after midnight.

14. *Observations at New York City*, (lat. 40° 43′, long. 74° 5′), *by* Prof. O. W. MORRIS.

Aurora from $10^h\ 20^m$ P. M. Sept. 1st, till dawn of the 2d. There was a dark band at the south of 10° or 15°, then a white one and streamers of a variety of colors, mostly red, shot up in the S.E. and yellow in the S.W. interspersed with white beams. The corona was after 1 A. M. a little S. E. of the zenith.

Sept. 2d aurora from 10 P. M. to $11^h\ 15^m$ P. M. At first a faint light, then a dark segment above it on the northern horizon of about 15° in breadth; above it a band of about 8°; then another dark band, surmounted by a white one from which beams of bright light shot up along the upper edge. About $10^h\ 15^m$ waves of white light shot up nearly to the zenith. It faded away gradually after 11 P. M. retaining only a steady light on the horizon.

15. *Observations at Carlisle, Pennsylvania*, (lat. 40° 12′, long. 77° 11′), *by* Prof. C. W. WILSON.

Very brilliant aurora lasting from midnight to 3 A. M. Sept. 2d. Also Sept. 2d, 9 P. M. It first appeared as a luminous arch extending from N.W. to N.E. and 10° or 20° high. From this the light shot up in streamers first white, then turning bright red about half way to the zenith. The arch finally disappeared and, the whole then presented the appearance of a mass of light clouds, with rapid flashes behind them. The whole lasted about half an hour.

16. *Observations at Urbana, Ohio*, (lat. 40° 6′, long. 83° 11′), *by* Prof. M. G. WILLIAMS.

Sept. 2d, my first observation was at $1^h\ 30^m$ A. M. when the entire northern heavens were covered with a uniform yellowish light. There was also a large mass of crimson light in the S.W. At 2 A. M. the whole south was covered with pink and crimson light, soon after a distinct and beautiful corona was formed a few degrees south of the zenith, and continued about 15 minutes. At $2^h\ 40^m$ A. M. a beautiful column of light 12° in breadth white, yellowish, pink and crimson rose from the west at an angle of 70° with the southern horizon. At 3 A. M. the aurora was considerably diminished in intensity, but continued with variations till dawn of day.

Sept. 2d, at $8^h\ 45^m$ P. M. the light was distinct and uniform along the northern horizon. At $9^h\ 15^m$ there were fine coruscations increasing in brilliancy till 10^h. At $9^h\ 30^m$ the coruscations had for their base a well defined arch extending from N.

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80° E. to N. 85° W., and its altitude at its center was 12°. Many of them reached the zenith but no corona was formed. The coruscations were mostly white.

Sept. 3d. The evening was cloudy, but the aurora was sufficiently strong to illuminate the clouds.

17. *Observations at Newark, Ohio*, (lat. 40° 4′, long. 82° 23′) *by* DAVID WYRICK.

Sept. 1st, about midnight the entire heavens except near the southern horizon were illuminated by a pale yellow light. In about ten minutes masses of red light appeared in the E. and W; and as they faded away others appeared in the N. and S. Subsequently there was a beautiful emanation of red rays from a circular center near the zenith. The whole phenomenon lasted about two and a half hours. In the S. and S.W. lay a dark cloud rising about five degrees above the horizon.

18. *Observations at Baltimore, Maryland*, (lat. 39° 18′ long. 76° 37′), *by* C. WESTBROOK, *Telegraph Superintendent.*

On the morning of Sept. 2d, I found the telegraph wires charged to an extent far beyond the strength of our ordinary batteries. Upon disconnecting the batteries I got clear and distinct writing from Cumberland, distant 179 miles. When the current was at its maximum strength, the manipulations of the operator at Cumberland worked the armature of the relay magnet here with a force nearly equal to that which would be produced by a Grove battery of 50 cups on a short circuit. The intensity of the spark at the instant of breaking the circuit, was such as to set on fire the wood work of the switch board. The current however was variable, and at times no sensible effect could be observed.

19. *Observations at Aurora, Indiana*, (lat. 39° 4′, long. 84° 54′), *by* GEORGE SUTTON, M.D.

Sept. 1st, about 10 P. M. a faint aurora was seen in the north; and about midnight the aurora extended over the whole heavens. In the north the light was of a pale color resembling the break of day, and a few faint streamers could be seen. About 1 A. M. the whole southern heavens presented a deep red appearance. At $1^h\ 30^m$ streamers were more frequently seen in the north, and occasionally a ray would appear in the S.E and S.W. Between 2 and 3 A. M streamers arose in all directions, but much paler in the north than in the south. The streamers converged to a point presenting the appearance of a vast and gorgeous tent. From 1 A. M. until the break of day, the most brilliant display was in the south.

Sept. 2d, about 8½ P. M. the aurora appeared again in the north. There were occasional flashes of light resembling distant lightning. It disappeared in a few hours.

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20. *Observations at Auburn, California*, (lat. 38° 53′, long. 121° 2′), *by* ROBERT GORDON.

Sept. 1st, from 9½ P. M. until daylight we had a most magnificent display of aurora, in which the whole sky north, south, east and west was almost all the night glowing with ruddy light. The northern point near the horizon where the aurora commenced continued rather dark.

21. *Observations at St. Louis, Missouri*, (lat. 38° 37′, long. 90° 15′), *by* A. FENDLER.

Sept. 2d, at $8^h\ 20^m$ P. M. the aurora exhibited a white hazy light 15° above the northern horizon. Soon long white streaks appeared alternately on the right and left to 45° above the horizon, and a light red tint was sometimes visible. At $8^h\ 30^m$ the aurora disappeared. At $9^h\ 20^m$ P. M. it again threw out several white streaks to about 60° above the northern horizon. Presently it changed to a few fiery red spots. At $9^h\ 27^m$ a few short streaks shot up a little to the east of north. At $9^h\ 32^m$ the aurora became brilliant. At $9^h\ 34^m$ red with no streaks. Soon after nothing remained but white diffused light.

22. *Observations at Moneka, Kansas*, (lat. 38° 30′, long. 98°), *by* L. CELESTIA WATTLES.

Sept. 2d, an aurora appeared from 1 to 3 A. M. On the same night about 10 P. M. a light appeared in the S.E. like the rising of the moon. It grew redder and more brilliant as it extended up the sky until it reached the zenith. It now shot out to the westward, streaming continually across the sky to the horizon. It did not wholly disappear until the morning dawn.

23. *Observations at New Albany, Indiana*, (lat. 38° 17′, long. 85° 45′), *by* ALEXANDER MARVIN.

Sept. 2d, at $1^h\ 30^m$ A. M. a broad beam of crimson light extended up from the eastern horizon and met another from the west considerably south of the zenith. Beautiful streamers shot up all around the north to the point of meeting of the beams, south of the zenith. In the north a dark cloud extended from E. to W. in the form of a flat arch. Above it was an ever changing display of white and deep crimson streamers. A similar arch of cloud was visible in the south but not quite so flat; and above it, streamers concentrated in the point above mentioned. The greatest observed brilliancy was from $1^h\ 50^m$ to $2^h\ 30^m$ A. M.

Sept. 3d, $8^h\ 45^m$ P. M. streamers of white and pale red light shot from the horizon half way to the zenith. At 9 P. M. only a grey light in the north.

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24. *Observations at Louisville, Kentucky*, (lat. 38° 3′, long. 85° 30′), *from the Louisville Journal.*

On the morning of Sept. 2d for some three hours commencing about midnight, the whole heavens were lighted up in the most brilliant manner. The light was generally diffused over the whole sky, but was reddest in a southwest direction. Towards the north it was whiter with occasional streaks of green and deep crimson darting up towards the zenith.

25. *Observations at the base of the Sierra Abajo, Utah*, (lat. 37° long. 110°, *by* Dr. John S. Newberry.

Sept. 1st, no aurora was observed at 9 P. M. the heavens being partially obscured by broken clouds. At 1 A. M. I waked and was startled by the red light that penetrated the tent and tinged the landscape, which was illuminated as strongly as by the full moon shining through thin clouds. On going out I found the whole heavens of a bright crimson with streaks of white and yellow converging toward the zenith, where they formed a beautiful corona. These rays reached down within 20° of the southern horizon. The aurora continued almost without abatement till day-light.

26. *Observations at Bentonville, Arkansas*, (lat 36° 22′, long 94° 16′), *by* Paul Graham.

Sept. 1st, at 11 P. M. there was a bright sheet of light embracing the eastern and northeastern part of the horizon, from thence extending upwards 60°. It became so light that objects were clearly seen at a considerable distance. At the same time, the clouds in the S. W. were tinged with red. It continued very bright until 3 A. M. at which time it disappeared.

27. *Observations at Asheville, N. Carolina*, (lat. 35° 37′, long. 82° 29′), *by* H. H. Strawbridge.

Sept. 2d, at $12^h\,30^m$ A. M. my attention was attracted to a singular fiery light in the N.E. at an elevation of about 45°. Some 3° or 4° to the east of north also apppeared a space of whitish light. Both lights increased, the spots of faint reddish light extending quite to the zenith. At $12^h\,50^m$ the lights diffused themselves over the sky, and from about N.E. to S.W. a complete belt of roseate light extended from horizon to horizon across the zenith. It was at first about 6° or 8° in breadth, its edges very ill-defined; but it gradually widened and changed its line of direction until it became a zone of rosy light from 25° to 40° in breadth, reaching from the west to a little north of east. From $1^h\,5^m$ to $1^h\,25^m$ A. M. I was able to read with perfect ease the smallest type in a newspaper. At $1^h\,30^m$ the light slightly decreased. At 2 A. M. a large space in the belt about E.S.E. and 50° above the horizon became more intense. From $2^h\,30^m$ the clouds so thickened as to prevent observations, although the red zone was still clearly traceable.

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28. *Observations at Memphis, Tennessee*, (lat. 35° 8′, long. 90° 0′), *by* R. W. MITCHELL, M.D.

Sept. 1st, at midnight a splendid aurora flamed up suddenly in the north. Its breadth was about 30°, extending 15° on each side of the meridian and its altitude was about 40°. At first its color was nearly blood red, and motionless; but about $12^h\ 30^m$ A. M. streamers began to appear though with little or no change of color. In less than an hour the deep red began to fade, and it continued thus until 4 A. M. when it vanished.

29. *Observations at Selma, Alabama*, (lat. 32° 25′, long. 86° 51′), *by* S. K. JENNINGS, M.D.

Sept. 2d, about $12\frac{1}{2}$ A. M. a strip of red cloud nearly transparent and 9° or 12° wide commenced in the east, and soon extended across to the west, forming a magnificent arch. It was striped with the various hues of red from light to brightest scarlet, with a tinge of straw color, and from the centre of the arch diverging rays looking to the south and reaching nearly to the horizon. The rays colored like the arch were soon scattered, but the main arch did not entirely disappear until $4\frac{3}{4}$ A. M.

30. *Observations at Paulding, Mississippi*, (lat. 32° 20′, long. 89° 20′), *by* Rev. E. S. ROBINSON.

Sept. 2d, at $2^h\ 10^m$ A. M. nearly the whole visible heavens were overspread with a gauze-like lurid tint which continued till $3^h\ 30^m$ A. M. It was most brilliant in the N. E. and N. W. and at $2^h\ 30^m$ extended thirty degrees south of the zenith.

31. *Observations at Cahawba, Alabama*, (lat. 32° 19′, long. 87° 16′), *by* Dr. MATTHEW TROY.

Sept. 2d, the aurora was first observed about 1 A. M. An arch spanned the heavens from E. to W. a few degrees south of the zenith. To the north the sky had a distinct greenish tinge. The most magnificent displays of colored light were nearly over head. The light was so great that fine newspaper print could be read by it; and it continued with varying brilliancy till obscured by daylight.

32. *Observations at Natchez, Mississippi*, (lat. 31° 34′, long. 91° 25′), *by* J. J. SCOTT.

Sept. 1st, the aurora commenced at 10 P. M. I began to observe it at $12^h\ 15^m$ A. M., Sept. 2d. A glowing arch of the deepest crimson and 8° or 10° in breadth, extending from the N.E. to the N. W. points of the horizon, rising to the height of about 40°, while a fainter arch appeared below it. At $12^h\ 30^m$ rays emanated in all directions except south of a great circle passing through a point situated in the wing of Pegasus. From this point issued a broad flare of light which waved like a pennant. Every

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where in the northern sky, patches of light would appear, glow for a time and gradually disappear. These appearances continued throughout the night, growing fainter as the dawn approached.

33. *Observations at Wheelock, Texas*, (lat. 30° 55′, long. 87° 29′), *by* F. Kellogg.

At 10 P. M. Sept. 1st, I first observed a zone of crimson light 30° in breadth, reaching from 10° above the horizon due east, vertically overhead, and terminating 10° above the horizon due west. From the zenith to the eastern extremity of the zone the light was mild, the color increasing in intensity toward the east, until within 15° of the horizon where it gradually faded. At $11^h\,20^m$ a beam of whitish light passed due north through the zenith. At $11^h\,30^m$ another beam diverged from the former to the west, making an angle with it of 40°. These two beams, if continued, would unite about 25° south of the zenith. At midnight the entire space between these beams was filled up with similar but shorter beams of light, converging toward each other. Soon these central rays began to shoot bright scintillating rays of white light from their northern and western ends which travelled with great velocity. The eastern boundary of the zone became gradually paler until $12^h\,30^m$ A. M. Sept. 2d, when the color in that direction entirely disappeared, and the brightest light was then in the west and northwest. At 1 A. M. the crimson color had entirely disappeared, and nothing remained but the fan-like appearance of the numerous divergent beams of white light. The two rays first formed never changed their form or position until they disappeared about 2 A. M.

34. *Observations at Thomasville, Georgia*, (lat. 30° 50′, long. 84° 0′), *by* W. Blavett.

Sept. 2d, about 2 A. M. the whole northern half of the heavens was beautifully illuminated. The daily track which the sun now describes formed the southern boundary of the illuminated portion of the heavens. Upon this southern boundary was a border of deep blood red light, of 2° or 3° in breadth, extending from the eastern to the western horizon. Streams of pale light diverged from the point where this band cut the meridian. These pale streamers, at one moment, were numerous, and the next moment scarce a trace of them could be seen. The great red belt sometimes changed to a beautiful orange color.

35. *Observations at Mobile, Alabama*, (lat, 30° 41′, 88° 1′), *from the Daily Mercury.*

Sept. 2d, the aurora appeared at midnight and soon after 1 A. M. the eastern sky seemed bathed in a flame of lurid light, while a yet deeper flame streaked with silvery beams the N.W. These two pillars were united in the zenith, by a broad belt of

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dimmer fire. The pyramidal foci of red light were situated, one in the east, the other in the N.W. A little later the red field in the N.W. extended southerly so that one half the western sky from the horizon to the zenith seemed a blaze of fire. Meantime brilliant streamers continued to shoot from the N.E. and N.W. towards the zenith, sometimes extending over 50° or 60° of the heavens. These streamers converged towards a point on the meridian about 15° south of the zenith, and from this point shot forth smaller pencils of silvery showers. At $3^h\ 30^m$ the play of the streamers had ceased, while the flash of fiery red had spread over the whole north. The red flush in the northern quarter of the heavens, continued to glow until obscured by the solar dawn.

36. *Observations at Washington, Texas*, (lat. 30° 26′, long. 96° 15′), *by* Maj. B. F. Rucker.

Sept. 1st, at $10^h\ 30^m$ P. M. I observed a bright light in the north and N.E. At $11^h\ 30^m$ the light had become much stronger and a good deal more extended in the base; and some beautiful rays shot far up on the sky in the north. At midnight the base of fiery looking vapor extended from N.E. to N.W. The rays of fiery colored light rose from every direction like an inverted fan and converged towards a point several degrees south of the zenith. Some of these rays appeared like immense columns; others only as a thin streak; some were pale, others fiery. At 1 A. M. the light continued undiminished, although the darting rays were not so numerous. The aurora continued until obscured by the light of the sun.

37. *Observations at Jacksonville, Florida*, (lat. 30° 15′, long. 82°), *by* Dr. A. J. Baldwin.

Sept. 2d, the aurora was witnessed from midnight till daylight. At 3 A. M. the entire heavens, even at the extreme south were in a red glow. Streamers ran up from a point in the N.W. and from the S.E. and tortuous waves swelled up from the bottom of these streamers and illuminated the whole heavens. At times these looked like lambent flames, flickering like a blaze of fire.

38. *Observations at Union Hill, Texas*, (lat. 30° 11′, long. 96° 31′), *by* Dr. Wm. H. Gantt.

Sept. 1st, at 11 P. M. a faint glimmering light was visible in the N.E., which gradually grew brighter and extended over a larger space. At midnight it reached from the north 35° eastward, and mounted nearly to the zenith, and soon began to be seen west of north. At 1 A. M. it extended from west to east, and beyond the zenith. Towards the north, extending east and west about 20°, and rising about 10° above the horizon was a dark looking cloud. Above this, the light was of a whitish color, and

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from it sprang streamers of pink merging into crimson. The grandest display was from 1 A. M. to $1^h\ 35^m$. It now began to fade, and at $3^h\ 30^m$ was nearly gone. A few flashes of it, however, remained until daylight.

39. *Observations at Micanopy, Florida*, (lat. 29° 30′, long. 82° 18′), *by* James B. Bean.

Sept. 2d, at $12^h\ 30^m$ A. M. I first saw a luminous haze in the north, and at $12^h\ 35^m$ streamers shot up in the north. A few minutes before 1 o'clock a luminous arch appeared, but not well defined. At 1 A. M. it included 160° of the horizon; at $1^h\ 10^m$ there were many distinct streamers; at $1^h\ 15^m$ beautiful quivering streamers, while patches of white light appeared in different parts of the northern hemisphere. At $1^h\ 25^m$ the corona was very bright; at $1^h\ 35^m$ corona very distinct, of vivid white clouds of light. At $1^h\ 40^m$ very brilliant red beams in the west; at $1^h\ 50^m$ the arch extended from E. to W. passing through Aries and Pegasus; at 2^h a faint corona; at $2^h\ 11^m$ distinct beams near the zenith and on each side E. and W.; at $2^h\ 25^m$ arch brighter red with red patches of light and distinct streamers reaching beyond the zenith; at $2^h\ 35^m$ arch fading, at $2^h\ 40^m$ red light in N.W. but streamers not so distinct; at $2^h\ 50^m$ beautiful beams in N.E.; at 3^h arch disappearing, beams indistinct; at $3^h\ 10^m$ red haze and no beams; at $3^h\ 30^m$ very faint red haze, and faint white light near the horizon.

40. *Observations at Corpus Christi, Texas*, (lat. 27° 45′, long. 97° 30′), *by* A. M. Lea.

Sept. 1st, the aurora began about $11\frac{1}{2}$ P. M, and continued until daylight. Two-thirds of the whole visible heavens were lighted up with a rich red glow, whilst the tremulous columns of variegated light swept over the heavens, from the northern horizon through the zenith to a line within 40° of the horizon on the south. Its greatest intensity was about $1\frac{1}{2}$ A. M. Sept. 2d.

41. *Observations at Fort Jefferson, Florida*, (lat. 24° 37′, long. 82° 52′), *by* Capt. D. P. Woodbury.

Sept. 2d, at $12^h\ 45^m$ A. M. a continuous arch of red color extended from N. 50° E. to N. 50° W. having an altitude of about 15°, and the thickness of the arch throughout was about 25°. The shade of red was deepest along the central part of the arch, gradually diminishing above and below. Soon rays began to appear in faint white lines; they grew brighter, and extended above and sometimes a little below the arch. Soon the rays became numerous, traversing the red arch in right lines, and converging to a point in the magnetic meridian somewhat south of the zenith. They sometimes extended as high as the zenith, and even beyond. The aurora continued, gradually fading, till day light.

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42. *Observations at Key West, Florida*, (lat. 24° 33′, long. 81° 48′), *by* William C. Dennis.

Sept. 2d, at 2½ A. M. there were two patches of brilliant ruddy lights one in the N.E. and the other in the N.W. From the North extending 15° toward the E. there were rays of light shooting toward the zenith, the longest reaching full 60°. At $2^h\ 50^m$ the patch of light in the N.E. broke up into most brilliant rays extending toward the zenith. At 3 A. M. the patch of light in the N.W. also broke up into rays of light in a similar manner. At $3^h\ 15^m$ the rays gradually disappeared, but there still remained brilliant ruddy lights in the N.E. and N.W. At 3½ A. M. the aurora was decidedly fainter, and at 4 A. M. nothing but a faint glow remained. This glow did not entirely disappear until overcome by the light of day.

43. *Observations at Sea*, (lat. 12° 23′ N., long. 88° 28′ W.), *by* Commander W. D. Porter, U. S. Navy.

Sept. 1st, about 11½ P. M. the sky had a lurid appearance in the north, and there were occasional flashes of lightning. The rest of the sky was clear, bright and beautifully blue. The red appearance was very much like the aurora of high latitudes, and now and then it had a wavy appearance. About 1 A. M. a body of heavy clouds passed over with rain.

Art. XXXII.—*On the direction of molecular motions in Plane Polarized Light;* by Prof. W. H. C. Bartlett.

Fresnel's Formulas for reflexion and ordinary refraction.

It is proposed, in the following paper, to deduce these formulas from the principle of the conservation of living force and the rules for the resolution and comparison of forces, without any hypothesis in regard to the condition of the ether in the co-terminous bodies. If this were the only object, however, the solution would hardly be worth the space asked for it in the Journal, because all now-a-days believe in the truth of these formulas. But there is a purpose beyond this. The mode of solution may settle the vexed question in regard to the direction of the molecular motions in plane polarized light; and, on this account, it will have an interest for those who are still in doubt upon the subject. There are few opinions among scientific men held more loosely than that which relates to this question. Some contend for vibrations in, and others perpendicular to, the plane of polarization; and a few of the most eminent and thorough investigators have entertained both opinions at different times,

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having changed not once but twice or thrice. Although Professor McCullogh has shown, in his elaborate treatise on crystalline reflexion and refraction, as published in the memoirs of the Royal Irish Academy, vol. xviii, that vibrations in the plane of polarization are absolutely essential to the theory, still other results are adduced which lean to the opposite view, and opinions are still divided.

When a wave of a given length is plane polarized, by a single reflexion, the plane which contains the incident and reflected rays, is called *the plane of polarization*. Are the molecular vibrations, which in this case are rectilinear, performed in this plane or at right angles to it?

Take the well known wave function, say the first of Equations (528), Bartlett's Analytical Mechanics, omitting the subscripts, x,

$$\xi = a \cdot \sin \frac{2\pi}{\lambda} (Vt - r) \quad . \quad . \quad . \quad . \quad . \quad (1.)$$

in which the molecular vibrations are parallel to the axis x, and the wave propagation in the direction of the plane yz, ξ the actual molecular displacement at the time t, a its maximum value, V, the velocity of wave propagation, λ the wave length, and r the distance of the molecules place of rest from the origin.

Living force and quantity of motion in a plane polarized wave.

Differentiate eq. (1), with respect to ξ and t, we find:—

$$\frac{d\xi}{dt} = a \cdot V \cdot \cos \frac{2\pi}{\lambda} (Vt - r) \frac{2\pi}{\lambda}.$$

Denote the density of the medium by Δ, and the area of any portion of the wave-front by a, then will the mass between two consecutive positions of this area be $a \cdot \Delta \cdot dr$, and the living force within a quarter of a wave-length be:—

$$\left.\begin{aligned} \int_{r+\frac{1}{4}\lambda}^{r} \Delta \cdot a \cdot dr \frac{d\xi^2}{dt^2} = \int_{Vt-r=\frac{1}{4}\lambda}^{Vt-r=0} \Delta \cdot a \cdot \frac{2\pi}{\lambda} \cdot a^2 \cdot V^2 \cdot \cos^2 \frac{2\pi}{\lambda} (Vt - r) \\ \times \frac{2\pi}{\lambda} dr = \tfrac{1}{2} \frac{\pi^2}{\lambda} \cdot V \cdot \Delta \cdot a \cdot V \cdot a^2. \end{aligned}\right\} \quad (2.)$$

Dividing by the volume $a \cdot V$, and recalling that π and $\frac{V}{\lambda}$ are constant, we shall find that the quantity of living force in a unit of volume of the medium will vary directly as the product of the density and square of the greatest displacement; and the relation of these products, in the case of any two waves, will determine the relation of the effects of these waves upon the organs of sense upon which they act.

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Again, the quantity of motion in this quarter of wave-length will be:—

$$\int_{r+\frac{1}{4}\lambda}^{r} \Delta.a.d\xi.\frac{d\xi}{dt} = \int_{Vt-r=\frac{1}{4}\lambda}^{Vt-r=0} \Delta.a.a.V.\cos\frac{2\pi}{\lambda}(Vt-r)\frac{2\pi}{\lambda}dr = \Delta.a.a.V. \quad (3.)$$

Resolution of living force and of motion, by deviating surfaces.

Take the co-ordinate plane $x\,z$ in the plane of incidence, and the axis z in the direction of the normal to the incident wave, the axis y will be parallel to the line of the nodes of the molecular orbit in the deviating surface, at the place of incidence. Then denoting any portion of the trace of the plane of incidence on the denoting surface by s, and the angle of incidence and of reflection by φ and φ', respectively, will the element of the deviating surface at the place of incidence be $ds.dy$, and its projections upon the incident, reflected, and refracted wave-fronts, respectively, be $ds.dy.\cos\varphi$, $ds.dy\cos\varphi$, and $ds.dy.\cos\varphi'$. These will take the place of a in Equations (2) and (3), in computing the living force and quantity of motion in the incident, reflected and refracted waves.

Now take a wave of common light and replace it by its two components, having the vibrations in the one parallel and in the other perpendicular to the plane of incidence.

First take the component wave in which the molecular motions are parallel to the plane of incidence, and therefore parallel to the axis x, and employ the subscripts i, r and t, to denote the incident reflected and transmitted or refracted component waves, respectively. The living force in the incident, must be equal to the sum of the living forces in the reflected and refracted components; whence, equation (2), omitting the common factors,

$$\Delta.\cos\varphi.V.\alpha^2_{xr} + \Delta_{,}.\cos\varphi'.V_{,}.\alpha^2_{xt} - \Delta.\cos\varphi.V.\alpha^2_{xi} = 0;$$

or, recalling that $\frac{V}{V_{,}} = \frac{\sin\varphi}{\sin\varphi_{,}}$,

$$\alpha^2_{xr} + \frac{\Delta_{,}}{\Delta}.\frac{\cos\varphi'}{\cos\varphi}.\frac{\sin\varphi'}{\sin\varphi}.\alpha^2_{xt} - \alpha^2_{xi} = 0 \quad . \quad . \quad . \quad (4.)$$

in which Δ and $\Delta_{,}$ are the densities of the medium of incidence and of intromittance.

The molecular motions are all parallel to the plane of incidence, and at the same time normal to the directions of their respective wave motions; they, therefore, make with one another angles equal to those made by the directions of these latter motions, and we obtain two more equations from the rules for the resolution and composition of oblique forces. The angles made by the direction of the motion in the incident with the directions of the motions in the reflected and refracted waves, are $180°-2\varphi$ and $360°-(\varphi-\varphi')$, respectively; and the angles under which the

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directions of the motions in the latter waves are inclined to one another, is $180°-(\varphi+\varphi')$. Whence:—

$$\Delta_{,}\cos\varphi\,.\,V.\alpha_{xr}=-\Delta.\cos\varphi\,.\,V.\alpha_{xi}.\frac{\sin(\varphi-\varphi')}{\sin(\varphi+\varphi')};$$

$$\Delta_{,}\cos\varphi'\,.\,V_{,}.\alpha_{xt}=\Delta.\cos\varphi\,.\,V.\alpha_{xi}.\frac{\sin 2\varphi}{\sin(\varphi+\varphi')};$$

or,
$$\alpha_{xr}=-\alpha_{xi}.\frac{\sin(\varphi-\varphi')}{\sin(\varphi+\varphi')} \quad . \quad . \quad . \quad . \quad . \quad . \quad (5.)$$

$$\alpha_{xt}=\alpha_{xi}.\frac{\Delta}{\Delta_{,}}.\frac{\cos\varphi}{\cos\varphi'}.\frac{\sin\varphi}{\sin\varphi'}.\frac{\sin 2\varphi}{\sin(\varphi+\varphi')} \quad . \quad . \quad . \quad . \quad (6.)$$

Substituting these in equation (4.), we readily find:—

$$\frac{\Delta}{\Delta_{,}}=\frac{4\cos^2\varphi'.\sin^2\varphi'}{\sin^2 2\varphi}=\frac{\cos^2\varphi'.\sin^2\varphi'}{\cos^2\varphi.\sin^2\varphi};$$

whence,
$$\sqrt{\Delta_{,}}=\sqrt{\Delta}.\frac{\sin 2\varphi}{2\cos\varphi'.\sin\varphi'}=\sqrt{\Delta}.\frac{\cos\varphi.\sin\varphi}{\cos\varphi'.\sin\varphi'} \quad . \quad . \quad (7.)$$

Substituting the above ratio of the densities in the equation just preceding, we get:—

$$\alpha_{xt}=\alpha_{xi}.\frac{2\cos\varphi'.\sin\varphi'}{\sin(\varphi+\varphi')}; \quad . \quad . \quad . \quad . \quad . \quad . \quad (8.)$$

multiplying this by equation (7), member by member, and the equation giving the value of α_{xr}, by $\sqrt{\Delta}$, and taking:—

$$\sqrt{\Delta}.\alpha_{xi}=1; \quad \sqrt{\Delta}\alpha_{xr}=v; \quad \sqrt{\Delta_{,}}.\alpha_{xt}=u,$$

we find:—

$$v=-\frac{\sin(\varphi-\varphi')}{\sin(\varphi+\varphi')} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (9.)$$

$$u=\frac{\sin 2\varphi}{\sin(\varphi+\varphi')} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (10.)$$

To which may be added the relations,

$$\sin\varphi'=\frac{\sin\varphi}{m}; \quad \cos\varphi'=\sqrt{1-\frac{\sin^2\varphi}{m^2}}.$$

Transposing the term of which α_{xt} is a factor to the second member in equation (4), subtracting equation (5) from $\alpha_{xt}=\alpha_{xi}$, dividing the first result by the second, and multiplying the quotient by equation (5), we readily find:—

$$\frac{\alpha_{xi}+\alpha_{xr}}{\cos\varphi}=\frac{\alpha_{xt}}{\cos\varphi'} \quad . \quad . \quad . \quad . \quad . \quad . \quad (11.)$$

That is, the projection in the direction of wave propagation and on the deviating surface, of the greatest displacement in the incident, increased by that in the reflected wave, is equal to like projection of the greatest displacement in the refracted wave.

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Next, take the wave in which the molecular motions are perpendicular to the plane of incidence and therefore parallel to the axis y; these are parallel to the deviating surface. The motions in the incident, reflected and refracted waves are parallel to one another, and, by the principles of parallel forces, the sum of the motions in the reflected and refracted waves must be equal to that in the incident. The equation for the living force will be the same as before. Whence:—

$$\Delta.\cos\varphi.V.\alpha^2_{yr}+\Delta_{,}.\cos\varphi'.V_{,}.\alpha^2_{yt}-\Delta.\cos\varphi.V.\alpha^2_{yi}=0;$$

$$\Delta.\cos\varphi.V.\alpha_{yr}+\Delta_{,}.\cos\varphi'.V_{,}.\alpha_{yt}-\Delta.\cos\varphi.V.\alpha_{yi}=0. \quad (12.)$$

In which Δ and $\Delta_{,}$ are, as before, the densities of the medium of incidence and of intromittance, respectively; or,

$$\alpha^2_{yr}+\frac{\Delta_{,}}{\Delta}.\frac{\sin\varphi'}{\sin\varphi}.\frac{\cos\varphi'}{\cos\varphi}.\alpha^2_{yt}-\alpha^2_{yi}=0$$

$$\alpha_{yr}+\frac{\Delta'}{\Delta}.\frac{\sin\varphi'}{\sin\varphi}.\frac{\cos\varphi'}{\cos\varphi}.\alpha_{yt}-\alpha_{yi}=0. \quad . \quad . \quad . \quad (13.)$$

Transposing the terms containing α_{yr} and α_{yi} to the second members, and dividing the first by the second, we find:—

$$\alpha_{yr}+\alpha_{yi}=\alpha_{yt}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (14.)$$

That is, the greatest displacement in the refracted is equal to the sum of the greatest displacements in the incident and reflected waves.

Substituting the value of $\frac{\Delta'}{\Delta}$, as given by equation (7), in equation (13), we have:—

$$\alpha_{yr}+\frac{\sin\varphi.\cos\varphi}{\sin\varphi'.\cos\varphi'}.\alpha_{yt}-\alpha_{yi}=0. \quad . \quad . \quad . \quad . \quad (15.)$$

Substituting in this, first the value of α_{yt}, and then of α_{yr}, deduced from equation (14), we readily get:—

$$\alpha_{yr}=-\alpha_{yi}.\frac{\tan(\varphi-\varphi')}{\tan(\varphi+\varphi')}; \quad . \quad . \quad . \quad . \quad . \quad (16.)$$

$$\alpha_{yt}=\alpha_{yi}.\frac{4\cos\varphi'.\sin\varphi'}{\sin 2\varphi+\sin 2\varphi'}. \quad . \quad . \quad . \quad . \quad . \quad (17.)$$

Multiplying the first of these by $\sqrt{\Delta}$, and the second by equation (7), and making:—

$$\sqrt{\Delta}.\alpha_{yi}=1; \quad \sqrt{\Delta}.\alpha_{yr}=v'; \quad \sqrt{\Delta_{,}}.\alpha_{yt}=u';$$

there will result,

$$v'=-\frac{\tan(\varphi-\varphi')}{\tan(\varphi+\varphi')} \quad . \quad . \quad . \quad . \quad . \quad . \quad (18.)$$

$$u'=\frac{\sin 2\varphi}{\sin(\varphi+\varphi').\cos(\varphi-\varphi')}. \quad . \quad . \quad . \quad . \quad . \quad (19.)$$

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18. Divide equation (10) by equation (9), and equation (19) by equation (18), replace v, u, v' and u' by their values, and substitute for the ratio of the square roots of the densities its value as given in equation (7), we find:—

$$\frac{\alpha_{xt}.\cos\varphi'}{\alpha_{xr}.\cos\varphi}=-\frac{\cos\varphi'}{\cos\varphi}.\frac{\sin 2\varphi'}{\sin(\varphi-\varphi')},$$

$$\frac{\alpha_{yt}}{\alpha_{yr}}=-\frac{\sin 2\varphi'}{\sin(\varphi-\varphi').\cos(\varphi+\varphi')}. \quad . \quad . \quad . \quad . \quad (20.)$$

But $\alpha_{xt}.\cos\varphi'$ and $\alpha_{xr}.\cos\varphi$, are the components parallel to the deviating surface of the displacements which are in the plane of incidence; α_{yt} and α_{yr} are already parallel to the deviating surface; whence, as long as $\varphi>\varphi'$, that is as long as the velocity of wave-motion in the medium of incidence exceeds that in the medium of intromittance, the molecular phases in the refracted and reflected waves will be opposite, and conversely.

Denote the living force in the incident wave of common light by unity, that in each of its component waves will be denoted by half of unity; and the total living force in the reflected components will, eqs. (9) and (18), be:—

$$v^2+v'^2=\tfrac{1}{2}.\frac{\sin^2(\varphi-\varphi')}{\sin^2(\varphi+\varphi')}+\tfrac{1}{2}.\frac{\tan^2(\varphi-\varphi')}{\tan^2(\varphi+\varphi')} \quad . \quad . \quad (21.)$$

and in the refracted components, eqs. (10) and (19),

$$u^2+u'^2=\tfrac{1}{2}.\frac{\sin^2 2\varphi}{\sin^2(\varphi+\varphi')}+\tfrac{1}{2}.\frac{\sin^2 2\varphi}{\sin^2(\varphi+\varphi').\sin^2(\varphi-\varphi')}. \quad (22.)$$

If $\varphi+\varphi'=90°$, then will:—

$$\sin\varphi=m\sin\varphi'=m\cos\varphi,$$

or,
$$\tan\varphi=m;$$

in which φ is the maximum polarizing angle; the wave will therefore be wholly polarized; and since eq. (21) will reduce, in the second member, to the first term, the wave will contain those vibrations only which are parallel to the plane of incidence; that is, the vibrations will be in the plane of polarization. The intensities of the reflected and transmitted components will be, respectively, under this condition,

$$\tfrac{1}{2}\cos^2 2\varphi \text{ and } \tfrac{1}{2}\sin^2 2\varphi+\tfrac{1}{2}\tan^2 2\varphi.$$

West Point, N. Y., 1860.

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ART. XXXIII.—*On some Questions concerning the Coal Formations of North America;* by LEO LESQUEREUX.

(Continued from page 74.)

Stratigraphical distribution of the Coal-flora.

DETAILS concerning the stratigraphical distribution of the coal-flora of North America, to be intelligible, ought to be prefaced by a few remarks on the order of superposition of the strata of the coal-measures.

A general section, showing the order of stratification in the whole extent of our coal-fields, would appear perhaps merely hypothetical. Such is, nevertheless, the uniformity of the distribution of the strata of our coal-basins, that a section made in Western Illinois or Western Kentucky or in any part of the coal-fields of these States, will prove comparatively similar, (that is with some difference in the thickness of the strata,) to any section made in the coal-fields of Pennsylvania or of Ohio. This analogy of stratification has been fully established by a series of comparative sections, reported for the 4th vol. of the State Geological Survey of Kentucky. Such a comparison of sections had been attempted before, for the State of Pennsylvania, by my friend J. P. Lesley, in his excellent Manual of Coal. But it was not based on palæontological evidence and thus the contemporaneousness of the juxtaposed strata was necessarily problematical. On the contrary, by admitting the similarity of the flora of the coal-strata as a coëval mark and as a basis for juxtaposition of the strata, the result of the comparison of sections gives evidence remarkable in a double point of view. First it shows, by juxtaposition of the coal strata of which the shales contain the same species of fossil plants, the uniformity of stratification in the whole area of the coal-fields of the United States; and secondly it proves that the distribution of the coal plants has followed the same developement, the same successive modification in the whole extent of the same coal-field.

Though plants of carboniferous genera are found below the upper bed of the Archimedes or mountain limestone, as, apparently, no coal has been formed at this low station, I take as the base of the true coal measures this Archimedes limestone, replaced in Eastern Pennsylvania and Northern Virginia by the red shales of the old red sandstone. From the top of the mountain limestone, to the top of the millstone-grit series, including some beds of coal at its base, the average thickness is from three to four hundred feet. The upper part of the millstone-grit series, sometimes its whole thickness, is a compound of coarse sandstone, shales and especially conglomerate, the last predominat-

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ing. The conglomerate formation thickens wonderfully at some places, generally increasing westward. Its greatest thickness, in the sharp Mountain near Pottsville, Penn., is 1100 feet.*

To elucidate the details of this general section, we can admit the numbered division of the coal strata, as it has been most satisfactorily established by Dr. D. Dale Owen for the State Geological Survey of Kentucky, vol. iii, p. 18. From the top of the millstone-grit to the base of another great sandstone formation, the Mahoning sandstone, there are generally five workable beds of coal. The lowest is No. 1A; then No. 1B to No. 4 inclusively. No. 1B coal is the Big, or Mammoth coal of the East. Like the conglomerate, it increases in thickness progressively westward. The fourth bed of coal is the Pomeroy coal of Ohio. The Mahoning standstone overlying No. 4 bed of coal, like the millstone-grit is generally conglomerate at its upper part. Its thickness averages from one hundred to one hundred and fifty feet. It might, as well as the true conglomerate, be considered as the basis of a new coal epoch; even more so, perhaps, since the vegetation at its base and at its top shows a greater difference than in the coal strata above and below the millstone-grit. From the Mahoning sandstone upward, the marine influence predominates more and more and the shales of the coal strata more generally contain remains of fishes and of marine shells than of terrestrial plants. This Mahoning sandstone is separated by about 500 feet of strata from another great sandstone formation still resembling in its composition and thickness the Millstone-grit or conglomerate. It is the Anvil-rock sandstone of the State Geological Survey of Kentucky. The 500 feet of measures between it and the Mahoning, are sometimes barren of coal. But westward they contain from Coal No. 6 to No. 12, five workable beds of coal and two, scarcely if ever, of a workable thickness.† The highest coal strata of this section, Nos. 11 and 12 united together, form the great Pittsburg coal bed of Pennsylvania.

Above the Anvil-rock sandstone, there are still 500 to 700 feet of coal measures, in which some thin beds of coal are formed. But this examination of the flora of the coal measures can not be extended higher than the Anvil-rock. The upper division has been locally washed away by erosion and where it is still present, its coal beds are too thin for working and thus their unexposed shales can not be satisfactorily examined. Near Shawneetown, Illinois, the first coal above the Anvil-rock

* H. D. Rogers's final report of the State Geol. Survey of Pennsylvania, vol. i, p. 109.

† In those coal regions of the United States where coal is abundant, a bed of bituminous coal is not remunerative, when under a thickness of three and a half feet. In the Anthracite basin, the working becomes unprofitable for a bed of less than two and a half feet.

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sandstone has a roof of laminated sandstone, blackened by broken and undeterminable leaves and small stems of ferns. The second bed at the same place contains only fossil shells and remains of fishes.

The first trace of a terrestrial vegetation in the Palæozoic strata of North America appears in the Marcellus shales or middle Devonian, in a species of *Lepidodendron* named *L. primævis* by Prof. H. D. Rogers.* It has not been described, but only obscurely figured by a wood-cut; and as I have not seen the specimen and could detect at the place where it has been found, near Huntingdon, only detached leaves of this species, its relation or its specific value is unknown to me. In the Devonian black shales of Ohio, and perhaps of Illinois,† large silicified trunks of trees have also been found, always very rare and at far distant localities.

Ascending higher, we find species of *Lepidodendron*, of *Sigillaria*, and especially of *Calamites* and stems (*Bornia* Sternb. and *Stemmatopteris* Göpp.) in the transition series of the Old Red sandstone of Pennsylvania, and especially in the Mountain limestone by which it is represented in the Western States. Leaves are very rare in this formation and of a different type from those of the coal measures. Heretofore, all those which have been found belong to the genus *Noeggerathia* (Sternb.), of which I have never seen a single specimen in the true coal measures.‡ Below the 3d or upper Archimedes limestone, there is in Illinois and in Arkansas a bed of schistose sandstone which, with peculiar species, already contains some others of the true coal measures. In the Cabinet of the State Geological Survey of Illinois, I have seen from this subcarboniferous sandstone, specimens of *Stigmaria anabathra* (Corda), and Göppert's varieties: *minor*, and *undulata, reticulata, stellata;* of *Lepidodendron Veltheimianum* Sternb., a species peculiar to the transition series, both in Europe and America; *of Lepidodendron Worthianum* Lsqx., a beautiful species related to *L. Volkmannianum* Sternb.; one *Lepidodendron* sp. nov.; one *Caulopteris*, one *Megaphytum*, all three new and undescribed species; *Calamites Suckowii* Brgt.; one *Bornia* Sternb.;

* Final report of the State Geol. Survey of Penn., vol. ii, p. 828.

† Mr. A. H. Worthen, State Geologist of Illinois has in his cabinet this Devonian silicified wood without label of locality. It is also in the cabinet of Dr. D. Dale Owen at New Harmony, also without label, All my specimens have been cut from a large tree protruding from Devonian strata near Delaware, Ohio, and discovered by Dr. Mann, who kindly communicated them to me. It is by all appearance the wood of a Lepidodendron, and it will be figured and described with the silicified Psaronius of the coal measures.

‡ Dr. Newberry mentions in his catalogue two species of Noeggerathia found in the coal above the conglomerate. One is *Noeggerathia Beinertiana* Göpp., a species evidently referable to *Cordaites* Ung., and probably only a small form of *Cordaites borassifolia* Ung. The other is *Noeggerathia microphylla* Newb., undescribed and consequently unknown to me.

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Cordaites borassifolia Ung., *Knorria imbricata* Sternb., and some undeterminable stems. From these plants, six have been found also above the conglomerate series, ascending to coal No. 1B, or even higher. They are *Stigmaria anabathra*, var. *undulata* Göpp., and var. *minor* Göpp., *Lepidodendron Worthianum* Lsqx., *Knorria imbricata* Sternb., *Calamites Suckowii* Brgt. and *Cordaites borassifolia* Ung. These two last species are present in the whole thickness of the coal measures, as high at least as the 12th coal.

As I have said before, no coal has been seen as yet formed below the 3d Archimedes limestone. But just overlying it, a bed of coal is generally present over the whole extent of the Western coal-fields. In Arkansas, this is the only workable bed of coal, its thickness varying from 18 inches to 4½ feet. In the western part of the Eastern coal-basin of Kentucky, and also in Virginia, two, even three beds of good coal have been formed below the millstone grit. All the species of fossil plants of the shales of these coal strata have been found also in Arkansas. At Pottsville and Mauch Chunk, near the eastern margins of the coal-fields, the coal is formed between strata of conglomerate, and even at the base of this formation.

The shales of the subconglomerate coal contain not only remains of trees of large size, like the subcarboniferous sandstone, but thus early and simultaneously many of the species of ferns which become more and more abundant above the conglomerate series. Thus *Pecopteris velutina* Lsqx., *Neuropteris flexuosa* Brgt., *N. hirsuta* Lsqx., *Sphenophyllum Schlotheimii* Sternb., *Pecopteris nervosa* Brgt., *Annularia sphenophylloides* Ung., *Odontopteris crenulata* Brgt., *Cordaites borassifolia* Ung., *Hymenophyllites furcatus* Brgt.: and *Sphenopteris latifolia* Brgt., all found there in connection with it, ascend to coal No. 1B or higher. Among the trees seen in this coal, and ranging still higher in the measures, there are six species of *Lepidodendron*, two of *Sigillaria*, two *Calamites*, *Stigmaria* and its varieties, and a few species of *Carpolithes* and of *Cardiocarpon*. Though this subconglomerate coal has not been yet explored over a large area, it shows already more than twenty species, representing all the essential genera of our coal-plants, which have a wider range of distribution, and appear still in the coal strata above the conglomerate. It is therefore evident that a separation of the subconglomerate coal, as a peculiar formation and under a peculiar name, is contrary to palæontological evidence. Like every other coal bed the subconglomerate coal has species peculiar to it. Two species of *Lepidodendron*, two of *Sigillaria*, one of *Sphenophyllum*, two species of *Trigonocarpum*, one very large *Cardiocarpum*, one *Stigmaria*, one *Alethopteris*, and three or four *Sphenopteridex*.* In the shales of this lowest bed of coal, near Frog

* All the new species of this coal, or at least most of them, belong to the State Geological survey of Arkansas, and are reserved for publication in the second volume of that Report.

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Bayou, Arkansas, I have found for the first time in America one of those beautiful wings of insects, referred by M. Germar to the genus *Blattina*. Though related to *Blattina didyma* Germ., our American species is new.

The first bed of coal above the conglomerate, our No. 1A, is generally thin (two feet to four feet thick at the most), and overlaid by a stratum of coarse sandstone or of black very bituminous shales. In the shales, I have never found any other remains of plants, than leaves and cones of *Lepidodendron*, six species of *Lepidostrobus* and *Lepidophyllum*. The shales are too bituminous for good preservation of specimens. With the remains of plants, the shales generally contain a few specimens of *Lingula umbonata* Cox, a shell extremely abundant in the shales of No. 1B, and rarely represented by a very few individuals in the coal No. 2. Sometimes coal No. 1A and No. 1B become united together, being separated only by a thin parting of shales, but more generally there is between them a stratum of coarse sandstone, marked with numerous prints or casts of great vegetables of the coal, especially of *Lepidodendron obovatum* Sternb., *L. rimosum* Sternb., *L. rugosum* Sternb., *L. aculeatum* Sternb., *Sigillaria alternans* Ll. and Hutt., *S. reniformis* Brgt., *S. Brardii* Brgt., *S. lævigata* Brgt., *Sirigodendron pachyderma* Brgt., and some fruits, *Carpolithes* and *Trigonocarpon*. Other species, one *Caulopteris*, one *Megaphytum*, some *Calamites*, all rendered undeterminable by the coarse compound of the sandstone, have been seen in this stratum.

Coal 1B. Its horizon appears to mark the epoch of the highest development of the vegetation of the coal formation. Not only is this coal bed the most reliable of all, and consequently the most extensively worked, not only does it attain locally an enormous thickness, justly meriting the name which it bears in some parts of Pennsylvania, viz., the *Big Coal*, the *Mammoth vein*, &c.; but the shales which cover it and sometimes divide it into two, three, or more members, afford to the explorer the greatest amount of species, distributed among nearly all the genera of plants which belong to the coal formation.

The vegetation of coal No. 1B may be characterized: First, by the great number of fruits, found in the strata connected with it, either in the shale above or in the sandstone below. Indeed, nearly all the species of *Trigoncarpum*, *Cardiocarpum*, *Rhabdocarpos* and *Carpolithes* belong to it. Its second characteristic, is the great abundance of species of *Lepidodendron*. Eighteen species of this genus have been found in this bed of coal, and no new species have been, as yet, seen above it. A third character is the constant presence in the shales of our No. 1B of *Alethopteris Lonchitidis* Brgt. which apparently belongs exclusively to it. Generally speaking, the coal has the great forms of the section of the *Pecopterideæ*, viz. *Alethopteris* and *Callipteris*, and is mostly

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deprived of the true *Pecopteris* or of the small forms of the order. *Alethopteris Serlii* Brgt., is met in its shales, but is very scarce; the species rather belongs to No. 3. It has *Alethopteris marginata* Brgt., not seen in any other horizon; also *Alethopteris lævis* Lsqx., probably peculiar to it, and *Alethopteris nervosa* Brgt., which ascends higher. Besides the genus *Pecopteris* Brgt., I cannot mention any species peculiar to this coal. *Pecopteris velutina* Lsqx., a fine species, which by its fructification would be referable to a separate genus, was found in its shales; but we have mentioned it also with the subconglomerate coal. *Pecopteris Miltoni* Brgt., or *P. polymorpha* Brgt. (probably the same) is a species found sometimes in its shales; but it is common in the whole thickness of the coal measures. *Pecopteris pennæformis* Brgt., *P. Sillimanni* Brgt., *P. plumosa* Brgt., *P. villosa* Brgt., are connected with it, but are found still higher in the measures. The fourth character of this coal is the great number of species of large *Sphenopteridæ* and of *Hymenophyllites* connected with it. *Sphenopteris latifolia* Brgt., *S. obtusiloba* Brgt., *S. glandulosa* Lsqx., *S. polyphylla* Ll. and Hutt., *S. Newberryi* Lsqx., *S. artemisiæfolia* Brgt., with *Hymenophyllites Hildrethi* Lsqx., and *H. spinosus* Göpp., have not been hitherto found except with this coal.

Of the genus *Odontopteris* Brgt., all the American species are connected with No. 1B, except *O. crenulata* Brgt., which first appears in the subconglomerate coal.

Among the *Sigillariæ*, it has as species peculiar to it, *Sigillaria stellata* Lsqx., *S. Serlii* Brgt., *S. tessellata* Brgt., *S. Brochanti* Brgt., *S. alveolaris* Brgt., *S. oculata* Brgt., *S. polita* Lsqx., *S. obovata* Lsqx., *S. alternans* Ll. and Hutt., *S. discoidea* Lsqx., and *S. catenulata* Ll. and Hutt.

By far the greatest number of specimens preserved in the shales of this coal belong to two species which we have seen already below the conglomerate, viz. the omnipresent *Neuropteris hirsuta* Lsqx., and *N. flexuosa* Brgt. Although these two species are so abundantly represented in our coal No. 1B, that sometimes their remains exclusively, fill the roof of some mines, other species of *Neuropteris* are scarcely found in connection with it. *Neuropteris tenuifolia* Brgt., and *N. smilacifolia* Sternb., belong to it; but both species are very scarce and closely related to *N. flexuosa* Brgt. The last especially may be considered as a variety of it. *Neuropteris lancifolia* Newb. is an undescribed species, found in the shales of this coal, perhaps only one of the numerous forms of *N. hirsuta* Lsqx.

Connected with this remarkable coal bed, we still find *Cordaites borassifolia* Ung., and *Dictyopteris obliqua* Bunby., locally distributed; *Whittleseya elegans* Newb., formerly described and found at one or two places only; species of *Asterophyllites*, *Annularia* and *Sphenophyllum*, but none of them peculiar to it, and species of *Calamites*, especially *C. Suckowii* Brgt., *C. Cistii*

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Brgt., *C. nodosus* Brgt., *C. approximatus* Brgt., and *C. cannæformis* Brgt., some of which ascend higher in the coal measures. I never found in it any specimen referable to *Calamites pachyderma* Brgt., which M. Geinitz unites, I think incorrectly, to *C. cannæformis* Brgt. Its true place is within the sandstone of the Millstone grit. Thus also, from dissimilarity of distribution, I would admit with Mr. Brongniart *Calamites decoratus* Brgt., and *C. Suckowii* Brgt., as distinct species, though they are also united by M. Geinitz; the last belonging especially to this coal, the former to a higher bed, No. 3d.

It is perhaps useless to mention that *Stigmaria ficoides* Sternb., is also present with our coal 1B. It has not only penetrated by its leaves the fire clay which the coal overlies; but has filled the coal also by its remains:—moreover, contrary to the assertions of some authors that *Stigmaria*, being a root, is found only in the bottom fire clay of the coal strata,—it has mixed its remains in the shales of this bed, and even sent them into the sandstone by which they are overlaid, or sometimes replaced. Thus, in the absence of other remains, or until I had discovered them, the abundance of *Stigmaria ficoides* in the roof-shales has helped me to identify this coal in many places.

The roof shales of No. 1B coal are occasionally overlaid by a stratum of limestone or argillaceous shale, containing a great abundance of fossil shells. Locally also, and where this limestone is absent, its place is taken by a bed of coal, variable in thickness from one to four feet. As it is rarely formed, and sometimes in close proximity to No. 1B, I consider it still as a member of No. 1B; and it is accordingly ranked in my sections as No. 1C. Though it has no great value,* its combustible matter being a compound of half decomposed stems, mostly transformed into charcoal and rusted by oxyd of iron, it is worth mentioning as an evidence of conformity of composition of the shales and of the coal strata at the same geological horizon over a wide area. The shales of this bed, even the coal itself, appear like a compound of broken, heaped up stems of ferns and *Calamites*. Now, it is found with exactly the same appearance, either of shales or of coal, on the western limits of the eastern coal basin of Kentucky, near Greenupsburg; in Virginia, on the Tug river; and in the middle of the great Apalachian coal basin, near the mouth of Yellow creek, in the Ohio river, above Steubenville, Ohio.

Coal No. 2, is generally placed at about one hundred feet above No. 1B, and separated from it by various strata of shales and sandstone, and occasionally by the limestone mentioned before, or also by a cherty compound named Burrstone. Its roof-shales

* It is particularly well developed in the northern part of the coal basin of Illinois and Indiana where I have seen it lately from four to six feet thick of pretty good coal.

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are coarse, micaceous, and generally barren of fossil remains: but it is separated into two by a shale parting, of from two to eighteen inches, which contains a great abundance of broken remains of plants. Here, we have scarcely any trace of *Lepidodendron*, but a remarkable abundance of *Calamites*, *Asterophyllites*, *Neuropteris flexuosa* Brgt., *Neuropteris hirsuta* Lsqx., *Cordaites borassifolia* Ung. I have found with this coal *Lepidodendron obovatum* Sternb., *Sigillaria Brardii* Brgt., with its leaves, and an undescribable *Lepidostrobus* Brgt. Without doubt, it has many peculiar species, but I have never found the shales hard enough for preserving specimens even of a moderate size. As soon as they are exposed to atmospheric influence, they are rapidly broken into minute particles by efflorescence of the sulphuret of iron, generally predominant in this coal. Its vegetation, as far as can be seen, is intermediate between No. 1B and No. 3d coal, the number of predominating species being from the genera *Asterophyllites*, *Annularia*, and especially *Calamites*.

Coal No. 3 is variable in thickness, and is not as generally and as uniformly formed as No. 1B. Wherever it has been seen it is overlaid by a roof of gray soft shales containing a great quantity of well preserved remains of plants. It has still a few species of *Lepidodendron*: *Lepidodendron dichotomum* Sternb., and especially its branches, are abundantly distributed in its shales, with a great number of its long cones, *Lepidostrobus variabilis* Brgt., and its leaves. *Dictyopteris obliqua* Bunb. is still found connected with it and locally distributed as in coal No. 1B. It has also in abundance *Neuropteris hirsuta* Lsqx., and *N. flexuosa* Brgt., and as peculiar species of this genus, apparently belonging exclusively to it, *Neuropteris Clarksoni* Lsqx., *N. gigantea* Sternb., *N. delicatula* Lsqx., and *N. rarinervis* Bunby. The genus *Sphenopteris* Brgt. is represented in this coal by *Sphenopteris flagellaris* Lsqx., *S. Lesquereuxii* Newb., and by an *Hymenophyllites*, intermediate between *H. elegans* Brgt., and *H. Hildrethi* Lsqx., and which is perhaps only a variety of the last species. The section of the large *Pecopterideæ* shows in it numerous remains of some of its species, especially *Alethopteris Serlii* Brgt., *A. nervosa* Brgt. and *A. muricata* Göpp. This last is uncommon and has been found only in connection with this coal. The species of true *Pecopteris* are more numerous than in coal 1B, beginning here to show their predominance. *Pecopteris unita* Brgt., *P. polymorpha* Brgt., *P. Loschii* Brgt., *P. Sillimani* Brgt. are connected with this bed, though none of them except perhaps the first is peculiar to it. *Asterophyllites*, *Annularia* and *Sphenophyllum* have left their remains in its shales, all still more abundant in No. 4 coal, except *Annularia sphenophylloides* Ung., which attains here its greatest development. Among the *Calamites*, it has *Calamites decoratus* Brgt., *C. undulatus* Brgt., the first apparently peculiar to it, with *C. cruciatus* Brgt. and *C. ramosus* in abundance. Thus

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far, I have never found with this coal *C. Suckowii* Brgt., which has left so many of its remains in coal 1B. A few of the most common species of *Sigillaria*, viz. *S. Brardii* Brgt., *S. Menardi* Brgt., *S. reniformis* Brgt. and *S. Yardleyi* Lsqx., all species (except *S. Menardi* Brgt.,) which ascend higher or are found also in No. 1B. To this enumeration, I may add two species of fruits, *Carpolithes amygdaliformis* Göpp, and a small *Cardiocarpon*, fruit of an *Asterophyllites*. It is found also with No. 4th coal.

Coal No. 4th is the coal of the small ferns, especially of the small *Pecopterideæ*. It has no more remains of *Lepidodendron*, neither cones, nor leaves of this genus. Still it has some *Sigillariæ*. *Sigillaria lepidodendrifolia* Brgt., *S. Brardii* Brgt., *S. Defrancii* Brgt., *S. sculpta* Lsqx., *S. obliqua* Brgt.; *S. dilatata* Lsqx., *S. fissa* Lsqx., *S. Schimperi* Lsqx. These five last species are peculiar to it. All belong to the sections of the uncostate *Sigillariæ*. Among the costate species, none have been found in the shales but *S. lævigata* Brgt. The remains of *Asterophyllites* and its cones, of *Sphenophyllum* and especially of *Pinnularia capillacea* Ll. and H., are extremely numerous in its shales. This last species scatters its threadlike branches in every direction and mixed with *Neuropteris hirsuta* Lsqx.; and *N. flexuosa* Brgt. still common species of this coal, covers the roof of the Pomeroy vein with a painting of beautiful garlands. *Dictyopteris obliqua* Bunby., still appears with this coal, but is very scarce indeed, a species apparently dying out. Now the *Neuropterideæ* become more predominant and varied than in any other horizon. Thus, *Neuropteris speciosa* Lsqx., the most beautiful of the genus, *N. fissa* Lsqx., *N. plicata* Sternb., *N. rotundifolia* Brgt., *N. Grangeri* Brgt., *N. Cistii* Brgt., *N. Villersii* Brgt., *N. gibbosa* Lsqx., *N. undans* Lsqx., *N. tenuinervis* Lsqx., *N. dentata* Lsqx., *N. Desorii* Lsqx., *N. heterophylla* Brgt., all these species, except perhaps the last, appear for the first time, and are probably contemporaneous with this coal only. It has still *Neuropteris crenulata* Brgt., a species connected also with No. 1B. The *Sphenopterideæ* are represented here by *Sphenopteris tenella?* Brgt. (The only specimen found was indistinct.) *S. Gravenhorstii* Brgt., *S. Dubuissonis* Brgt., *S. abbreviata* Lsqx., *S. intermedia* Lsqx., *S. plicata* Lsqx., all species of the same type with small dissected leaflets, far different from the characteristic type (the large leaflets) of the species of No. 1B. To this coal belong exclusively all the species of doubtful affinity, referred by some authors to *Aphlebia* Sternb., and connected in my catalogue with *Hymenophyllites* Göpp., viz. *Hymenophyllites fimbriatus* Lsqx., *H. affinis* Lsqx., *H. hirsutus* Lsqx. and *H. giganteus* Lsqx.

The *Pecopterideæ* have the first rank for the number of representatives in coal No. 4th. It has *Alethopteris Pennsylvanica* Lsqx., *A. aquilina* Brgt., *A. urophylla?* Brgt., (specimens incomplete,)

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A. obscura Lsqx., *A. serrula* Lsqx., *A. rugosa* Lsqx., with *Pecopteris arborescens* Brgt. (abounding), *P. notata* Lsqx., *P. distans* Lsqx., *P. oreopteridius* Brgt., *P. pusilla* Lsqx., *P. dubia* Lsqx., (referable with doubt to *P. arborescens* Brgt.,) *P. cyathœa* Brgt., *P. arguta* Brgt., *P. concinna* Lsqx., *P. incompleta* Lsqx., mostly species peculiar to this coal. *Pecopteris polymorpha* Brgt. is also found in its shales.

The genus *Calamites* is here represented by *Calamites cruciatus* Brgt, *C. dubius* Brgt., *C. bistriatus* Lsqx., *C. disjunctus* Lsqx. These two last species have been found only with this coal; but each has been established on single specimens and thus they are still uncertain.

From this enumeration, it is evident that we have passed at this geological horizon to a vegetation much reduced in the size of its representatives and of a quite different character. The arborescent species belong to the ferns, or to *Sigillaria*, of small size.

Our 4th coal is covered, as was said before, by a thick stratum of sandstone, generally conglomerate in part, varying in thickness from 10 to 150 feet. The vegetable fossil remains of this sandstone afford a new evidence of the predominance of the ferns at this geological horizon, and of the disappearance of *Lepidodendron*. It is the immediate deposit of silicified trunks of *Psaronius*, extending from Ohio to Virginia along the Great Kanawha river. At Shade river, near Athens, Ohio, the broken pieces of these trees are so numerous that they cover in places the bed of the creek.* They appear generally as pieces of small stems horizontally broken, varying from two to eleven inches in thickness. The largest specimen which I was able to find is apparently the base of a tree. It is nearly round, eleven inches in diameter, and perforated, not in its central part, but somewhat on one side, by a regular round hole four inches in diameter. Many of the small specimens have their surface regularly costate like species of *Sigillaria;* but their internal structure is apparently that of *Psaronius*. It would be very interesting to compare with ours the position of the *Psaronius* deposit of Europe, which, I suppose, is contemporaneous.

The five hundred feet of measures intervening between the Mahoning sandstone and the great Pittsburg coal, have not afforded hitherto very abundant materials for the study of their botanical remains. It has been seen already that, in Pennsylvania, this space is occupied by strata of shales, sandstone and limestone which are generally barren of coal, at least of

* Unhappily, I have not as yet been able to obtain for microscopical examination polished lamellæ of sections of the collected species and cannot give an exact account of them. But the work of preparing the specimens is now in progress, and the view of internal structure of those interesting remains will soon afford the possibility of determining the species.

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any bed of coal of sufficient thickness to encourage the working. Thus the shales are still unopened and no chance is given of examining them. In Kentucky where in the same space the coal is more developed and has been opened at different places and different geological horizons, the shales roofing the coal-strata are mostly very bituminous and contain especially animal remains, fishes and shells, either with or without few fossil plants. Nevertheless I have been able to find here and there some determinable species affording points of comparison for the distribution of the coal-plants in those higher measures. Thus, the fire-clay of the bottoms of all the coal-strata is always filled with remains (leaves only) of *Stigmaria ficoides* Brgt., and at any place where the coal is formed, it shows on its horizontal sections the carbonized prints of *Calamites*, *Pecopteris* and *Stigmaria*.

Coal No. 6 is covered by a schistose, gray, laminated sandstone, blackened by broken fragments of ferns. Leaflets of *Pecopteris*, *Sphenophyllum*, *Asterophyllites* and crushed branches of *Calamites*, are distinguishable in great abundance among the fragments; but their specific determination is not possible.

Coal No. 8, has in its shales *Pecopteris polymorpha* Brgt. in abundance. Near Marietta, Ohio, its place is occupied by a red soft shale full of the remains of *Asplenites rubra* Lsqx., closely related to *Pecopteris arborescens*. *Neuropteris hirsuta* is there also in its normal form.

Coal No. 9. Its roof shales contain mixed together a great quantity of well-preserved fossil shells, of scales and teeth of fishes, and of remains of ferns evidently floated. Two species of *Sphenopteris*, one of which appears identical with *S. gravenhorstii* Brgt., *Pecopteris polymorpha* Brgt., another *Pecopteris* referable by the small fragments found, to *P. arborescens* Brgt.; *Neuropteris flexuosa* Brgt., leaflets only separated from the stem, a few undeterminable species of *Calamites* and one *Sigillaria*.

Coal No. 11. The great Pittsburg coal has also both animal and vegetable remains, but generally on isolated strata among the shales. At Pittsburg the lower bed is roofed by black bituminous shales which appear formed wholly by remains of stems of *Neuropteris hirsuta* Lsqx. and by leaves of *Cordaites borassifolia*, and in the shales of the upper part of this coal, which corresponds to our No. 12 and which in Kentucky is sometimes separated from No. 11 by a thin and very irregular bed of limestone, there are at Pittsburg well preserved specimens also of *Neuropteris hirsuta* Brgt., *N. flexuosa* Brgt., *Pecopteris polymorpha* Brgt., *P. arborescens* Brgt., *P. cyathæa* Brgt., *Sphenophyllum emarginatum* Brgt., three species of *Calamites* and one *Sigillaria*. The same species with a far greater proportion of *Calamites* and also one

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*Sigillaria** are found at Paradise, on Green river, Kentucky, in the shaly or 'brashy' coal separating No. 12th coal from an iron blackband which overlies it. I must not omit to mention with this coal the first appearance of a species somewhat related by its form to the species of the Oolitic series. It is the remarkable *Neuropteris Moorii* Lsqx., which by its pointed leaves and alate primary or secondary rachis, is distinct from any typical form of the coal-plants. It may be compared by its general appearance to *Alethopteris Whitbiensis* Brgt. of the Oolite of England but the affinity is distant indeed, as the American species has no marked medial nerve and some of the forked and arched nervules of its leaflets emerge from the common rachis.

The third great sandstone formation, or the Anvil-rock sandstone, which underlies the highest section of the coal-measures, is from all appearance as distinct a point of division in the distribution of the fossil flora as any one of the other great sandstones, the conglomerate and the Mahoning. From my own observations and from the data collected by other geologists, especially by my friend J. P. Lesley, no trace of *Lepidodendron* appears either in the Anvil-rock sandstone or in the measures above it. At least, none has been found as yet. This sandstone nevertheless is not barren of fossil vegetables. Near Greensburg, Penn., it contains in abundance, petrified trunks half silicified, half transformed into sandstone. Among the specimens which I had an opportunity to examine in the cabinet of Prof. Moore, I found a portion of the base of a small tree which had been evidently imbedded in the sandstone, in its standing position, still preserving the embranchment of three diverging roots. The surface of the tree and of the roots is marked by peculiar scars. Though not perfectly distinct, it was at once evident that this fossil tree did not belong to any of the genera of plants before seen in the coal. It was nevertheless impossible to describe it, in its state of obliteration. Later, Dr. D. Dale Owen, the celebrated geologist of New Harmony, discovered in the Anvil-rock sandstone of Posey Co., Indiana, not far from the Wabash river, three or four standing stumps which by examination proved to belong to the same species of trees as the one found at Greensburg. The specimens of Dr. Owen have been carefully removed and are preserved in his cabinet, with the roots attached to them, just as they were found. The stem is round, about one foot thick, branching at its base in five or six large roots, diverging nearly horizontally. Its surface is narrowly reticulated, and is marked by double oval scars, united by a deep wrinkle, very regular in their position, and about one inch distant from

* It is a costate species which appears to be the same at No. 9th and No. 12th coal. All the specimens have lost the bark and I could never find the scars except those which were too much obliterated for satisfactory determination.

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each other. At the point where the roots branch and diverge from the trunk, the double scars become more and more separate, more irregularly placed, and thus the roots take somewhat of the appearance of *Stigmaria*. The difference is well marked, nevertheless; the scars being triangular, marked in the center by a deep point only, and the roots quickly diminishing in size and terminating in a point at a short distance from the base of the tree. The imbedding shales not having been preserved, there is no trace of rootlets. Thus we have here, at this high position in the coal measures, a new typical form which probably indicates a difference of vegetation in the subsequent and last stage of the coal formations.

This enumeration is already too long, and though still incomplete, it must be abruptly closed, for fear of becoming tedious to the reader. In order to be understood by those who are not acquainted with botanical palæontology, it is only necessary to sum up and briefly discuss some of the conclusions which are derived from this examination.

Considering its generic distribution, the American coal-flora is nearly related to the European. We have only two or three peculiar genera, representing distinct types, which have not been seen in Europe. On the contrary, Europe has no peculiar and true generic types of coal-plants which are not represented in the coal-fields of the United States.

Considering its species, a more marked difference in the coal-flora of both continents becomes evident. Some of our species represent marked and peculiar forms or types, which are not seen in Europe, though a much greater number of species has been found in its coal-measures. Thus the predominance of typical or distinctly characterized forms, belongs to our country. By comparison of the flora of our epoch on both continents, we find now the same proportional relation and difference as at the time of the coal formation, that is, on this side of the Atlantic a predominance of well marked types; a predominance of species of trees;* a number of species perfectly identical on both continents, and many American species so nearly related to European congeners that their specific characters can hardly be established.

Though further researches ought necessarily to increase the number of species of fossil plants belonging to our coal measures, the proportional difference is likely to remain as it is established above.

The fossil-flora appears identical at the same geological horizon, over the whole extent of our coal-fields. This proves uniformity of stratification and geological unity of the different coal basins of America.

* The distribution of the genus *Lepidodendron*, at the time of the formation of the coal, has some analogy with that of the Oak in our time.

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The first trace of vegetable terrestrial life appears in the middle of the Devonian in a species of *Lepidodendron*, represented by its bark, its leaves, its cones, and large trunks of silicified wood. No remains of any other form of terrestrial vegetation have been seen in strata either inferior or cotemporaneous to this. All the vegetable remains known in the Silurian and lower Devonian belong to species of fucoides or marine plants, mostly of small size, resembling some species of *Fucus* of our time. The first leafy terrestrial plants appear in the Old Red sandstone. All the representatives of this new vegetation belong to a peculiar genus, *Noggerathia* Göpp., more related to Conifers or even to Palms than to Ferns. They are found in the same geological horizon, both in Europe and in America, and entirely disappear at or before the beginning of the coal epoch.

Ascending to the base of the coal-measures, we find there simultaneously in the subcarboniferous sandstone and below the upper Archimedes limestone different species of trees of large size. At the base of the Millstone-grit, where the coal begins to be formed, the number of species of large sized trees, especially of *Lepidodendron*, increases. At the same time, we see here the first species of ferns belonging to the true coal-measures, and already some species which reappear periodically with each bed of coal, in the whole thickness of the coal-measures.

At coal No. 1B, the second above the Millstone-grit, we have apparently the maximum of predominance of species of large size, especially of *Lepidodendron*. From this horizon upwards, *Megaphytum*, *Ulodendron*, *Halonia*, *Caulopteris*, *Lomatophloios* and *Knorria* are not seen any more. The species of *Lepidodendron* diminish in number to coal No. 4, where they are entirely lost; at least they have not been found as yet in any strata of the coal-measures above it. The genus *Sigillaria* follows also from coal 1B the same gradual diminution in the number of its species; but it is preserved in one or two representatives as high as coal No. 12.

As fast as these species of trees decrease in number, the ferns mostly of small size invade the coal-fields. They become predominant and show the greatest number of species at the base of the Mahoning sandstone.

This substitution of species is not the result of any perceptible change in their character or in their relative affinity. The relation of *Lepidodendron* is to *Lycopodites*. Both genera appear or at least disappear at the same time, and are replaced by typical forms, which have no analogy whatever with them.

The distribution of the ferns in the coal-measures is equally contrary to the supposition of a change of species by successive variations. They appear, it is true, grouped together, in a kind of relation between contemporaneous species; but we do not see, either before or after any of them, a trace of an intermediate

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form between the lost types and the following ones. The large-leafed *Sphenopterideæ*, the *Odontopterideæ* belong to coal No. 1B with most of the fruits of the coal measures; the *Neuropterideæ*, the *Pecopterideæ* and a peculiar section of small leafed *Sphenopteridæ* belong to coals No. 3 and 4.

As if to show how useless it would be to argue on the distribution of the coal-flora as resulting from successive variations of species and of genera, we find predominant genera represented in the whole thickness and in the whole extent of the coal-fields by species so variable that they can be called polymorphous, and which nevertheless preserve everywhere their identity. Thus appear *Neuropteris hirsuta* Lsqx., *N. flexuosa* Brgt., *Pecopteris polymorpha* Brgt. In the palæontological report of the Pennsylvania geological survey, I have figured eighteen forms of the first of these species, passing by insensible transitions from a small round leaflet to a large, nearly square *Cyclopteris;* then to cordate or reniform leaf of medium size; then to opposite, oval-lanceolate leaflets united by a narrow margin; then to a digitate leaf of which the five divisions are lanceolate-obtuse, and thus *ad infinitum*. Nevertheless, this species is perfectly well characterized and may be identified at first sight in any of its multiple forms.*

There is not in the number of *Neuropterideæ* and *Pecopterideæ* a well characterized species which could be admitted as a modified form of the predominant and variable species. Moreover, the numerous species of *Neuropteris* and *Pecopteris* appear at coal No. 3 and 4 in the middle of the coal-measures, and do not ascend higher, while those species which should be considered as originators or parents and consequently ought to be destroyed (from the law of selection) by their offspring, continue to predominate to the top of the coal-measures. Thus the vegetation of the coal shows at different geological horizons, both a gradual and sudden disappearance of species and even of types; both a gradual and sudden appearance of other species or of peculiar types, without regard to the former or extinct ones, and a continual reappearance in the whole thickness of the coal-measures of well-established species which neither by their form or their nature appear particularly prepared to sustain without varying the successive changes which have acted on the surface of the coal-measures at the time of their formation.

It is certainly possible to suppose that some disturbance, immersion, or upheaval of the coal-marshes, has modified their

* As the specimens are mostly found in simple leaflets detached from the stem, and consequently the species cannot be identified in its varieties by the examination of a stem bearing leaves of different forms, this facility of identifying any single leaflet may answer the reproach made to palæontological Botany, that it is a science directing its researches to variable parts of beings of which the true nature and real form is generally unknown, and that consequently its conclusions are more or less problematic. This reproach was long since conclusively answered in a letter of Prof. Heer to Sir Charles Lyell.

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vegetation. But this supposition would not explain how it happened that the most obvious changes in the vegetation of the coal have not followed the formation of those great strata of sandstone, which, like the Millstone-grit or the Mahoning sandstone, must have employed a longer time than any other stratum for the gathering up of their materials.

The supposition may be further extended, and it may be asserted that the disappearance of certain types, or of vegetables of a large size, from the marshes of the coal-fields is only apparent. That in their forced migrations by the gradual submersion of the marshes, some species have been either arrested somewhere, and thus left inhabiting other countries or destroyed for a time by a too slow migrating process, while others, having the faculty of migrating developed in a larger degree, have reappeared again and again on the marshes, living there for a longer period.

The field of supposition is unbounded, but it is a field where science is not permitted to wander. Where have the new types or species come from? How is it, if the disappearance of vegetables like *Lepidodendron* is only apparent, or local, that we do not find any of their remains succeeding the carboniferous epoch.* And admitting that species of *Lepidodendron* have been arrested in their migrations on some dry land, should we not find above our 4th coal some remains of this genus in the strata of sandstone where so many trunks of *Psaronius* have been imbedded.

I wish that I had time to discuss here at length and with the attention which it merits, a subject of importance connected with the examition of the stratigraphical distribution of the plants of the coal-epoch. Are the coal-measures a single, unique formation? Do they belong to a single epoch, or are they composed of a succession of formations separated by immense space of time, and of which the different stages might be compared to those of the recent formations: the Eocene, the Miocene, and the Pliocene, for example? In the last case, can we admit the vegetation of which the remains have been preserved in the shales of the coal, or the vegetation of the coal-marshes, as a true representative of the flora of the various epochs where the coal was formed; or was it then, as the bog vegetation is at our time, composed of a peculiar group of plants, adapted to the formation of the coal, pertaining to the marshes only, while another

* A single specimen of *Stigmaria* is said to have been found in Germany in the Todtliegende or Permian. But the locality has not been ascertained and consequently the statement cannot be relied upon. Moreover for a long time and in many places, as I have seen it myself, the Permian in Germany was taken for the Old Red sandstone and vice-versa, from the difficulty of ascertaining its position and from the want of fossil remains. I cannot take into account as contradicting my assertion, one *Lepidodendron* mentioned by Mr. Murchison as found in the Permian of Russia.

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flora of a different character was covering the dry land, if there was any dry land, at the carboniferous epoch?

From the thickness of some beds of coal, representing a mass of combustible matter as great at least as that which is contained in our oldest and deepest peat bogs; from the thickness and various composition of the strata which separate the beds of coal, and from the successive changes in the vegetation of the coal, it appears that the last alternative is admissible. Different hypotheses have been put forward to explain the so-called huge or gigantic vegetation of the coal-formations. But there is nothing in the carboniferous epoch authorizing the supposition that the power of vegetable life was greater than it is at our time. The combustible matter heaped in some of our peat bogs is sometimes sufficiently thick to be equivalent to the coal of a bed of four to five feet. The trees growing in our marshes or on the peat bogs are generally larger than those which have been preserved in the strata of the carboniferous measures. The Dismal Swamp is impenetrable on account of the great compactness of its vegetation. It is not an easy matter either, to get across the heaped, half prostrated or erect and closely pressed trees of our cedar-swamps of the North. If such marshes were extended over the greatest part of the United States, they would present a fair representation of those of the carboniferous period.

The occasional appearance of petrified trees, standing imbedded in sandstone, does not give evidence of a rapid formation either of the coal or of the other strata. Local disturbances may throw a few feet of sand upon a marsh, covered with active vegetation, and thus preserve the stumps from decomposition and by-and-by these may be converted to stone. The bald cypress and other species of trees grow sometimes in the marshes near the sea shore under ten feet of water. Whole forests of those trees have been imbedded in a standing position in the marshes around New Orleans. Thus I do not find in the geological records of the carboniferous period any indication of a rapid process of formation, either cataclysmic or abnormal, and I readily admit that each bed of coal with its accompanying strata of fire-clay and shales has required for its formation a period of time as long as any of our recent geological divisions.*

The question concerning the existence or non-existence of dry land covered with a peculiar vegetation at the epoch of the coal formation, cannot be answered positively or negatively by sufficient evidence. The only fact that would indicate that the

* Thus, if a peculiar nomenclature for a classification of the different strata of the old red sandstone, of the subconglomerate coal, and of the Millstone-grit is admissible, the process of division should be extended to each bed of the coal-measures.

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marshes of the carboniferous epoch were surrounded by land-bearing plants of different kind than those living on the bogs is the presence in coal No. 1B and in the sandstone underlying it of a great number of fruits of different species which by their nature have no relation to any of the other remains preserved in the coal. They have been generally referred to species of *Cordaites*. But the two only species of our coal measures are found in abundance at geological horizons where the fruits are entirely absent. And even at coal No. 1B shales appearing entirely composed of heaped remains of leaves of *Cordaites borassifolia* do not contain any fruit. The species of fruit, *Carpolithes Cordai* Gein., referred by M. Geinitz to *Cordaites borassifolia, our most common and omnipresent species*, has not been found in the coal measures of America. Therefore, either the fruits of unknown relation, *Carpolithes*, *Trigonocarpa* and *Rhabdocarpos** belong to vegetable species which have grown on the marshes, and of which the remains, leaves and stems, have been entirely obliterated or those fruits belong to species growing out of the marshes, around them, and have been floated and thus disseminated in the shales and in the sandstones. This last opinion appears at first confirmed by a similar process of distribution of species in our deep swamps. I have already mentioned elsewhere† how the hollow trunks of the bald cypress which grows in Drummond lake (Dismal Swamp of Virginia) are filled by fruits, acorns, nuts, &c., of trees which grow on the dry land near its borders. But, it is not presumable that species of fruits only could have been floated and disseminated by the agency of water without any of the branches and of the leaves of the plants to which they belong. And nowhere have the shales, covering what is called the tail of a coal bank, viz: the part abutting against a hill of sand or losing itself in sandstone, exposed any remains of plants of another type than those belonging to the true coal formation. Even where the shales of the coal are covered with remains of shells and of fishes, and consequently formed when the marshes were immersed, all the floated remains of plants which are found with those of animals belong to the common species of the coal. I believe then that the plants preserved in the shales of the coal give us a fair representation of the general flora of the carboniferous epoch, as true and as general at least as the fossil plants of the miocene represent the general flora of the tertiary period. And I suppose that if there was any dry land around the marshes, the vegetation contained only a few species different from those living on the marshes. But this last opinion is merely hypothetical.

[*To be continued.*]

* I consider the *Cardiocarpa* as the fruits of Asterophyllites and probably of some species of Calamites.

† Introduction to the fossil flora of Pennsylvania, Geol. Rep. of Penn., p. 847.

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ART. XXXIV.—*Additional observations on the Circulation in the Eye;* by OGDEN N. ROOD, of Troy University, N. Y.

IN a notice which appeared in the September number of this Journal, I described a subjective phenomenon, which is seen when a bright sky is viewed through two or three plates of cobalt glass. Most persons who are in the habit of directing their attention to this class of phenomena, have even perceived *faint indications* of the circulatory movement with the naked eye. But in the experiment with the cobalt glass, where this motion is seen with distinctness, as well as in the faint indications of it obtained with the unassisted eye, the moving bodies are not observed to be in compact masses, but as it were, sprinkled over the field of view.

If however the light of a spirit-lamp with a salted wick, be concentrated on the eye by means of a convex lens, 3 inches in diameter, having a focal length of about 3 inches, the bright field soon resolves itself into a mass of small, round, *densely packed* moving bodies, which appear light on a dark ground.

This is seen with varying degrees of readiness by different persons: some perceiving it in a few seconds, others requiring a protracted gaze of several minutes.

The moving bodies at first appear very closely packed together like fine mosaic-work, but as the view grows more distinct, their path can be traced, and the conviction is forced on the mind of the observer, that they are moving at slow uniform rates, through narrow channels; the whole reminding one strongly of the circulation seen in the web of a frog's foot, by a microscope slightly out of focus.

The interposition of plates of yellow glass rather adds to the distinctness of this appearance.

The indistinct vision attendant on faintness or on mere eye-weariness, is, I suspect, in many cases, mediately caused by the appearance of this circulation: for example, some weeks ago, I tried the experiment of binding up the right eye, and using only the left, for two days. After reading with left eye for half an hour, the page grew indistinct, the letters apparently dissolved and this dense circulation, which I had previously observed, set in, and continued to be visible for some minutes. It reappeared a number of times during that, and the following day.

Subjoined is an extract of a letter from Prof. Wm. B. Rogers.

"Your experiment with the blue glass appears to render much more *distinctly visible* a phenomenon which I have often observed and which has been noticed by others. After a continued effort of vision in experiments on binocular combination or in the use

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of the microscope as well as after active exercise, or any other stimulus to the circulation, I am able to reproduce it easily and with great clearness.

"When in these conditions I look intently with the naked eye upon a bright surface as that of a white cloud or a sheet of letter paper in the sunlight, the first appearance presented is that of numerous bright points darting around in various broken curves, coming in view and disappearing fitfully, but in such positions as to indicate the recurrence of the same motions, or the passage of successive particles in certain prescribed and permanent channels.

"As the eyes continue to be fixed on the surface, a shade comes over it, and on the dark ground I see innumerable streams of particles, moving in infinitely various loops and other curves, which, by a little attention, are observed to maintain a constant pattern, strikingly analogous to the capillary circulation as seen in the microscope. These streams of particles are of a tawny yellow tint well contrasted with the dark brownish surface in which they appear.

"Under the most favorable conditions of the organ I can usually obtain the effect with both eyes open, but it is more certain and far more distinct with a single eye, the other being closed. I should add that the appearance rarely lasts more than one or two seconds at a time, being obliterated by the recurrence of the white illumination and after a short interval returning, though usually with less distinctness than when first seen.

"In looking through a tube of black paper at a white surface until the eye has become fatigued, and we should perhaps add *greatly excited*, I have often noticed the same phenomenon and I believe I could at any time bring into view the yellowish mass of the circulation by continuing the experiment for a minute or two. With the lens of my pocket microscope held at about the focal distance I scarcely ever fail to obtain this effect in a few seconds.

"Your experiment with the blue glass is very interesting and succeeds with me perfectly. It will be a great gain if with comparatively little discomfort and without risk we shall be able to scrutinize steadily this intraocular phenomenon."

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ART. XXXV.—*Some experiments and inferences in regard to Binocular Vision*; by Prof. WILLIAM B. ROGERS.

IN the theory of binocular vision which has been so ably expounded by Sir David Brewster, Brücke, and others, it is contended that no part of an object is seen single and distinctly, but that to which the optic axes are for the moment directed, and that "the unity of the perception is obtained by the rapid survey which the eyes take of every part of the object." According to this view our perception of an object in its solidity and relief, instead of being the simple and direct result of the pictures formed at any one moment in the two eyes, is acquired by a cumulative process, in which the optic axes are successively converged upon every point of the object within view.

In an experimental discussion of some points in binocular vision published in the Am. Journ. of Science several years ago,* I was led to conclude that the phenomena of the stereoscopic resultant do not necessarily or even usually conform to these conditions, and that "the perception of a perspective resultant line or of a physical line in the same attitude does not require the successive convergence of the axes to *every point*." The grounds of this conclusion were,—first that the perspectiveness of the resultant although not perceived when the axes are steadily maintained at any one convergence, appears as soon as they are allowed sufficient freedom of motion *to unite a few contiguous points* of the component lines, and that it then effects the whole extent of the resultant; and—second that the resultant presents a perspective attitude even when the component lines, instead of being united into one, are brought together so as to intersect at a small angle, *each of the intersecting lines in this case appearing in relief*.

Satisfied from these considerations that the perceived singleness and relief of the resultant are to be ascribed rather to a *process of suggestion* then to the exhaustive binocular survey which has been supposed necessary, I was led to the opinion that, while some change of direction and convergence of the axes is needed for the effect, the successive view of a few contiguous points is all that is required to bring the suggestive action into play and to give rise to the full perception of the position and relief of the resultant.

In this view while rejecting the theory of successive vision in the form in which it has been propounded, I still considered some degree of motion of the axes as one of the steps by which we obtain the perception of the binocular resultant.

The following experiments, intended still further to test the theory of the successive combination of corresponding points

* This Journal, [2], xx, 86, 204, 318, and xxi, 80, 173, 439.

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in binocular vision, are believed to be in part new, and are in part modified repetitions of experiments already described by Profs. Wheatstone and Dove. They offer what seems to be decisive proof that such a successive combination of pictures point by point, however it may enter in many cases into the complex process of vision, *cannot be regarded as an essential condition to the singleness and perspectiveness of the binocular perception.*

1. Let a brilliant line, held in a perspective position at a convenient distance midway between the eyes, be regarded intently for a few seconds so as to produce a lasting impression on the retinæ. On turning the eyes towards a blank wall or screen the subjective impression will be seen projected against it and having the same perspective altitude as the original line. If then one eye be closed the line will appear to subside into the surface of the screen taking an inclined position corresponding to the optical projection of the original line as seen by the unclosed eye and therefore corresponding to the image formed in that eye. By opening and closing the eyes alternately, and finally directing both to the screen we are able to see the two oblique lines corresponding to these projections and their binocular resultant corresponding to the original object.

2. Let two slightly inclined luminous lines formed by narrow slits in a strip of black card-board be combined into a perspective resultant either with or without a stereoscope. Looking at this for a few seconds so as to induce the reverse ocular spectrum, and then directing the eyes towards the opposite wall of the apartment, *a single spectrum will be observed having the attitude and relief of the original binocular resultant.*

As a strong illumination of the lines is necessary to bring out the most striking effect, the card-board should be held between the eyes and some brilliantly white surface, as the globe of a solar lamp or a strongly illuminated cloud, care being taken to prevent the entrance of extraneous light.

3. Using the same arrangement, let the luminous lines be regarded in succession each by the corresponding eye, the other eye being shaded so that no direct binocular combination can be formed. On looking toward the wall it will be seen that *the two subjective images unite to form a single spectral line having the same relief as if the lines had been directly combined* with or without the stereoscope.

While the perspective image continues distinctly visible, let either eye be closed the other being still directed towards the wall. The image will instantly lose its relief and take its position on the plane of the wall as an inclined line corresponding to the subjective image in the eye which has remained open. When the subjective impressions have been sufficiently strong, it is easy to alternate these effects by projecting first the picture

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proper to the right eye and then that of the left on the plane of the wall, with their respective contrary inclinations. On then looking with both eyes we see the resultant image instantly start forth in its perspective altitude.

It is hardly necessary to say that in order to obtain these effects satisfactorily even with lines very strongly illuminated, the observer should have some practice in experiments on subjective vision. *In these circumstances, however, I have found the results to be perfectly certain and uniform.*

The conditions of the experiments are obviously such as to exclude all opportunity of *a shifting of the image on the retina.* Such a shifting however is essential to that successive combination of pairs of points in the two images which on the theory of Brewster is required for the production of the single perspective resultant. Hence according to this theory the resultant spectrum in these experiments, instead of being a single line in a perspective position, ought to present the form of two lines inclined or crossing, situated in the plane of the wall without projection or relief.

In reference to the first two experiments it might perhaps be maintained that as the perspectiveness of the original line or resultant on which the eyes were converged formed part of the direct perception in the first stage of the experiment, it would be likely through association to be included also in the spectral or subjective perception. But this consideration, which at best does not impress me as of much weight, is entirely inapplicable to the conditions of the last experiment, where the eyes are in the first place impressed *in succession* with their respective images, and where yet when they are together directed to the wall, *the perspective single resultant at once springs into view.*

4. Without resorting to these troublesome efforts of subjective vision the following is a simple proof that pictures *successively* impressed on the respective eyes are sufficient for the stereoscopic effect. Let a screen be made to vibrate or revolve somewhat rapidly between the eyes and the twin pictures of a stereoscope, so as alternately to expose and cover each, *completely excluding the simultaneous vision of the two.* The stereoscopic relief will be as apparent in these conditions as when the moving screen is withdrawn.

Here there is no opportunity for the combination of pairs of corresponding points in the two diagrams by the simultaneous convergence of the optic axes through them, but at each moment the actual picture in the one eye, and the retained impression in the other, form the elements of the perspective resultant which we perceive.

5. The ingenious experiments described by Prof. Dove many years ago in which the stereoscopic effect was obtained by the

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momentary illumination of the electric flash, furnish a further and most powerful argument against the theory of successive binocular combination here referred to.

In repeating these I have found great advantage in using one of Ritchie's improved Ruhmkorff coils having a coated jar included in the outer circuit, the intensely brilliant spark of which can be made to throw its light upon the object viewed in any direction or at any interval that may be desired.

When a twin-diagram of any simple geometrical solid was placed in the stereoscope and viewed by this momentary light it was found to exhibit the perspective resultant in most cases with a single spark, and it never failed to present it in perfection with a succession of sparks even when they followed each other slowly.

A large circular disc of brass, marked with the usual concentric striæ, was placed in a position to catch the illumination and produce the peculiar intersecting lines of reflected light. At each spark the bright resultant line due to the binocular union of these intersecting lines was seen penetrating the disc and extending in a steep angle beyond and in front of it.

As, according to Wheatstone, the duration of an electric spark is less than one-millionth of a second, it can hardly be supposed that in either of these experiments the eyes have time to make the successive changes of direction required, by the theory, for the singleness and relief of the observed resultant. Not less at variance with this theory is the familiar fact that the illumination of a single flash of lightning is sufficient to give us a clear perception of the forms and positions of objects to which the eyes are for the moment directed. So the long straight spark of one of Geissler's narrow vacuum tubes, glowing for an instant in a dark room, impresses a precise perception of the altitude and place of the tube and its included line of light, and even their regular path of the long spark through the air produces a distinct perception single and faithful to its devious directions.

We may therefore conclude—*first*, that the perception of an object in its proper relief, or that of the perspective resultant through binocular combination in a stereoscope, or otherwise, may and most usually does arise, by *direct suggestion* from the two pictures impressed, without requiring the successive combination of corresponding points;—*and second*, that for the singleness of the resultant perception, it is not necessary that the images should fall on what are called corresponding parts of the two retinas.

The condition of single vision in such cases seems to be simply this, that the pictures in the two eyes shall be such and so placed as to be identical with the pictures which the real object would make at a given distance and in a given altitude before the eyes.

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XXXVI.—*Geographical Notices.* No. XIV.

PROF. GUYOT'S MEASUREMENTS OF THE ALLEGHANY SYSTEM.—It is well known to the scientific men of this country that Professor Arnold Guyot of Princeton, New Jersey, has devoted a portion of his summer vacations for ten years past to the study of the different portions of the great Alleghany system which faces the Atlantic coast from Canada to Georgia. Several years ago he measured the highest peaks of the Adirondack, Green and White mountains, in the northern part of the chain, and more recently he has been at work on the southern portion of the system which is found to possess the most elevated peaks of the whole Appalachian chain. His determination of some of the highest peaks of the Black mountains of North Carolina was published in this Journal, for September, 1857.

By a private letter from Professor Guyot we learn that during last summer (1860) he has devoted two full months to further measurements in the south, in company with Messrs. Sandoz and Grand Pierre. The weather has been propitious and he has accomplished much work, having measured between one hundred and fifty and two hundred points in addition to those which were previously determined. He has extended his investigations as far as Georgia, and has seen the extremity of the Blue Ridge and the Unaka. It may now be affirmed with safety that the southern portion of the Alleghanies is better known so far as pertains to its hypsometry, than any other portion of the system. There is reason to hope that at an early day Professor Guyot will lay before the readers of this Journal, in detail, the results of his important and prolonged investigations; meanwhile the reader will be interested in the following partial summary of his observations in North Carolina.

These measurements sufficiently indicate the grand traits of structure of that loftiest portion of the Appalachian system. It may be seen that the Roan and Grand Father mountains are the two great pillars on both sides of the Northgate to the high mountain region of North Carolina, which extend between the two chains of the Blue Ridge on the east and the Iron and Smoky and Unaka mountains on the west. That gate is almost closed by the Big Yellow mountain. The group of the Black Mountain rises nearly isolated on one side in the interval between the two chains touching by a corner the high Pinnacle of the Blue Ridge, and overtowering all the neighboring chains by a thousand feet. In the large and comparatively deep basin of the French Broad Valley, the Blue Ridge is considerably depressed, while the western chain preserves its increasing height. Beyond the French Broad rises the most massive cluster of highlands, and of moun-

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tain chains. Here the chain of the Great Smoky mountains which extends from the deep cut of the French Broad at Paint Rock, to that, not less remarkable, of the Little Tennessee, is the master chain of that region and of the whole Alleghany system. Though its highest summits are a few feet below the highest peaks of the Black Mountain; it presents on that extent of 65 miles a continuous series of high peaks, and an average elevation not to be found in any other district, and which give to it a greater importance in the geographical structure of that vast system of mountains. The gaps or depressions never fall below 5000 feet except towards the southwest and beyond Forney Ridge, and the number of peaks, the altitude of which exceeds 6000 feet, is indeed very large. On the opposite side, to the southeast, the Blue Ridge also rises again to a considerable height, in the stately mountains of the Great Hogback and Whiteside, which nearly reach 5000 feet, and keeps on in a series of peaks scarcely less elevated far beyond the boundary of Georgia.

Moreover the interior, between the Smoky mountains and the Blue Ridge, is filled with chains which offer peaks higher still than the latter. The compact and intricated cluster of high mountains, which form the almost unknown wilderness covering the southern portion of Haywood and Jackson counties, is remarkable by its massiveness and the number of lofty peaks which are crowded within a comparatively narrow space. The Cold mountain chain, which constitutes one of its main axes, shows a long series of broad tops, nearly all of which exceed 6000 feet. Near the south end, but west of it, not far from the head waters of the French Broad, the Pigeon and the Tuckaseegee waters, Mount Hardy raises its dark and broad head to the height of 6133. Still further, to the northwest, the group culminates in the Richland Balsam, 6425 feet, which divides the waters of the two main branches of Pigeon river and of the Caney fork of the Tuckaseegee. Amos Plott's Balsam, in the midst of the great Balsam chain, which runs in a parallel direction between the two main chains, measures 6278 feet. Considering therefore these great features of physical structure and the considerable elevation of the valleys which form the base of these high chains, we may say that this vast cluster of highlands between the French Broad and the Tuckaseegee rivers, is the culminating region of the great Appalachian system.

New Map of the Alleghany System, by Mr. E. Sandoz.—The measurements of Professor Guyot, just referred to, furnish important data for the correction as well as the completion of all existing maps of the regions which he has examined. These data, with the exception of those collected in the past summer, have been employed by Mr. Ernest Sandoz, a nephew of Prof. Guyot, and an accomplished draftsman, in the construc-

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tion of a new map of the entire Alleghany chain, which has been published in the July number of Petermann's *Mittheilungen*. Mr. Sandoz had accompanied Mr. Guyot on many of his mountain expeditions and took the results with him to Gotha, where the chart was drawn and engraved under the direction of Dr. Petermann. To the editor of that excellent repository of geographical science, we are indebted for an early and proof impression of this map. As it is by far the most satisfactory chart of the Eastern portion of the United States in existence,—a request has been sent to Dr. Petermann to permit its republication in connection with this Journal, and there is reason to hope that at no distant day it may be laid before our readers, with a paper illustrative and explanatory of it, from the pen of Professor Guyot. The new edition when issued will contain some emendations derived from the more recent surveys to which allusion has been made.

The scale of the map is 1 : 6,000,000. Two detailed subordinate maps are printed on the same sheet with it, having a scale of 1 : 600,000, one of which gives the White Mts. of New Hampshire, the other the Black Mts. of North Carolina, both according to Mr. Guyot's measurements.

NARRATIVE OF A VOYAGE TO SPITZBERGEN IN THE YEAR 1613.—In the Transactions of the American Antiquarian Society, vol. iv. just published, S. F. Haven, Esq., the Society's Librarian, has edited with an introduction and notes, a Narrative of a Voyage to Spitzbergen made in the year 1613 at the charge of the English "Muscovey Company." Although this voyage is one of the series embraced in the celebrated collection of Purchas, commonly known as "His Pilgrimes," yet this account, which has been lying among the Manuscripts of the Antiquarian Society, has never before been printed. There is reason for believing, says Mr. Haven, that the Journal now first printed was from the pen of Robert Fotherby, whose name both as an author and as a skillful navigator is connected with two succeeding voyages.

"The expedition of 1613," he continues, "was fitted out with unusual care, and intrusted to the charge of some of the ablest men in the service. Besides the chief Captain, Benjamin Joseph, William Baffin and the author of our narrative, it was accompanied by Thos. Edge, who had already twice sailed to Spitzbergen. Purchas was indebted to Edge for the map of the coast inserted in his work, and also for a summary of Northern Discoveries which appears in the same volume. Baffin was attached to the ship of the commander of the fleet; and from that circumstance, apart from his personal reputation and the value of his scientific observations, his journal would naturally be the one selected for publication. The author of our account was in another vessel

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often separated from the rest. He thus experienced a different series of incidents or observed the same from a different point of view."

At this period when Arctic explorations are attracting so much attention, the printing of this early record is peculiarly acceptable. The introduction and notes, with which the paper has been enriched by the pen of its learned editor, illustrate many interesting points pertaining to polar discoveries. The cuts which accompany this edition are fac-similes (except in their size which is half that of the originals) from some rude drawings which are attached to the Manuscript.

DR. ENGELMANN'S MEASUREMENT OF THE ELEVATION OF ST. LOUIS, ABOVE THE GULF OF MEXICO.—In the Transactions of the Academy of Science in St. Louis, vol. i, No. 4, 1860, there is an article by Dr. Geo. Engelmann on the elevation of St. Louis above the Gulf of Mexico, from which the following statements are derived.

"A knowledge of the exact altitude of St. Louis is important as an element in the physical geography of North America, not only for the reason that this city stands, so to say, in the centre of the great Mississippi Valley and not far from the confluence of the four great rivers, the Mississippi, the Missouri, the Illinois and the Ohio, but, also, because most of the hypsometrical measurements throughout the northern and western regions of this valley and into New Mexico and Utah, made by the different explorers during the last twenty years and more, by Nicollet, Fremont, Owen, Wislizenus, Emory, Stansbury, and several of the Pacific Railroad exploring expeditions, took the altitude of St. Louis as their starting point, and were based to a great extent on the barometrical observations of those explorers compared with mine.

"Mr. J. N. Nicollet was the first who ascertained the elevation of St. Louis as well as a great many points on the Mississippi and Missouri rivers, as he was the first to give us a physical geography of the Mississippi Valley, based on careful instrumental observation. He laid down an abstract of his labors in his Report on the Hydrographical Basin of the Upper Mississippi, in 1841, published by order of the U. S. Senate, after his death, in 1843. On pages 93–101 he gives a detailed account of the methods employed to obtain the desired results, and on pp. 122–125 is contained a most valuable table of geographical positions, distances, and altitudes."

In this table, the altitude of St. Louis is stated to be 382 feet from which two feet must be deducted to reduce it to low water-mark.

Dr. A. Wislizenus next calculated the elevation of St. Louis. His results are published in his Report on a Tour to Northern

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Mexico, printed by the U. S. Senate in 1848. They place the altitude of the present low water-mark at 389 feet 6 inches.

Dr. Engelmann's recent calculations and measurements give the height of the low water-mark at St. Louis at 374 feet 4 inches, a few feet lower than either of his predecessors had estimated it.

The following table shows their comparative results for three different points measured.

Height of St. Louis above the Gulf, in English feet.

	Nicollet.	Wislizenus.	Engelmann.
Engelmann's Barometric Station,	486·5	496·0	480·9
City Directrix,	410·5	420·0	404·9
Low water-mark,	380·	389·5	374·4

"Intimately connected with the altitude of St. Louis and other points along our river is the question of the fall of the river and the velocity of its current. Nicollet's tables, mentioned above, give us the only data at present available for an approximative estimation of the fall of the Mississippi in its different sections. The following little table, calculated from these data, explains itself:

	Distances in miles.		Fall in feet.		Fall of the River in feet pr mile
	From point to point.	Total from St. Peter's.	From point to point.	Total from St. Peter's.	
Mouth of S. Peter's River to					
Prairie du Chien,	260	260	102	102	0·39
Rock Island,	210	470	114	216	0·54
Mouth of Desmoines,	128	598	84	300	0·65
St. Louis,	204	802	62	362	0·30
Mouth of Ohio,	174	976	58	420	0·33
Mouth of White River,	462	1438	122	542	0·26
Natchez,	348	1786	116	658	0·33
New Orleans,	302	2088	76	734	0·25
Mouths of Mississippi,	104	2192	10	744	0·09
Or in the great natural sections of its course: Mouth of St. Peter's River to					
Prairie du Chien,	260	260	102	102	0·39
Mouth of Desmoines,	338	598	198	300	0·59
New Orleans,	1490	2088	434	734	0·29
Mouths of Mississippi,	104	2192	10	744	0·09
Total average fall of the Mississippi from mouth of St. Peter's River to the Gulf,					0·34

"The Mississippi River has therefore an average fall of about 4 inches per mile; between St. Peter's and the Rapids, a little more; from the lower end of the Rapids to New Orleans, a little less; in the region of the Rapids, about 7 inches; and from New Orleans to the mouth, about 1 inch per mile. A further analysis of the tables shows the fall on both rapids to be 21 inches to the mile."

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Dr. Engelmann then gives the following data, based, as he says, on his own "rather loose observations" respecting the velocity of the Mississippi. As he remarks on "the absence of all other information" on this point it seems proper for us to refer to Marr's Report of Observations at Memphis in 1849, and to Dr. Ellet's work, to the measurements of Riddell, Forshey, and Dickenson reported to the American Association, and to an article by Lyell in this Journal, [2], iii, pp. 36 and 118.

Date of Observation.	Height of River above low water.	Current per hour.	One mile in
1845. Feb. 20,	5 feet,	3·00 miles,	20 minutes.
" " 28,	10 "	3·50 "	17 "
1844. Mar. 5,	15 "	4·00 "	15 "
" Ap. 26,	20 "	5·00 "	12 "
1839. May 27,	21 "	5·09 "	11¾ "
1837. July 10,	27 "	5·55 "	10⅝ "
1844. May 19,	27 "	5·68 "	10½ "
" Jun. 22,	35 "	6·25 "	9⅝ "

KIEPERT'S NEUER HAND ATLAS, (Berlin, Reimer: N.Y., Westermann, 1860.)—This admirable work which has been for some years in progress is at last complete, presenting a collection of forty maps of different portions of the world. They are drawn with great beauty and skill and the whole work is at once attractive, convenient and trustworthy,—as might indeed be expected from the reputation which the author enjoys as a cartographer. Our limits do not admit of an extended criticism on the several maps, but we append a list for the information of those who may wish to purchase a complete general Atlas for every day use.

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EXPLORATION OF WESTERN BRITISH AMERICA.—In the last number of this Journal a condensed account was given of the expeditions sent out by the Canadian Government for the exploration of the Red river and Saskatchewan districts. Since then, we have received several important Canadian documents, referred to in that article. As they possess a permanent value we subjoin a notice of their contents.

1. Report from the Select Committee on the Hudson's Bay Company, together with the proceedings of the Committee, minutes of evidence, appendix and index. Ordered by the House of Commons to be printed 31st July and 11th August, 1857.

This public document presents in the usual style of the Investigations of a Committee of a House of Parliament, a vast amount of matter relating to the affairs of the Hudson's Bay Company and the regions under their control,—derived from the examination of Hon. J. Ross, Col. Lefroy, Dr. Rae, Sir Geo. Simpson, Sir John Richardson and other gentlemen. It is accompanied by three maps; 1, of the British and Russian possessions in North America; 2, of the aboriginal tribes of British America; and 3, of the Northwestern portion of Canada, Hudson Bay and Indian Territories.

2. Papers relating to the Affairs of British Columbia. Part I. Copies of Dispatches from the Secretary of State for the Colonies to the Governor of British Columbia, and from the Governor to the Secretary of State relative to the Government of the Colony; also Copies of the Act of Parliament to provide for the Government of British Columbia; Governor's Commissions and Instructions; Order in Council to provide for the Administration of Justice; and Instrument revoking so much of the Crown Grant of the 20th May, 1838 to the Hudson's Bay Company for exclusive trading with the Indians as relates to the Territories comprised within the Colony of British Columbia. Presented to both Houses of Parliament by Command of Her Majesty, 18th February, 1859. London, 1859.

This blue book relating chiefly to Fraser's river and the gold discoveries has an outline sketch of the western portion of British America showing the different routes of communication, across the country to the mouth of the river.

3. Northwest Territory.—Reports of Progress together with a preliminary and general Report on the Assiniboine and Saskatchewan exploring Expedition, made under instructions from the principal Secretary, Canada. By HENRY YOULE HIND, M.A. in charge of the Expedition. Toronto, 1859.

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4. Report on the exploration of the Country between Lake Superior and Red River Settlement and between the latter place and Assiniboine and Saskatchewan. By S. J. Dawson, Esq., C.E. Printed by Order of the Legislative Assembly. Toronto, 1859.

A particular account of these expeditions was given in the last number of this Journal, p. 218.

5. Geological Survey of Canada. Report of Progress for the year 1858. Montreal, 1859.

6. Map of the Northwest part of Canada, Indian Territories and Hudson's Bay; compiled and drawn by Thos. Divine, Provincial Land Surveyor and Draftsman, by order of Hon. Jos. Cauchon, Commissioner of Crown Lands, Toronto, March, 1857. Published by S. Derbeshire and G. Desbarats.

This is a map in outline, geologically colored, of the region above mentioned, extending as far north as 75° north latitude and as far south as 45° north latitude. Being made previous to the recent exploring expeditions it is now behind our present knowledge. The authorities on which it is based are J. Arrowsmith, A. Mackenzie, D. Thompson, the Admiralty and Coast Survey Charts, Pacific Rail Road Survey Reports. Sir Geo. Simpson, Governor of the Hudson's Bay Company, Sir Wm. Logan and others are also quoted. The isothermal lines are given according to Dove and Blodget.

7. Government Map of Canada from Red River to the Gulf of St. Lawrence, compiled by Thomas Devine, Head of Surveys, Upper Canada Branch, Crown Land Department. November, 1859.

This map in three sheets, is limited to Upper and Lower Canada, the counties of which are distinctly indicated by color, and the more detailed topography is also clearly given. It is clearly drawn, and presents in its margin a variety of useful information respecting the resources of Canada.

Dr. Barth in Asia Minor.—Dr. Henry Barth, the celebrated traveller in Africa, has published in a Supplement to Petermann's Mittheilungen an account of a journey which he made in 1857–1858 from Trapezund, through the interior of Asia Minor, by way of Tokat, Amássia, Yûsgad, Cesarea and Angora to Constantinople. His attention was directed to the physical structure of the country and also to the remains of ancient art. The archaeological inquiries which he made will hereafter be made public, in detail,—though many of the more important facts are brought out in Petermann with illustrative wood cuts. A sketch of his journey is given on an accompanying chart with plans of Tókat, Amássia, Kara-hissár and Kyr-schehr.

D. C. G.

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Art. XXXVII.—*Further Remarks on Numerical Relations between Equivalents;* by M. Carey Lea.

In papers on this subject published in the January and May numbers of this Journal for the present year, I endeavored to show that a large number of so-called elements could be arranged in seriated groups, the members in each series differing from each other by a common quantity, in most cases the number 44 or one approximating to it. I endeavored to show that not only were these groups natural groups, but that the chemical properties of the members of each group corresponded in many cases with their position in it. These observations seemed to favor the view at present gradually gaining ground, that those bodies which we have as yet failed to decompose, we have not proved to be elementary.

An interesting and elaborate paper by Gustav Tschermak, published in the Proceedings of the Academy of Science of Vienna, and extracted in an abridged form in Knop's Centralblatt, (July 4, 1860,) on the subject of the law of volumes of liquid chemical compounds affords a support to the views above expressed, from a new source. The author therein shows that many of the substances usually classed as elements comport themselves in the physical properties exhibited by their combinations as compound bodies, and that it is possible from these physical properties to determine (hypothetically) the number of "physical" or absolute atoms which he supposes to be contained in a chemical atom of such body or pseudo-element. He endeavors to show that it is possible to calculate the specific gravity of a liquid from its atomic weight and the number of simple (chemical) atoms in its compound molecule, as data, but that the results lead to the immediate inference that each chemical atom contains with few exceptions several physical atoms.

For particulars of his theory I must refer to the original paper, but some of his results are as follows:

	Physical atoms to each chemical atom.*
O (O=16)	2
S (S=32)	4
F	2
Cl	4
N	2
P	4
As	5
Sb	6

* These numbers are taken from the table of mean numbers, p. 508 of Centralblatt and are those subsequently used by the author for determining the "physical atomic weights" $= \frac{m}{n}$.

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If now we arrange the first six of these substances in parallel series we shall find

	Atomic weight.	Physical atoms.
Sulphur,	32	4
Oxygen,	16	2
difference,	16	difference, 2
Chlorine,	35·5	4
Fluorine,	19	2
difference,	16·5	difference, 2
Phosphorus,	31	4
Nitrogen,	14	2
difference,	17	difference, 2

Thus a common difference in each case amounting to 16–17 corresponds with a difference of two of the physical atoms into which Tschermak divides the chemical atoms.

If now we put O=2o, Cl=2cl, P=2p, the approximate difference between S and O, Cl and F, &c., (16–17)=2Δ', the difference (48) between S and Se=Δ'' and the difference (44–45) between the terms of the nitrogen series =Δ, we can express the whole of three important series in terms of these six quantities, so that at one and the same time both the numerical value of the atomic weights and the number of Tschermak's physical atoms shall be correctly expressed.

	Symbols.	At. weights.	Physical atoms.
A. Oxygen group.			
Oxygen,	o_2	16	2
Sulphur,	$o_2\Delta'_2$	32	4
Selenium,	$o_2\Delta'_2\Delta''$	80	5
Tellurium,	$o_2\Delta'_2\Delta''_2$	128	6
B. Chlorine group.			
Fluorine,	fl_2	19	2
Chlorine,	$fl_2\Delta'_2$	35·5	4
Bromine,	$fl_2\Delta'_2\Delta$	80	5
Iodine,	$fl_2\Delta'_2\Delta_2$	127	6
C. Nitrogen group.			
Nitrogen,	n_2	14	2
Phosphorus,	$n_2\Delta'_2$	31	4
Arsenic,	$n_2\Delta'_2\Delta$	75	5
Antimony,	$n_2\Delta'_2\Delta_2$	120·3	6

In which table the number of radicals by which the chemical atom of each body is expressed, corresponds with the number of Tschermak's physical atoms, while their numerical value is equal to the atomic weight of the body.

Thus tellurium $o_2\Delta'_2\Delta''_2$ would have two each of three radicals, in all six, agreeing with the number of physical atoms as-

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signed to it, while their value $2\times8+2\times8+2\times48=128$, at. wt. of tellurium.

These observations of Tschermak, taken in connection with the numerical relations which exist between atomic weights, give rise to very interesting results, and if the conclusions which he arrives at from his experiments should be confirmed, they cannot fail to exercise a very important influence on the progress of chemical science.

Philadelphia, August 28th, 1860.

ART. XXXVIII.—*On the Production of Ethylamine by Reactions of the Oxy-Ethers*; by M. CAREY LEA.

[Read before the Am. Assoc. for the Adv. of Sci. at Newport, August, 1860.]

WHILE engaged in making a series of experiments on this subject I met with the paper of Juncadella* and the observations of De Clermont† on the same subject. Finding that the subject had less novelty than I supposed I merely offer here one or two of the results which I have obtained.

Nitrate of ethyl C_4H_5O, NO_5 heated in sealed tubes with chlorid of mercurammonium $\left.\begin{matrix}H_3\\Hg\end{matrix}\right\}N\,Cl$ for many hours in the water bath did not appear to react upon it. Kept for some time in a boiling saturated solution of chlorid of calcium the tubes although extremely thick green glass-combustion-tube of small calibre was used, exploded with great violence, shattering the vessel in which they were contained, although they had been wrapped in strong cloth.

Nitrate of ethyl heated in a sealed tube with chlorid of zinc-ammonium $\left.\begin{matrix}H_3\\Zn\end{matrix}\right\}N\,Cl$ in the water bath, does not appear to act upon it.

Nitrate of ethyl heated in the water bath in a sealed tube with carbamate of ammonia $NH_4O, NH_2C_2O_3$ dissolves the salt. On cooling radiated crystals form. The contents of the tube evaporated to dryness with excess of chlorhydric acid, and then exhausted with ether to which a few drops of strong alcohol have been added, yielded a solution which gave a chamois colored precipitate with bichlorid of platinum, consisting of chloroplatinate of ethylamine.

·1185 gm. substance gave ·0465 metallic platinum corresponding to 39·25 per cent; theory requires 39·29

The product was but small. Probably portions remained undissolved by the ether. No doubt portions of di- and tri-ethylamine are also formed in the above reaction, in the same manner as in those of the halogen ethers with ammonia.

Philadelphia, July 11, 1860.

* Rép de Chimie pure, Tome 1, 273. † Ibid., 274.

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ART. XXXIX.—*On the Optical Properties of the Picrate of Manganese;* by M. CAREY LEA, Philadelphia.

[Read before the Am. Assoc. for the Adv. of Sci. at Newport, August, 1860.]

BREWSTER and HAIDINGER have described a remarkable property possessed by certain crystalline surfaces, of reflecting, besides the ray normally polarized in the plane of incidence and reflection, another ray, polarized perpendicularly to that plane, and differing from the former in being colored, a property rendered more conspicuous by the fact that the color of the ray so polarized abnormally is either complementary to, or at least quite distinct from the color of the crystal itself.

I find that this property is possessed to a remarkable degree by the picrate of manganese. This salt crystallizes in large and beautiful transparent right rhombic prisms, sometimes amber-yellow, sometimes aurora-red, exhibiting generally the combination of principal prism, and macrodiagonal, brachydiagonal and principal end planes. In describing this substance in a paper on picric acid and the picrates,* I mentioned that in a great number of specimens examined, no planes except those parallel with or perpendicular to the principal axis had been met with. Since then I have obtained in several crystallizations specimens exhibiting a brachydiagonal doma, but this appears to be rather unusual.

The optical properties of this salt are very interesting. It exhibits a beautiful dichroism. If the crystal be viewed by light transmitted in the direction of its principal axis, it appears of a pale straw color, in any other direction, rich aurora-red in some specimens, in others salmon color. A doubly refracting achromatised prism gives images of these two colors, except the light be transmitted along the principal axis of the crystal of picrate, in which case both are pale straw color.

But it also possesses in a high degree the property of reflecting two oppositely polarized beams, and the great size of the crystals in which it may readily be obtained, renders it peculiarly fitted for optical examination. If one of these crystals be viewed by reflected light while it is held with its principal axis lying in the plane of incidence and reflection, the reflected light is found to be not pure white, but to have a purple shade. Examined with a rhombohedron or an achromatised prism of Iceland spar, having its principal axis in the plane of incidence and reflection, the ordinary image is white as usual, while the extraordinary is of a fine purple color, the phemenon having the greatest distinctness when the light is incident at the angle of maximum polarization.

* This Journal, Nov., 1858.

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The experiment may be varied and the purple light beautifully seen without the use of a doubly refracting prism by allowing only light polarized perpendicularly to the plane of incidence to fall on the crystal; in this case the surface of the crystal appears rich deep purple, no white light reaching the eye.

This property is not possessed by all the planes of the crystal, but is limited to the principal prism and brachy- and macrodiagonal end planes, in other words to the planes parallel with the principal axis of the crystal. The brachydiagonal doma and $0P$ planes do not possess it. Nor is it exhibited by the first mentioned planes, when the crystal is turned with its prismatic axis at right angles to the plane of incidence.

All specimens of picrate of manganese do not possess this property to an equal extent. The crystals vary considerably in color, and those which are full red exhibit it more strongly than the amber colored. Picric acid boiled with aqueous solution of cyanhydroferric acid and saturated with carbonate of manganese gives crystals of a rich deep color, which exhibit the purple polarized beam particularly well.

These properties are not possessed by the manganese salt alone, but also by the picrates of potash and ammonia, (especially when crystallized by very slow spontaneous evaporation in prisms of sufficient size) and the picrates of cadmium and peroxyd of iron —with this difference however, that while the prismatic axis of the crystal in the case of the cadmium and manganese salts must be in the plane of incidence, in the alcaline salts it must be perpendicular to that plane. As they all crystallize in the right rhombic system, it is probable that either the alcaline salts on the one hand, or the manganese and cadmium or the other, are prismatically elongated in the direction of a secondary axis.

It is convenient that distinct phenomena should have distinct names, and none appears to have been assigned to this. Brewster speaks of it as a "property of light," and Haidinger uses the word "Schiller" for it. The terms dichroism, trichroism and pleiochroism are limited to properties of transmitted light. I therefore suggest for that here in question the name *catachroism*, using the preposition κατα in the same sense as in the word κατοπτριζω, to reflect, (as a polished surface,) applying it to express the property of reflecting two beams, one normally polarized in the plane of incidence and the other polarized in a plane perpendicular to it.

The chromatic properties exhibited by the picrates of ammonia and potash are very remarkable in their variety. Their crystals possess:—

1st. The well known play of red and green light. If a little very dilute solution of pure picrate of potash be spontaneously

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evaporated in a hemispherical porcelain basin, so as to form a net work of extremely slender needles, and these be viewed by gas light, the play of colors is singularly brilliant.

2d. Dichroism. When by spontaneous evaporation of large quantities of solution of potash, or better, of ammonia salt, transparent prisms of $\frac{1}{15}$ to $\frac{1}{10}$ inch diameter are obtained, these, viewed with a doubly refracting prism by transmitted light give two images, one pale straw color, and the other deep brownish red.

3d. The above described property of catachroism, or reflection in the plane of incidence of oppositely polarized beams.

Philadelphia, August, 1860.

ART. XL.—*On our inability from the Retinal impression alone to determine which Retina is impressed;* by Prof. WILLIAM B. ROGERS.

[Read before the Am. Assoc. for the Advancement of Sci., at Newport, Aug. 1860.]

ALTHOUGH on first view it might be supposed that an impression made in either eye must necessarily be accompanied by a mental reference to the particular organ impressed, it will be seen from the following simple experiments that the impression of itself is not essentially suggestive of the special retinal surface on which it is received.

Exp. 1. Let a short tube of black pasteboard $\frac{1}{2}$ inch in diameter be fixed in a hole in the centre of a large sheet of the same material. Hold the sheet a few inches before the face of a second person and between him and a bright window, moving it to and fro until the bright circular aperture of the tube is brought directly in front of one of the eyes, suppose the left eye; and let him fix his attention upon the sky or cloud to which the tube is directed. He will feel as if the impression or image of the hole *belongs equally to both eyes* and will be unable to determine which of them really receives the light.

On moving the aperture towards the right, or nearer the nose, but not so far as to be out of the view of the left eye, or to be visible by the right, the observer will imagine that it is now in front of the right eye and chiefly seen by it. Shifting it still further in the same direction, until it is brought within view of the right eye but not fairly in front of it, it will appear as if placed before the left eye, and by an additional motion bringing it fairly in front of the right eye it will seem to be equally before both eyes or to be in the medial line between them.

Like effects may be observed by using a half sheet of rather stiff foolscap with a large pin hole in the centre. Bending this over the face and moving it until the hole is in front of one of

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the eyes, the same uncertainty and contradiction will be produced as in the preceding experiment.

Exp. 2. Similar results may be obtained by rolling half a sheet of letter paper into a tube of about one inch in diameter, and holding it before and a little in advance of one eye while both are directed to a white wall some feet distant. Keeping the view *fixed* upon the wall there will be seen upon its surface a circular image of the remote aperture of the tube. This as we look intently at it will appear as if seen equally by both eyes, occupying a midway position between them. If now the eyes be converged to some point nearer than the end of the tube the circular image will appear against the side of the tube giving the impression that it is seen by the eye which is remote from the tube and is at the same time directed toward the outside. For the complete success of this experiment the wall should be only moderately bright, and but little light should fall on the exterior of the tube next the uncovered eye.

Exp. 3. Let two tubes of stiff paper each one inch in diameter and six inches long be held close to the two eyes in a converging direction so that the outer ends may touch each other. Then directing the view through them to a white wall at a short distance, the observer will see the two tubes as one, with a single circular opening clearly marked out on the wall. If now a small object as the end of the little finger be brought near and in front of one of the tubes, it will take its place within this circle and will seem to be equally an object of vision to both eyes, so that the observer will be wholly unable to decide before which eye it is actually placed.

Let the observer next direct his view to a very remote object, as the sky, seen through the window, still retaining the previous adjustment of the tubes. He will now see two circles, continuing separate as long as he keeps his eyes fixed on the distant surface; and if the finger be held up as before in front of one of the tubes it will appear within the circle which is in front of *the other eye;* thus causing the impression on the right eye to be apparently transposed to the left, and vice versa.

Exp. 4. Fasten a small disc of white paper on a slip of black pasteboard of the size suitable for a stereoscope, and place this in the instrument so that the white spot shall be centrally in front of one of the glasses.

To a person not aware of the position of the spot it will appear in the stereoscope as if equally in view to both eyes and he will be entirely unable to decide on which retina its picture is impressed. Indeed properly considered the spot does not appear directly in front of either eye but is seen at the intersection of the optic axes, in the medial or binocular direction between the two.

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Let the spot be now moved toward the right side but still within the range of the left eye and it will seem to be before the right eye rather than the left. Shift it into the right compartment but not far from the dividing line and it will appear as if seen chiefly by the left eye, and finally bring it to the middle of the right compartment and it will seem as at first to belong equally to both eyes.*

Referring to the results observed in the above experiments when the object is directly in front of either eye it may be concluded that the mere retinal impression, on either eye is unaccompanied by any consciousness of the special surface impressed, and that the formation of the visual perception appertains to that part of the optical apparatus near or within the brain, *which belongs in common to both eyes.*

These observations show moreover that the *perceived direction is just as truly normal to the central part of the retina which has received no light as to that of the retina on which the white spot has been painted.* Indeed as before indicated, it is normal to neither, but is felt to be in the middle line between the two. that is, in the binocular direction. It need scarcely be added that this conclusion is at variance with the law of visible direction maintained by Brewster, which requires that the apparent direction of an object shall in all cases be normal to the part of the retina impressed.

The reference of the object in certain cases above noticed, (parts of 1, 2 and 4) to *one eye chiefly*, and that the eye from which *it is actually hidden*, is accounted for by the direction in which the other eye receives the light. As this direction, in the case of the left eye for instance, would be decidedly toward the field of view of the right eye, it would at once suggest the place of the object as somewhere before that eye, and so when the object is actually before the right eye, but in a position towards the left, it would excite the idea of an object somewhere before the left eye. As the retinal picture alone gives no indication of the particular eye in which it is formed, but only excites a visual consciousness common to both, the object in these cases will seem to be visible by both eyes but chiefly by that before which the suggestion just mentioned would naturally place it.

A like explanation applies to the transposition observed in Exp. 3, when the view is directed to a distance through the converging tubes. Here the false visual reference of the finger depends on the fact that the circle in front of either eye is sugges-

* The effect here described is one of a series of phenomena which Dr. O. W. Holmes attributes to an *actual transfer* of impressions from one eye to the other, and which he proposes to explain by the hypothesis of reflex vision. Proc. Am. Acad. Arts and Sciences, Feb. 1860.

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tive, merely by its position, of a *special vision by that eye*, while from the conditions of the experiment these circles are in fact reversed in their places as compared with the tubes and eyes to which they appertain.

We have seen in the above experiments that when an object is presented to one eye without any accompanying circumstances leading us to refer the visual act specially to this or to the other eye we have a consciousness of seeing it equally with both eyes. *The same result occurs when separate objects are presented to the two eyes*, provided as before, extraneous sources of suggestion are excluded.

Exp. 5. Thus if we place on the black slide of the stereoscope two spots, differing either in shape or color, one before each eye, we perceive them both in the middle or binocular direction, each seemingly visible in an equal degree to both eyes, the one being seen through or upon the other according to the fitful attention or suggestion of the moment. A pleasing modification of this experiment is made by using two unequal white spots on the black slide and interposing a green or other colored glass between one of them and the lens. The spot which appears colored will give as strongly the impression of being seen by both eyes as the white one, in spite of our knowledge of the position of the colored glass.

Even in cases where the two objects are wholly unlike, and at very different distances from the eyes by which they are severally regarded, this feeling of a common or united visual act in regard to each of them is often easily recognized. Of this we have a ready illustration in the familiar experiment on ocular parallax in which a distant object, hidden from one eye by an interposed finger or pencil, is seen through or behind the pencil when both eyes are directed towards the distant object.

Exp. 6. To observe this effect satisfactorily it is well to make the experiment in an apartment in which a single small lamp is placed at some distance from the spot on which we stand. Looking intently at the lamp, we bring the pencil before the face in such position as to give us an image on each side of the lamp, and then move the pencil toward the right until its left hand image seems to coincide in direction and position with the lamp, which appears to shine through or to partially replace it. As we continue to look thus at the lamp, we have a clear impression that both lamp and pencil are equally visible to both eyes, and without some consideration of the previous adjustment and motions we are unable to determine which is actually visible to the right and which to the left eye.

The same experiment furnishes also an incidental illustration of the principle of *transposed* visual reference before alluded to.

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If, while the above adjustment is maintained, we contemplate the other image of the pencil situated some distance to the right of the lamp, and endeavor to decide, from the mere visual impression, to which eye it appertains, we almost unfailingly refer it to the right eye as that which most nearly fronts it, although obviously it belongs to the other, as will be found at once on closing either eye.

Where the eyes are externally very sensitive, any strong illumination of one as compared with the other will interfere with the effect above described by referring the impression specially to the eye thus unduly excited. In such cases the observation is best made in a moderately lighted room by interposing the pencil between the eye and a vertical stripe on the wall.

Exp. 7. Recurring to Exp. 2, in which with a tube in front of one eye we perceive a bright circle on the wall in the medial direction, we may obtain a pleasing illustration of the point now under consideration by bringing a dark card or book or even the hand between the uncovered eye and the wall. The spot instead of being intercepted will appear as a perforation in the opaque screen.

Here as in the case of the pencil and lamp, the bright circle and the screen are both optically referred to the intersection of the two lines of view. But the luminous circle almost or entirely obliterates the corresponding part of the screen. As the full view of the screen and its connections continually remind us that it is in front of the uncovered eye, we are led to refer the luminous circle seen as coincident with a part of it, to the same eye, and thus to believe that *we are looking through the screen with that eye.* It is however not difficult, by intently regarding the luminous circle, so to counteract the force of this extraneous suggestion as to feel even in this case as if the circle were equally in view to both eyes.

These considerations explain very simply the experiment of the pseudo-diascope described by Mr. Ward of Manchester, which like several of those above mentioned is but an instance of the old observation of Da Vinci, that when we see behind a small opaque object presented near the eyes "it becomes as it were transparent." In making this experiment with a tube of paper supported between the thumb and forefinger of the left hand and held before the right eye so that the back of the hand may be some inches in advance of the left eye, it will be noticed that the effect varies with the amount of convergence of the eyes and that the bright perforation in the hand *may or may not* be referred to the left eye according to the force of the accessory suggestions or the intentness with which we fix our gaze upon the distant spot to which the axes are converged.

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In concluding, it may be remarked that the experiments which have been described are for the most part too obvious and familiar to have merited such a special notice but for the peculiar and in some respects new interpretation which they have offered of many visual phenomena. Considered in this relation we are I think entitled to conclude from them:—

First that the retinal impression of an object presented directly to either eye is accompanied by the feeling of a united visual act, and of itself gives no indication of the particular eye impressed; and:—

Second, that the reference of the impression to one eye rather than the other is the result of collateral *suggestion*, which may either locate the image in the eye that actually receives it, or may transpose it seemingly to the other, according to the particular conditions of the observation.

Art. XLI.—*Correspondence of J. Nicklès, of Nancy, France.*

Physical Chemistry.—Polarized Light employed as a Reagent.—Biot, who entered his 86th year on the 21st of April last, and who continues to labor with the ardor of youth, has just published a résume of his complete works on circular polarization, applied to the study of chemical species. These researches, which he has greatly promoted, have often occupied us and daily gained more importance. Biot has given us the following account of the manner in which he was led to the discovery of the fundamental fact which has given point to all his labors in this field. He says:—

"Accident, that great promoter of physical novelties, gave me in 1815 the first hint in some experiments made for an entirely different object.

Wishing to study the changes of a polarized ray, when it passes very obliquely through thin plates of sulphate of lime, I had enclosed such a plate in a large metallic tube, about 8 centimetres long terminated by parallel faces of glass, and filled with colorless essence of turpentine. This was attached to a metallic rod projecting out transversely, which turned on its axis around the centre of a graduated circle, allowing me to change at pleasure the inclination of the interior incidence of the polarized ray. Having thus brought the plane of the plate in the direction of the ray itself, so as to fix the point of departure of their inclinations, the principal section of the analyzing prism being previously placed in the plane of the primitive polarization so as not to disturb it, I perceived in that part of the field of view occupied by the essence an indistinct, extraordinary image, of a blue color, caused by the interposed essence.

Water and alcohol previously introduced into the same tube, produced nothing similar. The effect observed, was not therefore due to the interposition of the essence, as a *liquid* mass. It must necessarily result from some physical property, peculiar to the substance which produced it.

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I hence inferred that there was a deviation from the plane of polarization, to the right or the left, as in plates of quartz cut perpendicular to the axis. Revolving the analyzing prism successively in these two directions, towards the left, the blue image gradually grew fainter, until at a few degrees distance, it disappeared entirely, and reappeared beyond colored red, which I had found to be a distinctive character of rotary phenomena produced by plates of left-handed quartz. Hence I immediately concluded that a phenomenon of the same kind, was here presented except that it was produced by the successive actions of similar particles of the essence."

The principal fact, its molecular character, and the general consequences which are deduced from it, were made known to the Academy of Sciences, the 23d of October, 1819, and were published in a few days after in the Bulletin of the Philomathic Society of the same year, p. 190.

Since then Biot has most perseveringly prosecuted these general studies and labored to have chemists adopt them as a means of studying the intimate constitution of bodies without decomposing them, inasmuch as chemical analysis is applied to the study of those bodies only after they have ceased to exist: he explains the difficulties he has surmounted in persuading chemists to adopt this method. It is credible that these difficulties would not have been overcome, if the author of circular polarization had been an obscure *savan*—just as the value of the beautiful discoveries of Pasteur, would not yet have been recognized had not Biot been their interpreter. Biot closed his recapitulation of the principal discoveries of which the circular rotatory power of molecules had been alike the occasion and the instrument by the reflection, if his investigations had left undiscovered other facts of a kindred nature, no one could regret the involuntary omission more than himself.

On the existence of new simple bodies.—The beautiful labors which Bunsen and Kirchhoff have recently published (see page 415) on this interesting topic, the great advantage which they derive from the wonderful and peculiar action of the different metals upon the solar spectrum, the use made of it for qualitative chemical analysis and which they propose to make of it, in investigating the nature of the light of the sun and the fixed stars,—all these and many other questions raised by their memoir, have recalled attention to a research published ten years ago by Leon Foucault on the rays of the electrical spectrum. This physicist first proposed the use of points of gas carbon to form the voltaic arc.*

In studying this arc in 1847–48, he discovered that the ray D of the electrical spectrum coincides with that of the solar spectrum, and hence we may produce a superposition of two spectra, by throwing on the voltaic arc a solar image formed by a convex lens. When metals, which produce the ray D only feebly, as iron or copper, are used as poles, remarkable intensity can be secured, by touching them with a salt of potash, soda, or lime.

Foucault further adds:—"Before inferring anything from the constant presence of the ray D we must ascertain if its appearance does not indicate the same material mingled with all our conductors. Still this phenom-

* L'Institut, Feb'y, 1849; see also this Journal, [2], xxix, 424.

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enon urges us to study the spectra of the stars—for if fortunately the same ray were there found, stellary astronomy would be advanced."

We recall in fact, that relying on their own observations, Messrs. B. and K. have admitted the presence of potassa and soda in the sunbeam, (*Journal de Pharmacie et de Chimie*, May, 1860, and this Journal, xxix, p. 424.) in the complete account they lately gave in Poggendorff's Annals, T. cxl, p. 161. These savans have shown how sensitive this characteristic is that even infinitesimal traces of potassa, soda and lithia can be recognized in the rays they produce and these characteristics continue even when the bases are reunited.

We will not dwell further upon this important research which is elsewhere presented in this Journal: but we should point out another direction of these researches, that which relates to the discovery of new simple bodies. Messrs. Bunsen and Kirchhoff made known lately the probable existence of a fourth alkaline metal, placed in relation to the rays of its spectrum immediately after lithium and preceding strontium.

It is thought that this process will determine the existence of the new metal dianium announced by M. Kobell (this volume, July, 1860, p. 123) and whose reality is contested by H. Rose.

Catalysis and Contact Actions.—The class of phenomena in chemico-physical research which for the want of a better term we distinguish as above, has been enriched by the discoveries of H. Loewel,* whose researches have remained hitherto too little known, but which demand attention from the very original facts they have brought to light; which are the fruits of several years of observation.

His researches on supersaturated saline solutions.—It is well known that a supersaturated solution of crystallized sulphate of soda exposed to the air crystallizes suddenly when touched by a glass rod, but that it does not crystallize when this rod is heated to 100° C. This fact is connected with many investigations which Loewel followed for a part of his life, and which are the subject of a critical analysis published by Hirn, a pupil of Loewel, who is well known by his interesting researches on heat.

Hirn's analysis of his teacher's results has rather the character of a dissertation filled with new facts, which although they may have been disputed, have yet remained so hidden in the diffuse memoirs of Loewel that they appear, as it were, now for the first time. As the research is too long to reproduce here, we content ourselves with citations from it.

* Poggendorff's Biographical Dictionary speaks of this Author thus: "Loewel, Henri, Director of the chemical works at Choisy-Le-Roi, from 1815 to 1819, since then colorist at Münster (Upper Rhine), born on the 29th of Sept. 1795 at Münster, died 5th of Sept. 1856, at Colmar: his published researches are:—

Upon some salts of chromium, *Annales de Chim. et de Phys.* (Ser. iii, T. xiv, 1845.)

Upon the chlorids of chromium (ib. xv, 1845.)

On the supersaturation of saline solutions (ib., xxix, 1850, xxxiii, 1851, xxxvii, 1853, xliii, 1855, xliv, 1855, xlix, 1857.)

Upon the action of zinc and iron upon the solution of the salts of sesquioxyd of chromium (ib., xl, 1854).

On the solubility of the carbonate of soda (ib. xliv, 1855).

On the chlorohydrate and sulphate of chromium (*Comptes Rendus*, xx, 1845.) See also *Le Recueil du Journal de Pharmacie et de Chimie*, which has also published these memoirs, a little earlier than the dates above quoted."

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Reverting to the experiment with a supersaturated solution of sulphate of soda, we see it is the *air* adherent to the glass rod which determines the act of crystallization, since if the rod is heated to 100° C. no such result follows. Is it then the air which suffices for this action or some peculiar quality contained in the air? The latter supposition seems the most probable since it is not caused by air which has been filtered through cotton contained in a tube, nor by air which has passed through a properly arranged series of flasks connected by tubes of glass. Air thus agitated or heated by friction, may be brought in contact with the supersaturated solution, under the form of a continued current, without determining the crystallization, which commences immediately in presence of normal air. Loewel attributes the modification produced by the air to the friction produced in his mode of experiment, and a recent experiment of Hirn proves that it is so.

The air thus rendered passive by Loewel is called *adynamic* air. Hirn has observed that the air is rendered completely adynamic when it escapes after compression in the form of a jet from the receiver in which it was confined. After this compression it can be directed with impunity into a solution of sulphate of soda saturated by heat in a closed vessel. On the contrary, the solution solidifies instantaneously if by the same tube and without any derangement of the apparatus, some bubbles of ordinary air are allowed to pass into the solution. Here then, is an action purely mechanical which replaces the action of heat, a remarkable example of the correlation of force which raises a crowd of questions and which leads to the inquiry if an identical composition of the air should always have an identical action upon a living being; if air rendered adynamic by a storm is not found in different conditions in relation to organized beings than air long undisturbed.

This brings to mind that Schroeder and Busch have shown that fermentation is not caused by air filtered through cotton, and we now ask if the air rendered adynamic by the process of Hirn will not possess still more passivity.

It is an argument more in favor of this theory now held by the advocates of spontaneous generation to know that it is not by germs of infusoria suspended in the air that fermentation or putrefaction is carried on. These experiments appear to us to touch questions of the greatest importance in the sciences of observation, as well as others relating to the most interesting considerations in cosmogony.

Empiricism.—Application of the Physical Sciences to Medicine.—A discussion which has recently taken place in the Academy of Medicine on the action of iron used as a medicine has made known to us this unexpected fact that there are physicians who deny any influence exercised by medicines in virtue of their chemical properties, and who think that the physical and chemical actions of the animal economy differ entirely from those which are observed in the vegetable kingdom.

At the head of this retrograde school (which ignores the progress made by physics and chemistry in the last eighty years and to whom organized beings are composed of a material which is not subject to the general laws of matter which composes the universe) appears a physician celebrated more than the rest, Dr. Trousseau, who raising the banner of

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vitalism has declared that chemical laws explain nothing when used in relation to man, and that medicinal agents act by unknown and very different means from those which chemists suppose. Space does not allow us to notice the reply made at the same sitting by another physician, Poggiale, who is also somewhat of a chemist. But we shall be asked what is the precise meaning of *vitalism?* *Vitalism* is a force in the category of what has been called catalytic force; this is a word which conceals our ignorance and which is evidently an obstacle to progress. This recalls that saying of Liebig, "If we allow forces to be created, investigations become useless and it will be impossible to arrive at the knowledge of truth." Vital force is then entirely for those physicians who ignore the first notions of physics or chemistry and think all has been said when they have installed this senseless word in place of an organic fact which ranks under the laws of mechanics, physics or chemistry. Vital force is insufficient to explain how it happens that a large number of substances, such as sugar, tartaric and malic acids, sulphur, sulphurets, salicine, &c., &c., undergo in the animal economy the same changes as when subjected to chemical action.

When we remember that slight compression of a muscle suffices to develop heat, and that its contraction evolves electricity, that in order to establish chemical action it suffices to place two heterogeneous bodies in contact—one is surprised that medical men should seek to explain the phenomena of life by "vital force"; as if the material of our bodies was exempted from the laws that regulate matter, as if what they call vital laws could interfere with the play of physical, mechanical, or chemical laws.

The discussion is not yet closed. It is still continued in the Medical Journals, some of which like M. Trousseau are vitalists in pathology and empirics in the domain of therapeutics.

*Electro-Magnets and Magnetic Adhesion.**—We have on several occasions given in this Journal the progress of our researches upon magnetic adhesion, as well as the laws which regulate electro-magnets. At the request of several physicists and mechanics we have published the whole of our researches upon this subject, and a small octavo volume under the above title is the result. For the two electro-magnets, the rectilinear and the horse-shoe which were known in 1850, we must now substitute a larger number (several hundred) differing from each other by determinate properties. All these new electro-magnets which are described in this book, or whose existence has been foreseen, have at first rendered our task very difficult, for we were obliged to give names to them in order to facilitate their study. To give them such names as '*horse-shoe*' only rendered their study more difficult, we therefore preferred to group them under a systematic nomenclature according to the principle of their natural classification.

We divide the electro-magnets into two great classes according to their form, these are:—

1. Branched Electro-magnets.
2. Diak-shaped Electro-magnets.

* *Les Electro-aimants et l'adhérence magnétique.*

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These classes are subdivided into families which rank according to the number of branches or disks of which they are composed; thus the rectilinear electro-magnet having only one branch will form the first family. The electro-magnet with two branches will serve as a type of the second family which will be that of bifurcated magnets, and the trifurcated electro-magnets or those with three branches form the third family; and finally the fourth family is composed of multifurcated electro-magnets, i. e., those with more than three branches. The families of electro-magnets stop here; there are consequently no *quinto, sexto . . . n* furcated electro-magnets, experience having shown that the properties of electro-magnets with more than three branches are very much the same, one new branch adding no new property.

The same method is followed with disk-shaped electro-magnets whose name is derived from *dromos*, course, in order to distinguish their most characteristic property, that of turning or revolving. These electro-magnets are divided into two groups, viz:

1st. Para-circular. 2d. Circular.

The first group is subdivided into *para-circular uni-dromes, bi-dromes, tri-dromes*, or *multi-dromes*, according as they are composed of one, two, three or more disks, in the same manner as for the branched electro-magnets.

Thus the three groups of electro-magnets are composed each of four families. These are subdivided into *genera*, determined by the *number* of helices; into *species*, characterized by the nature of the poles; and finally, into *varieties*, determined by the intensity of the latter.

The number of helices is expressed by the Greek words *monos, di, tri, . . . knemes*, from κνημις–ιδος, (a greave or leggin,) the nature of the poles by the words *isonomes* or *antinomes*, and their intensity by the words *isodynamic* and *heterodynamic*. The use of these expressions requires some explanation, one fact heretofore unperceived in electro-magnets, one to which at least no importance has been attached, is that we use this apparatus without inquiring if the poles have the same or different intensities; and yet we have shown on several occasions:* 1st, that there is a great difference between electro-magnets of two categories and a difference no less great between bifurcated electro-magnets with poles of the same name or poles of different name.† Take for example, the horse-shoe, which has only one bobbin or spool, and which for that reason is called *electro-aimant boiteux*; it has two poles of unlike names, but these two poles are of different intensity; if we apply to this magnet the above nomenclature we shall have a *bifurcate monokneme* electro-magnet, with *antinome* and *heterodynamic* poles which consequently teaches us the properties of this apparatus just as the expression "sulphate of potash" tell us much more of the composition of that ternary than did the *Sal polychrestum Glaseri* of the alchemists.

If the question is in regard to the common horse-shoe magnet we shall say *Bifurcated dikneme*, for the circular magnets with three disks before described;† then we shall call them *tridome dikneme;* that of the two disks‡ we shall call *bidome dikneme* with isodynamic poles or with heterodynamic poles according as the helix is placed symmetrically or otherwise.

* This Journal [2], xv, 107 and 383. † Vol. xvi, p. 110. ‡ Vol. xx, p. 101.

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This very simple classification suggests the kindred simplicity of symbolical notation in chemical compounds. Space fails us to pursue this question farther, as well as many others agitated in this work in which the theoretical points particularly discussed are the following:—

Of isodynamic and heterodynamic poles—Of anomalous poles (*points conséquents*)—Of magnetic phantoms—On the power of electro-magnets as affected by 1st, the elongation of the legs, 2d, the position of the helices, 3d, the distance between the poles—Of armatures—Of the form of polar surfaces—New process for measuring the effective power of electro-magnets—Of magnetic adhesion—Magnetization of locomotive wheels.

The latter process, which we have already described in this Journal (vol. xvi, [2], 337,) has since been successfully repeated in the United States, as we learn from a paper read by Mr. Blake at the Am. Sci. Assoc., 1859.

This work is illustrated by five large plates.

Nancy, Aug. 20, 1860.

SCIENTIFIC INTELLIGENCE.

I. CHEMISTRY AND PHYSICS.

1. *Chemical Analysis by Observations of Spectra.*—It is well known that many substances when introduced into a flame, possess the property of causing in the spectrum, certain bright lines. Bunsen and Kirchhoff have based upon these lines a method of qualitative analysis which materially extends the domain of chemical reactions, and leads to the solution of many difficult problems. In the present memoir, the authors develop the method for the metals of the alkalies and alkaline earths.

The lines in question become more distinct the higher the temperature and the less the specific illuminating power of the flame itself. Bunsen's gas lamp which gives a flame of very high temperature and very low illuminating power is therefore peculiarly adapted to these experiments.

The apparatus employed by the authors is sufficiently simple and does not require a large apartment for its successful use. It consists essentially of two telescopes and a hollow prism filled with bisulphid of carbon, and having a refracting angle of 60°. The eye-piece of one of the telescopes is removed and a metal plate substituted having a narrow slit which is placed in the focus of the objective lens. The lamp is placed before this slit, so that the border of the flame lies in the axis of the telescope. The substance to be examined is placed in a little loop on the end of a fine platinum wire, and is supported in the flame, a little below the point where this is intersected by the axis of the telescope. The rays diverging from the slit are made by the objective to fall upon the prism, and after refraction are received upon the objective of the other telescope the magnifying power of which is about four. The slit should be so wide that only the most distinct of the dark lines in the spectrum are visible. The lower part of the prism carries a small mirror: a telescope directed towards this mirror permits the observation of the image of a horizontal scale placed at a short distance. By turning the prism, the whole spectrum of the flame may be made to pass by the vertical wire of the observing telescope, and every part of the spectrum to correspond with this

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wire. A reading of the scale is to be made for each position of the spectrum; this reading however is not necessary for those who know the particular spectra by repeated observation.

Bunsen and Kirchhoff show in the first place, that the different states of combination of the metals examined, as well as very great differences of temperature in the flames produced, exert no influence on the position of the spectral lines corresponding to the particular metals. The same metallic compound gives a spectrum which is the more intense the higher the temperature of the flame; moreover, the most volatile compound of any particular metal always gives the greatest intensity of light.

When small pieces of potassium, sodium, lithium and calcium, are attached to the extremities of fine platinum wires enclosed in glass tubes and the spark of a Ruhmkorff's induction-apparatus is allowed to pass from one pole to the other—the spectra are found to contain the same bright lines as the flames. From this, it appears that these bright lines may be looked upon as certain indications of the presence of the metals in question. They serve as reactions by which these substances may be recognized more sharply, more quickly and in smaller quantities, than by any other analytical process.

The authors give in their memoir colored drawings of the spectra produced by the flames of the six alkaline and earthy metals, together with the pure solar spectrum for the sake of comparison: for these however, we must refer to the original memoir. The special results of the investigation, are as follows:

Sodium.—The spectral reactions of sodium are the most delicate of all. The yellow line Naα the only one found in the sodium spectrum corresponds to Fraunhöfer's line D, and is remarkable for its particularly sharp definition and its extraordinary brilliancy. If the temperature of the flame is very high, and the quantity of substance employed very large, traces of a continuous spectrum are observed in the neighborhood of the line. For this reason, faint lines in the spectra of other substances only become visible, in many cases, when the sodium reaction begins to fade away. The reaction is most distinct with the oxygen, chlorine, iodine, and bromine compounds, as well as with the sulphate and carbonate, but is also distinctly seen in the silicate and other non-volatile salts.

By volatilizing a known weight of soda in a room, the dimensions of which were known, and observing the effect produced upon a non-luminous flame, Bunsen and Kirchhoff have shown that the optical reaction is sufficiently delicate to detect less than the $\frac{1}{3,000,000}$ of a milligramme of soda. It will easily be conceived from this that the atmosphere almost always gives a more or less distinct sodium reaction. The authors suggest that daily and long continued spectral observations may possibly show a connection between the presence and distribution of endemic diseases, and the quantity of sodium in the atmosphere, since chlorid of sodium is an antiseptic substance.

From the inconceivable delicacy of the sodium reaction, it will readily be understood that substances which have been in contact with the atmosphere even for a short time, rarely fail to produce the characteristic spectral line. Striking a dusty book for example produces at a distance of several steps the strongest sodium reaction.

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Lithium.—The ignited vapor of lithium compounds exhibits two sharply defined lines, a very faint yellow line Li α, and a brilliant red line Li β. The former lies between Fraunhöfer's lines, C and D, but nearest to D—the latter lies between B and C. The reaction is somewhat less sensitive than that for sodium, perhaps because the eye is more sensitive to yellow than to red rays. The authors find that less than $\frac{1}{1,000,000}$ of a milligramme of carbonate of lithia can be detected with the greatest certainty.

Minerals containing lithia like triphyllin, triphane, petalite, etc., require only to be held in the flame in order to give the most intense line Li α. In this manner the presence of lithia in many feldspars may be immediately detected. Direct observation fails however to detect lithia when present in very minute quantities in natural silicates. In such cases the following process may be adopted: a small portion of the substance is to be digested and evaporated with fluo-hydric acid or fluorid of ammonium, the residue is to be evaporated with a little sulphuric acid and the dry mass extracted with absolute alcohol. The alcoholic solution is to be evaporated to dryness and again extracted with alcohol, and the solution evaporated on a flat watch glass. The residue is to be scraped up with a knife and introduced into the flame by means of the platinum wire: $\frac{1}{10}$ of a milligramme is usually quite sufficient for the experiment.

This process places beyond a doubt the unexpected fact that lithia is one of the most universally diffused substances in nature. The authors have detected it in sea-water, in the ashes of seaweeds, and of various species of wood growing on granitic soils, as well as in many minerals and in spring waters. A mixture of volatile soda and lithia salts gives the reactions of lithia with scarcely diminished distinctness. In consequence of the greater volatility of the lithia salts, the sodium reaction usually lasts rather longer than that of the lithia. In order therefore to recognize the presence of ~~very~~ small traces of lithia, when mixed with soda, the test should be placed in the flame while the observer is looking through the telescope: the lithium line is then often observed only for a few instants, among the vapors which first arise.

Potassium.—Potassium compounds give in the flame a very extended continuous spectrum, which exhibits only two characteristic lines—one at the extreme outer border of the red rays, Ka α, exactly corresponding to the dark line A of the solar spectrum, the other Ka β far in the violet, also corresponding to a dark line in the solar spectrum between G and H, but much nearer to the latter. A very faint line corresponding to Fraunhöfer's line B is only visible when the light is very intense, and is not very characteristic. The blue line Ka β is rather faint but as well adapted as the red line to the detection of potassium. The position of the two lines near the limits of the visible rays renders the reaction less delicate; only about $\frac{1}{1,000}$ of a milligramme of potassium can be rendered visible in this manner.

All the volatile compounds of potash exhibit this reaction: silicates and other fixed salts only yield it when the quantity of potash is very large. When the quantity of potash is small, it is only necessary to fuse the substance with carbonate of soda, in order to produce the characteristic lines. To detect extremely slight traces of potash, the silicate must

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be gently ignited upon a platinum crucible with a large excess of fluorid of ammonium and the residue introduced into the flame by the platinum wire. In this manner, it is found that nearly all silicates contain potash. The presence of salts of lithium and sodium do not sensibly affect the reaction; thus it is sufficient to hold the ashes of a cigar in the flame before the slit, in order to detect immediately the yellow line of sodium and the two red lines of potassium and lithium.

Strontium.—The spectra of the alkaline earths are much less simple than those of the alkalies: that of strontium is specially characterized by the absence of green lines. Eight lines are very remarkable in this spectrum—six red, one orange, and one blue. The orange line Srα which lies close to the sodium line, the two red lines Srβ and Srγ and the blue line Srδ are most important from their position and their intensity. The authors find that $\frac{6}{100,000}$ of a milligramme of chlorid of strontium may be detected by the spectral reaction. In order to detect strontium in its less volatile compounds, as in sulphates, silicates, etc., it is best to fuse the substance to be tested, with carbonate of soda, and to moisten the fused mass with muriatic acid. The authors give a very simple and elegant process for effecting the fusion in a spiral of platinum wire, for which however, we must refer to the original memoir. The reactions of potassium and sodium are not disturbed by the presence of strontium. The lithium reaction is also distinctly seen when the proportion of lithium to strontium is not too small.

Calcium.—The Calcium spectrum may be readily distinguished from the four spectra hitherto described by the presence of a highly characteristic and intense line in the green, Caβ. The very intense orange line, Caα, which lies considerably farther toward the red end of the spectrum, than the sodium line, Naα, or the strontium line, Srα, is not less characteristic.

Kirchhoff and Bunsen find that $\frac{6}{100,000}$ of a milligramme of chlorid of calcium may be easily and certainly recognized by the spectral analysis. The volatile compounds of calcium exhibit the reaction with the greatest distinctness: sulphate and carbonate of lime give the spectrum, as soon as the salt begins to become basic.

The compounds of calcium with fixed acids are indifferent in the flame. Those which are attacked by muriatic acid may be advantageously treated as follows: A small quantity of the finely pulverized substance is introduced on the platinum wire into the less hot portion of the flame, until the powder packs together without fusing. A drop of muriatic acid is then added, so that the greater part of it remains in the loop of the wire. On bringing this drop into the hottest part of the flame, it evaporates without boiling, and at the instant that the last portions are evaporated, a brilliant calcium spectrum is seen.

Those silicates which are not attacked by muriatic acid, are most advantageously treated by heating the very finely pulverized substance on a flat platinum crucible cover with about a gramme of half deliquesced fluorid of ammonium, and igniting after the volatilization of the latter. The residue is then to be moistened with one or two drops of sulphuric acid, and the excess of this expelled by heat—the substance thus prepared is then to be introduced into the flame. When potassium, sodium, lithium

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and strontium are all present together, the characteristic reactions of the alkalies appear first; those of calcium and strontium usually somewhat later. When these last are present only in very minute quantities, their spectral reactions do not appear; we obtain them however, immediately when the wire is moistened with muriatic acid, and held for a few moments in the reducing flame.

The authors point out the importance of these simple processes in a geological and minerological point of view. Thus sea-water is easily shown to contain potassium, lithium, calcium, and strontium, and many other very interesting and important examples are given.

Barium.—The spectrum of Barium is the most complicated of all those yet investigated. It contains two bright green lines between the spectral lines E and F but nearer to E; these the authors denote by Ba α and Ba β. A third line Ba γ is less sensitive, but still characteristic. The spectral reaction of the barium compounds is somewhat less sensitive than that of the metals already considered: about $\frac{1}{1,000}$ of a milligramme of barium is exhibited with perfect distinctness. The chlorid, bromid, iodid, fluorid, hydrated oxyd, carbonate and sulphate of barium give the reaction immediately on heating in the flame. Those silicates which are attacked by muriatic acid, must be treated in the manner pointed out for the silicates of lime. Other silicates should be fused with carbonate of soda, as in the case of strontium. In a mixture containing chlorids of all the six metals and at most $\frac{1}{10}$ of a milligramme of each, each metal was readily detected by the spectral analysis.

In conclusion, the authors point out the great advantages of the new method of qualitative analysis in detecting the presence of minute quantities of particular substances and in tracing their distribution upon the surface of the earth. They even venture to assert that there exists a hitherto undiscovered metal belonging to the same group with potassium, sodium and lithium, which gives a spectrum as simple and characteristic as that of lithium. This spectrum exhibited with their apparatus, only two lines—one, a faint blue, which almost corresponds to the strontium line, Sr δ and the other a blue line which lies a little farther toward the violet end of the spectrum and compares in intensity and distinctness with the lithium line.

The spectral analysis promises to furnish also a method of investigating the chemical nature of the atmospheres of the sun and of the brighter fixed stars.

Kirchhoff has shown from theoretical considerations, that the spectrum of an ignited gas is inverted when a source of light of sufficient intensity and giving a continuous spectrum, is placed behind it. In other words, the bright lines are under these circumstances converted into dark ones. From this it appears, that the solar spectrum with its dark lines is nothing else than the inversion of the spectrum, which the atmosphere of the sun would show by itself. The chemical analysis of the sun's atmosphere requires us therefore only to determine what substances introduced into a flame will produce bright lines, corresponding to the dark lines in the sun's light. The authors have verified by direct experiment the above conclusions, and have inverted the bright lines of potassium, sodium, lithium, calcium, strontium and barium. They promise a further extension of

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their very beautiful and valuable investigations.—*Pogg. Ann.*, cx, 161, June, 1860.

[Note.—The inferences of Kirchhoff and Bunsen with respect to the chemical nature of the sun's atmosphere require two assumptions neither of which is as yet supported by observation. The first is that the body of the sun is intensely luminous and that its spectrum contains no dark lines: the second is that no substances except those of which our earth is composed and with which we are acquainted exhibit in their spectra bright lines corresponding to the dark lines in the solar spectrum as we see it.—W. G.]

2. *On some numerical relations between the densities and equivalents of certain elements.*—Playfair has communicated to the Royal Society of Edinburgh some remarkable observations on the densities of several of the elements. The numerical relation which the author has detected amounts simply to this—that the densities are in certain cases accurately represented by the square roots, cube roots, or fourth roots of the equivalents. In obtaining the densities, the author takes a mean of all the best recorded observations in each case. The following table exhibits his results which are certainly very striking, the coincidences being too numerous and too perfect to be accidental:

	Equivalents.	Density as found.	Density as Calculated.
Diamond,	12	3·48	$\sqrt[3]{12} = 3·46$
Graphite,	12	2·29	$\sqrt[3]{12} = 2·28$
Charcoal,	12	1·38	$\sqrt[4]{12} = 1·36$
Silicon, (adamantoid,)	14·2	2·46	$\sqrt[3]{14·2} = 2·42$
Silicon, (graphatoid,)	28·4	2·33	$\sqrt[4]{28·4} = 2·30$
Boron, (adamantoid,)	7·2	2·68	$\sqrt[3]{7·2} = 2·68$
Bromine,	80·0	2·98	$\sqrt[4]{80} = 2·99$
Iodine,	127·0	4·99	$\sqrt[3]{127} = 5·02$
Sulphur,	16	2·00	$\sqrt[4]{16} = 2·00$
Selenium,	80	4·31	$\sqrt[3]{80} = 4·31$

It will be seen that there are some inconsistencies in the selection of the equivalents which however only affect the degree of the root to be extracted. It remains to be determined whether the other elements exhibit similar relations.—*Chemical News*, No. 27 and 29, 1860. W. G.

3. *On the Loss of Light, by Glass Shades;* by Wm. King of Liverpool, and Prof. Verver of Maestrecht; *with a note of additional experiments;* by Frank H. Storer.* Under date of Feb. 24, 1860, Mr. King writes to the Editor of the London Journal of Gas Lighting, etc., (see vol. ix, p. 111,) as follows: "Sir,—Having recently tried some experiments for the purpose of ascertaining the amount of light lost by the use of various descriptions of glass shades, I thought that the results obtained might prove not uninteresting to some of your readers, more especially as it is a subject of practical importance, and does not seem to have attracted the notice which it deserves.

The following table exhibits the amount of light lost by the use of the various shades therein enumerated:—

* Communicated for this Journal, by Mr. Storer.

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Description of shade.	Loss of light.
Clear glass,	10·57 per cent.
Ground glass, (entire surface ground	29·48 "
Smooth opal,	52·83 "
Ground opal,	55.85 "
Ground opal, ornamented with painted figures, the figures intervening between the burner and the photometer screen,	73·98 "

As the large amount of light lost by the use of a clear glass shade excited some surprise, a sheet of common window glass was placed between the burner and the photometer screen, when it was found that 9·34 per cent of the light was intercepted, thus confirming the result obtained by the employment of a shade of clear glass.

I may state that the shades were selected from a large number, and great pains were taken to obtain an average specimen of each kind."

It should be mentioned in passing that Verver* had previously called attention to the subject in these words :—"It is not necessary to surround the wicks [of platinum, placed in the flame of the 'water gas,'] with glass chimneys as is done with ordinary coal gas; on the contrary it is preferable not to employ them, because the chimneys, no matter how well polished or how clean they may be, always absorb a considerable portion of the light which is produced. This loss (*déperdition*) was shown by the following experiment. A burner with twelve jets without any chimney afforded an illuminating power of 6·75 candles; but on surrounding the wick with a clean and perfectly polished chimney, the illuminating power amounted only to 5·25 candles; it had consequently diminished 1·50 candles, *i. e.*, 22 per cent." But, as Schilling† has already remarked, this experiment alone does not prove that the whole of the lost 22 per cent was absorbed by the glass of the chimney, since the conditions under which the gas is consumed when a chimney is used must be entirely different from those which obtain when no chimney is employed.

Immediately after the arrival in this country of the journal containing Mr. King's note, I was requested by Mr. W. W. Greenough, Agent of the Boston Gas Light Co., to institute a series of comparative experiments upon this subject. Since the results of these experiments have in the main fully corroborated those of Mr. King it seems but just to this gentleman to give them publicity.

Instead of lamp shades, flat sheets of glass (ordinary window panes) six inches wide by eight inches high, were fitted to a rack of blackened wire which was fastened to the photometer bar (100 inches long) at a distance of three feet from the gas light.‡

The gas employed was prepared, from the caking coal of Pictou, N. S., expressly for these experiments and was contained in a special gas-holder.

* *L'Éclairage au Gaz à L'Eau à Narbonne et L'Éclairage au Gaz Le Prince, examinés et comparés par le* Dr. B. Verver, *Prof. de Chimie et de Physique à l'Athénée Royale de Maestricht.* Leide, 1858, p. 26.

† In his *Journal für Gasbeleuchtung, etc.* München, 1859, ii, 377.

‡ It does not appear that the distance from the source of light at which the glass screen is placed has any appreciable influence upon the amount of light transmitted by it. At all events no such influence could be detected in a number of experiments made purposely to test this question.

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The "illuminating power" of this gas when consumed from the Parliamentary Argand burner* at the rate of five cubic feet per hour—as in the following experiments—was equal to 16·00 candles consuming 120 grains, by calculation, (in reality about 135 grains as carefully determined by the balance in each case) of spermaceti per hour.

The experiments were made in the blackened experimental chamber of the Boston Gas Light Co., with the photometer (Bunsen's) which I am accustomed to employ in my daily tests of the gas furnished by the Company to consumers, every precaution being taken to ensure accuracy. It may also be mentioned that none of the measurements (of the distances of the photometer, from the standard candle) obtained by actual experiment were calculated,—*i. e.*, reduced to their equivalents in candles,—until the whole series of experiments was completed, and that no comparison of my own results among themselves or with those of Mr. King was made until each member of the table had been calculated as it stands below. Whatever the experiments may be worth therefore, they have at least the merit of being entirely independent and wholly unbiassed.

Description of glass.	Thickness of glass.	Loss of light.
Thick English plate,	$\frac{1}{8}$ of an inch,	6·15 per cent.
Crystal plate,	$\frac{1}{8}$ " - -	8·61 "
English crown,	$\frac{1}{8}$ " - -	13·08 "
"Double English," window glass,	$\frac{1}{8}$ " - -	9·39 "
"Double German,"† "	$\frac{1}{8}$ " - -	13·00 "
"Single German,"† "	$\frac{1}{16}$ " - -	4·27 "
Double German,† ground,‡	$\frac{1}{8}$ " - -	62·34 "
Single German,† ground,‡	$\frac{1}{16}$ " - -	65·75 "
Berkshire, (Mass.,) ground,‡	$\frac{1}{16}$ " - -	62·74 "
Berkshire enameled, *i. e.*, ground only upon portions of its surface,—small figure,	$\frac{1}{16}$ " - -	51·23 "
"Orange-colored" window glass,	$\frac{1}{16}$ As used for church windows, &c.	34·48 "
"Purple" " "	$\frac{1}{8}$ As used for church windows, &c.	85·11 "
"Ruby" " "	$\frac{1}{16}$ As used for church windows, &c.	89·62 "
"Green" " "	$\frac{1}{16}$ As used for church windows, &c.	81·97 "
A porcelain transparency, (Tyrolese Hunter,)	$\frac{1}{16}$ " - -	97·68 "

The term "loss of light" employed by Mr. King does not at first sight seem to be strictly appropriate, for a very considerable portion of the light not transmitted by a glass shade might be reflected against the walls of

* With the ordinary chimney.—Mr. King does not specify what burner was used in his experiments, but it was probably the "fish-tail," which has been so often recommended as a standard by his father, consuming four feet per hour, of course without a chimney.

† Among the Boston dealers, the term German is applied to glass of Belgian manufacture.

‡ The enormous resistance to the passage of light which is offered by ground glass is certainly worthy the attention of those using it for windows, &c.

The discrepancy between Mr. King's results and my own as regards ground glass may perhaps be owing to the fact, that the window glass used by myself was *more coarsely ground* than the lamp shades employed by him.

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the apartment in which the lamp is burning and thus aid in the general illumination of the room. The meaning which Mr. King attaches to the term is however so perfectly evident that I have not hesitated to follow him in using it. For that matter, there can be no doubt but that the numbers given by us express as accurately as the circumstances of the case will admit, the actual diminution of the amount of light, falling for example upon the pages of a book held near to its source, which would be occasioned by the interposition of the shades enumerated in our tables.

Boston, April 20, 1860.

[We cannot doubt that the great loss of light proved by the experiments above given, is to be, in part at least, accounted for by the conversion of a portion of the light into heat—an effect perfectly in harmony with the theory of transverse vibrations as applied to explain the phenomena of polarization of heat. On this theory, heat and light are different effects produced by one and the same cause, and they differ physically only in the rapidity and amplitude of their vibrations. The screen through which the vibrations of light are propagated serves to diminish first the rapidity of the vibrations requisite to produce the most refrangible rays, and in proportion as the transparency of the screen is diminished by any cause, inherent or superficial, this arrest becomes more and more complete. As the more rapid etherial vibrations have probably the least amplitude, we infer from analogy in sound-waves, that as waves of least intensity have the greatest amplitude, so with the luminiferous ether the extreme red has but little brilliancy. Hence the loss of light from polished screens is small compared with that observed in screens of opaline or roughened glass. It would be instructive to examine the spectrum obtained from a pencil of rays under each of the cases given, by means of a sulphid of carbon prism.

The subject of absorption of light by screens has long since been carefully examined by *Bouguer*.* By a photometric method essentially like Rumford's, Bouguer measured the loss of light in the beam of a candle compared with a flambeaux and also with the light of full moon, in passing through 16 thicknesses of common window glass having an united thickness of 21·43 millimetres = ·85 inch. The mean loss of light shown by these trials was as 247 : 1, or over 99 per cent of the whole quantity.

Six plates of the purest mirror plate-glass having an united thickness of 15·128 millimetres diminished the light in the ratio of 10 to 3, occasioned a loss of about 70 per cent of diffuse daylight. A mass of very pure glass about three inches thick diminished the light only about half the latter amount, owing to its being a single mass and not cut up into many planes.

He also measured the absorbing power of sea-water for light and found as the results of experiments made in France and of observations also in the torrid zone, that at the depth of 311 French feet the light of the sun would be equal only to that of the full moon, and at the depth of 679 feet would wholly disappear. He estimates the transparency of the air as 4575 times greater than that of sea-water, and

* *Traité d'Optique sur la Gradation de la Lumiere* (*ouvrage posthume*) : *publié* par M. l'Abbé De La Caille. *Paris*, 1760, 4to, pp. 368.

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from the properties of a logarithmic curve (which he calls *gradulucique*) whose functions he had determined experimentally, he seeks to fix the outer limits of the atmosphere. Bouguer was an expert geometer and sustains all his conclusions by mathematical demonstrations. His results seem to have received less attention than they merit, the only reference to his researches I have seen being by Daguin in his excellent *Traité de Physique*, iii, 300, 1859.

We should not omit in this connection to refer to the very interesting observations of Draper* on the spectrum formed by means of a platinum wire heated gradually from dull redness to perfect whiteness by a volta-electric current. He observed the red part of the spectrum to appear first and as the heat and brilliancy of the wire increased the other colors of the spectrum appeared after the violet. This result perfectly harmonizes with views above expressed.—B. S., JR.]

ASTRONOMY.—*New Planets.*—A new planet, the 59th of the group between Mars and Jupiter, was discovered Sept. 15, 1860, by Mr. James Ferguson, of the Washington (D. C.) Observatory. It was then about as bright as a star of the 11th magnitude.—*Gould's Astron. Journal*, No. 140.

The 58th Asteroid (since named *Concordia*) was discovered March 24, 1860, by Dr. Luther of the Observatory at Bilk.

PERSONAL.—Prof. J. D. WHITNEY, State Geologist of California, sailed from New York for his post of duty on the 22d of October last with Prof. WM. H. BREWER (late of Washington College, Pa.) in charge of the Department of Agricultural Chemistry and Botany, and Mr. WILLIAM ASHBURNER who is assistant in the field Geological and Astronomical work. The head-quarters of the survey will be for the present at San Francisco. But the Act authorizing the survey contemplates the establishment of a State museum, on the most extensive scale, the location of which is to be determined by a future legislature, which place, wherever it may be, will doubtless be also the permanent head quarters of the survey. For the present, letters addressed to any member of the corps, "State Geol. Survey, San Francisco," will reach their destination. Parcels or books for Prof. Whitney or any member of the corps may be sent to care of B. Westermann & Co., 440 Broadway, New York. No similar enterprise in the United States has ever been set on foot on a more liberal and enlightened basis, or opened under more favorable auspices as respects either the importance of the work to be done or the ability of those charged with the duty.

THE GENERAL INDEX to the 3d decade of volumes of the 2d Series of this Journal, now complete, occupies more than the space usually appropriated to our Scientific Intelligence, and our numerous friends, whose contributions are thereby excluded from the present issue, will pardon the unavoidable delay.

OBITUARY.—Died in Montreal, Oct. 9, Dr. W. P. HOLMES, well known as an early cultivator of mineralogy and botany in Canada, an active promoter of the Montreal Natural History Society, and for the last ten years professor of Medical Jurisprudence in McGill College, Montreal.

* This Journal [2], iv, 388, and v, 1.

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XXI.—P. 91, l. 9 from top, after 'from' *insert* 'the denominators of.'—P. 159, l. 7 from top, for '0″·6,' *read* '0″·6.'

XXII.—P. 1, l. 2 from bottom, for '*Dictyopyxis,' *read* 'Dictyopyxis; l. 4 from bottom, for 'Dicladia,' *read* '*Dicladia.'—P. 2, lines 1 and 2 from top, for '*Coscinodiscus,' *read* 'Coscinodiscus; l. 2 from top, for 'Rhizosolenia,' *read* '*Rhizosolenia;' l. 8 from top, for 'Diffiugia,' *read* '*Diffiugia;' l. 16 from top, for 'Eucyrtidium,' *read* '*Eucyrtidium,' in both cases.

XXIII.—P. 88, l. 2 from bottom, for 'northeast,' *read* 'east.'—P. 178, lines 3 and 5 from bottom, for 'Tannenschein,' *read* 'Sonnenschein.'

XXIV.—P. 123, lines 24 and 25 from bottom, *transpose* 'mica pseud.' and 'unalt'd scapolite.'—P. 313, l. 13 from top, for 'equalities,' *read* 'qualities.'—P. 314, l. 21 from bottom, for 'propositions,' *read* 'proportions.'—P. 428, l. 14 from bottom, for 'Loccopteris,' *read* 'Laccopteris.'—P. 443, l. 1 from bottom, for 'JOHN WARREN,' *read* 'JOHN WARNER.'

XXV.—P. 318, l. 23 from bottom, for 'of the south,' *read* 'of the north;' l. 8 from bottom, for 'and separates,' *read* 'separating;' l. 2 from bottom, for 'Nevada,' *read* 'Sierra.'—P. 375, l. 19 from top, after 'water,' *add* 'or metallic oxyd;' l. 18 from bottom, after 'the,' *insert* 'gaseous.'—P. 422, *dele* last paragraph of note.—P. 437, *dele* lines 5 and 6 from bottom.

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XXVI.—P. 67, lines 4 and 5 from top, for '$\frac{1}{3}\dot{R} : 2\frac{1}{3}\dot{H} :: 4\ddot{S}i : 1\dot{H}$,' *read* '$\frac{1}{3}\dot{R}^3 : 2\frac{1}{3}\dot{H} :: 4\ddot{S}i : 3\dot{H}$,' and *dele* the index 3 after ($\frac{1}{3}\dot{R}^3 + \frac{1}{3}\dot{H}$).—P. 108, l. 7 from bottom, for '**J. W. Bill**,' *read* '**J. H. Bill**.'—P. 158, l. 1 from top, for '95,' *read* '96.'—P. 169, lines 12 and 14 from bottom, for '*v*,' *read* '*V*.'—P. 170, l. 3 from bottom, after '**as much as**,' *insert* '**one-fourth of**.'—P. 236, l. 17 from top, for '80,' *read* '90.'—P. 298, l. 19 from bottom, for '**inside**,' *read* '**irised**.'—P. 305, in the title of Hayes's article, *dele* '**with a map**.'—P. 331, l. 5 from bottom, after '**the beak of the**,' *insert* '**smaller valve in the**.'—P. 332, l. 19 from top, after '**identical**,' *insert* '**with foreign forms**.'—P. 333, l. 10 from top, after '**Cretaceous**,' *insert* '**and perhaps Tertiary**.'

XXVII.—P. 243, l. 1 from bottom, after '**Delphi Slate**,' *insert* '**or Black Lingula shale, equivalent of the Genesee Slate or Marcellus shale of New York**.'

XXVIII.—P. 236, l. 1 from top, for '**lightly**,' *read* '**slightly**;' l. 7, after '**other**,' *insert* '**side is a**.'—P. 240, l. 1 from top, after '**Actinocrinus**,' *insert* '**grandis**;' l. 16 from top, for '**obscure**,' *read* '**obscurely**.'—P. 241, l. 2 from bottom, for '**oval**,' *read* '**oral**.'—P. 244, l. 14 from top, for '**armbones**,' *read* '**umbones**;' l. 6 from bottom, for '**cyclostomus**,' *read* '**Cyclostomus**.'—P. 248, l. 23 from top, for '**p. c.**,' *read* '**oz.**'—P. 252, l. 28 from top, for '$\frac{1}{2}$,' *read* '$\frac{1}{3}$.'—P. 291, l. 1 from bottom, for '**Englemann**,' *read* '**Engelmann**.'—P. 353, l. 14 from bottom, for '3·8924+121·80,' *read* '3·8924×121·80;' l. 10 from bottom, for '4·6440+121·80,' *read* '4·6440×121·80.'—P. 356, l. 20 from bottom, after '**cultivation**,' *insert* **a period**, and for '**where**,' *read* '**Where**;' l. 19 from bottom, *dele* '**then**.'—P. 357, l. 10 from top, for '**sand**,' *read* '**mud**.'—P. 408, (in some copies,) l. 8 from bottom, for '**Andreas**,' *read* '**Andras**.'

XXIX.—P. 272, l. 9 from bottom, for '**Hentz**,' *read* '**Heintz**.'—P. 295, l. 2 from bottom, for '*Carten*,' read '*Garten*.'—P. 452, l. 11 from bottom, and p. 453, l. 14 from top, for '**Leiber**,' *read* '**Lieber**.'—P. 454, l. 13 from top, (in some copies,) for '**Daguerin**,' *read* '**Daguin**.'

XXX.—P. 162, l. 16 from bottom, for '**many**,' *read* '**some**.'—P. 167, l. 23 from top, for '**best**,' *read* '**test**.'—P. 174, l. 18 from top, for '**point**,' *read* '**part**.'

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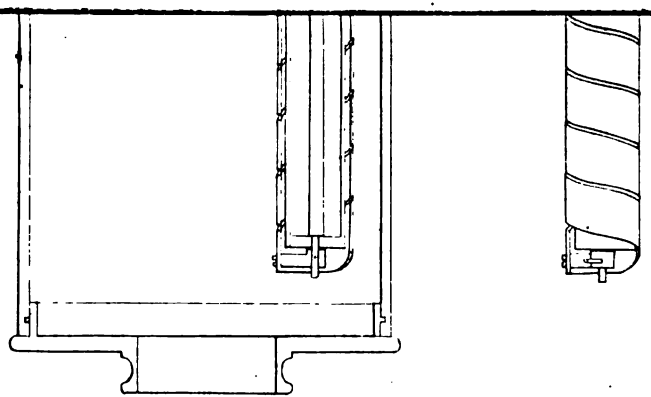
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Plate IV.



SAXTON'S METALLIC THERMOMETER.

For Deep Sea Temperatures

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